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A Statistical Approach  
to Textual Criticism,  
with special reference to the  

Peshitta of the Old Testament  

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Volume 2
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* i.e. A 1. Similar abbreviations are used throughout the Plan of Contents.
7. The Map of the Peshitta Psalter and its interpretation; with a discussion on the value of the Florentine Codex

The Peshitta Psalter presents quite a challenge to the textual critic. On the one hand, our material is remarkably rich; we have two mss which go back at least to the seventh century A.D., and other textual evidence dating from the fifth. Moreover, the critic will find that much helpful work has already been done. A full collation of over twenty mss and printed editions was accomplished by W.E. Barnes. There is also a concordantial dictionary of the Peshitta Psalter, by L. Techent; and the translation technique of the Peshitta in \( \psi \) has been the subject of several studies, the latest being that of A. Vogel. Yet despite the abundance of our ms evidence and of useful preliminary work, the history of the text remains largely unknown, and we have no clear-cut policy on dealing with variant readings.

1. Hereafter called P'. Other abbreviations: MT = massoretic text, G' = Septuagint, T' = Targum, V' = Vulgate (Gallican Psalter, in Psalms), H' = Vulci. uxta Hebraeos, \( \alpha' \gamma' \theta' \) = minor Greek versions, \( \psi = \) Psalms, Psalter.

2. I refer to the citations of Aphraates, extant in mss of the fifth cent.


We must view the efforts of these and other scholars with gratitude and admiration; but we may wonder why we have not reached more definite answers to those questions which especially concern the textual critic. First, a perusal of Barnes' apparatus will show that no stemma will explain all the ms groupings, and we may deduce that contamination has been extensive. This was the verdict of Barnes himself: "The history of the Peshitta is a history of never ceasing admixture of texts." Under such circumstances, it is not surprising that nobody to my knowledge has explained the variation in terms of a stemma; and so the way was closed to distributional methods.

The intrinsic approach, on the other hand, is beset with difficulties of its own. If we try to choose, out of two or more alternative readings, the one which best explains how the others arose, then we find all too often that we can argue equally well for more than one reading as original. Thus in 7:2, the mss diverge between

Both אֵלֶּדָּה ("Lord, they have made plans against me") and יָכִּי ("My Lord hath taken thought for me"? "Do thou, O Lord, take thought for me"?) are well attested among our mss and other authorities. Now it is not difficult to imagine a copyist changing either reading

1. The latter interpretation was found among the Nestorians; see Barhebraeus ad loc.
to the other; indeed, more than one change in either direction may have taken place independently. Evidently this intrinsic criterion will not be adequate.

Another intrinsic consideration is the relation of the alternative readings to MT and to G'. As it is generally agreed that P' was translated from a Hebrew Vorlage - with or without consultation of G' - it follows that agreement with MT will count in favour of a reading. On the other hand, G' was highly esteemed by many Syriac-speaking Christians, from the earliest times, and attempts were made to conform the text of P' to that of G'. Thus agreement with G' must count against a reading. The trouble is that on the majority of occasions where we might recommend one particular reading as that which stands closest to MT, we must also be wary of it because it also agrees with G'. Thus, in the choice between מָּשַׁל and מַשַּׁל, the latter is closer not only to MT (שֶׁ בָּשַׁל) but also to G' (בַּשַּׁל מֹע); and the two agreements all but cancel each other out. This much can be said, that in general, double coincidences of this sort are more likely to be due to preservation of an element from the original translation, than to the subsequent influence of G'; but we can hardly decide confidently between those two possibilities when we have in mind any particular passage.

1. Barnes excluded from this any reading found only in Codex F (see below), which he believed to have been revised after MT.

2. We may compare the problem which Prof. J.A. Emerton faced in his edition of "The Feshitta of the Wisdom of Solomon", Leyden 1959 (=Studia Post-Biblica 2). Here of course we have a Greek Vorlage, and he observes: "Agreement between the Feshitta and the Greek may be interpreted in two different ways. It may be suggested either than a Syriac reading which stands closer than another to the Greek is more original, or that it is a later correction. It is sometimes impossible to decide unambiguously."
There are, of course, some variants (about 30, see below) in which MT and G¹ do not coincide; but they are very much a minority.

The application of intrinsic criteria is rendered more complicated—and thus less likely to yield definite answers—by certain other considerations. Several passages are quoted or adapted in the New Testament, the text of which may have influenced those who were copying the Psalter. Again, the principle that resemblance to MT (when it is not accompanied by agreement with G¹) points to an original reading has been attacked, in that Barnes believed that one ms (the Florentine codex) was revised after MT. Finally there is the "Kahle view"¹ that P¹ as we know it was preceded by a number of independent translations which were worked into a single version, and that variant readings in our mss may go back to different early attempts at rendering the Psalter into Syriac. According to this, there are times when we ought not to choose between rival readings at all, but to record both of them as ancient renderings.

Since the intrinsic criteria are in most cases unlikely to yield results which inspire confidence, I believe it expedient to turn to distributional methods—to study further the inter-relations of the mss and to learn what we can of the history of the text.² Some progress has been

1. Kahle himself held such a view of the origin of G¹ and T¹, but did not explicitly suggest this about P¹. Nevertheless the possibility has to be investigated.

2. It is an ineluctable fact that even our earliest mss are several centuries later than the translation itself, whether we assign it to cent. 11 A.D. (Nöldeke) or to cent. 11 B.C. (Netz) or the question of dating, see Chap. 9 below.
made in this direction, and the position has been found to be far from chaotic. We know that P', originally the Bible of the entire Syriac-speaking Church, was retained by the mutually hostile sects into which that Church split up, and if— as we may hope—they have each imparted a peculiar character to their P' texts in some degree, we might identify on internal grounds certain ms groups which correspond to the different Christian sects. Thus we may distinguish Jacobite, Nestorian, Malkite and (perhaps) Maronite forms of the Peshitta.¹

The emergence of separate streams of textual transmission would naturally be of the greatest importance. A. Rahlfs² believed that the enmity between these sects ensured that any mutual influence between their biblical texts was negligible. Thus any feature attested in both Western (i.e. Jacobite, Malkite, Maronite) and Eastern (Nestorian) mss may safely be held to go back to the text common to all Syriac-speaking Christians before the schism of the fifth century. Against this view, there seems to be evidence of some contact between these two forms of text, so that the border between them is somewhat blurred.³ A consideration of the ms groupings in Barnes' apparatus will confirm that mss habitually desert the group to which they "ought" to

1. Barnes, p. xxxvi. On the Maronite text, see p. 7:8 n. 3 below.
belong; we find a sprinkling of "Western" readings in all our "Eastern" authorities, and vice versa. Thus the textual history seems to be far more complex than Rahlfs envisaged, and so the distributional approach has also led to something of an impasse. The situation is substantially the same as when Barnes wrote (p. xxxiv):

"This subject [i.e. the history of the text of the Peshitta version of the Psalter] is one of great difficulty, and the time has not yet come for even an outline of the History to be written."

Let us therefore try to see whether the mapping method will throw any light on the problem. In order to draw the map on p. 7:9, I have taken virtually all the variants noted in the edition of Barnes, on whose reports I have relied entirely. Only orthographic details of no obvious importance, and unique readings, were omitted.

The map was based on 21 authorities, listed in Table B.7.1.

Each of these witnesses contains at least the major part of the Psalter, and although I could find only 36 variants in which the readings of all 21 witnesses are known, there are 447 passages in which we have the readings of the majority. The map is therefore based on these 21 witnesses. In addition, I located the following authorities by the second "fragment" technique:

(a) Barnes' P (B.M. Add. 14674, fol. 79-126; 12t7 in Leyden notation, see the list, p. 21). P is a fragment containing nothing after ψ 40.
### Table B. 7. 1.

<table>
<thead>
<tr>
<th>Siglum</th>
<th>Full Title</th>
<th>Siglum</th>
<th>Reference</th>
</tr>
</thead>
<tbody>
<tr>
<td>(A) Ambros. B.21</td>
<td>7a1</td>
<td>28f.</td>
<td></td>
</tr>
<tr>
<td>(B) Cambridge U.L., Co. 1. 1,2</td>
<td>12a1</td>
<td>4</td>
<td></td>
</tr>
<tr>
<td>(C) B.M. Add. 17110, foll. 1-72</td>
<td>7t1</td>
<td>23</td>
<td></td>
</tr>
<tr>
<td>(D) B.M. Add. 14436, foll. 1-67a</td>
<td>9t2</td>
<td>16</td>
<td></td>
</tr>
<tr>
<td>(E) B.M. Add. 17109</td>
<td>9t3</td>
<td>23</td>
<td></td>
</tr>
<tr>
<td>(F) Florence: Laurent. Orient. 58</td>
<td>9a1</td>
<td>9</td>
<td></td>
</tr>
<tr>
<td>(G) B.M. Add. 14435</td>
<td>9t1</td>
<td>16</td>
<td></td>
</tr>
<tr>
<td>(H) B.M. Add. 17111</td>
<td>10t4</td>
<td>23</td>
<td></td>
</tr>
<tr>
<td>(J) B.M. Add. 14433</td>
<td>10t2</td>
<td>16</td>
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</tr>
<tr>
<td>(K) B.M. Add. 17219</td>
<td>13t1</td>
<td>21</td>
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</tr>
<tr>
<td>(L) B.M. Add. 14677</td>
<td>13t2</td>
<td>22</td>
<td></td>
</tr>
<tr>
<td>(O) B.M. Add. 14674, foll. 1-73</td>
<td>12t1</td>
<td>21</td>
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</tr>
<tr>
<td>(Q) B.M. Add. 17125, foll. 1-72b</td>
<td>10t5</td>
<td>23</td>
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</tr>
<tr>
<td>(R) B.M. Add. 14436, foll. 77b-129</td>
<td>10t3</td>
<td>17</td>
<td></td>
</tr>
<tr>
<td>(S) B.M. Add. 14676, foll. 1-42</td>
<td>cent. xii</td>
<td>58</td>
<td></td>
</tr>
<tr>
<td>(T) B.M. Add. 14672</td>
<td>cent. xii/</td>
<td>58</td>
<td></td>
</tr>
<tr>
<td>(m) Cambridge U.L., Co. 1. 22</td>
<td>17t1</td>
<td>xiv</td>
<td></td>
</tr>
</tbody>
</table>

1. Cf. "List of Old Testament Peshitta Manuscripts (Preliminary Issue)," edited by the Peshitta Institute, Leiden University (Leyden, 1961). The first number in the siglum gives the century; in case of doubt, a ms is dated later rather than earlier. References in the last column are to the pages of this list.

I have used the sigla of Barnes in my work, in preference to those of the Leyden scholars, not only because his sigla are easier to handle, but also because not all the above witnesses are to be used in the Critical Edition of P', and some of them have therefore been assigned no Leyden siglum.

2. This ms was written by two scribes; the second hand, which supplied 50:5-118:29, is called Tb by Barnes. I have counted the ms as a unity, however, for textual purposes.

3. Barnes uses the italic sigla _LrL _.
(b) Barnes' X (Add. 12138, foll. 1b-303b; 9m1; cf list, p. 14). A massoretic ms, presenting only a small proportion of the text.¹

(c) Barnes' Z (Add. 12178, foll. 1b-223a; 10m1; cf list, p. 15). Another massoretic ms.

(d) The commentary of Barhebraeus, after Lagarde's edition². Barhebraeus is cited in Barnes' apparatus, under the siglum bH; only a few readings seem not to have been noted (see e.g. 85:7; 90:1; 99:1; 115:1; 126:6).

On external grounds (e.g. form of alphabet and orthography), the mss KLNOMn and editions UaUc are regarded as Nestorian; PT as Malkite; the edition Le as based largely on a Maronite text³; and the rest as Jacobite⁴. The main cleavage is between the Eastern (i.e. Nestorian) family and the rest ("Western" texts), in that the Nestorians separated at an early stage from the body of the Syriac-speaking Church.

1. On the massoretic mss, see pp. 9-56 ff.
3. It is said to be little more than a reprint of the text of Gabriel Sionita, who based himself on a Maronite ms (Barnes, p. xxix).
4. C is Western in orthography, but shows signs of Nestorian influence. In particular its Ps titles agree with those of the Nestorian mss, and are quite different from those of the other Western mss (W. Bloemendaal, "The Headings of the Psalms in the East Syrian Church", Leyden 1960, p. 13).
The map shows a close-knit Nestorian group, whereas all the Western mss form a second, far looser, group, in which there seems to be no gulf separating the three sects represented—Jacobite, Malkite and Maronite. We may observe that the first dimension (i.e. along the "east/west" axis) corresponds roughly to the geographical opposition of east and west; whereas the second ("north/south") seems to reflect the age of the witnesses, the younger ones tending towards the top of the page.

FIG. B. 7. 2.
We are now brought to the problem of interpreting the map, and here we must ask to what we attribute the variation among our mss. Is it due in part to the former existence of more than one rendering of the Psalter from Hebrew into Syriac, or does the Peshitta Psalter represent but a single translation from the Hebrew?

I think it not unreasonable to lay the onus of proof on those who would believe that the Peshitta Psalter is the resultant of several attempts at providing a translation\(^1\), and not a single version. After surveying the evidence available to me, I see no reason to adopt such a hypothesis. The two main considerations on which I base this view are discussed in Chapters 8 and 10, to which the reader may now turn if he prefers to deal with this point immediately. These considerations are:

(1) If we consider any of the variant passages recorded by Barnes, we never find more than one reading which must be regarded as a rendering from a Hebrew source. The others can invariably be explained as inner-Syriac developments through: scribal corruption; assimilation to parallel passages, to G\(^\prime\) (sometimes by way of the Syro-hexaplar) and to the New Testament; the occasional substitution of a synonym, either for a divine name (e.g. \(\xi\omega\lambda\alpha \leftrightarrow \chi\iota\nu\alpha\nu\)) or otherwise (e.g.

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1. Thus M.H. Goshen-Gottstein, "Prolegomena to a critical edition of the Peshitta", Scripta Hierosolymitana (1961) pp. 26-27, mentioned the possibility that in Ez. 7:27 the variation in the mss between \(\xi\omega\lambda\alpha\nu\gamma\nu\epsilon\upsilon\) and \(\chi\iota\nu\alpha\nu\chi\iota\eta\nu\) went back to two different Hebrew Vorlagen, reading \(\gamma\gamma\iota\upsilon\alpha\nu\gamma\nu\epsilon\upsilon\) and \(\chi\iota\nu\alpha\nu\). This hypothesis presumably involves two translations from the Hebrew (p. 49). However, on p. 39 he inclines to the view that "the text of the Peshitta represents one translation only".
and adaptations due to the use of the Psalter for Divine Service (e.g. לְבָנָה for לְבָנָה in ψ 51:21).

(2) The indirect tradition of Psalter quotations in patristic literature shows, in the main, little short of complete agreement with the text transmitted in our mss; whatever variation is observed can be attributed, in almost every case, to the character of the work in which the quotation appears. Thus no trace of an initial plurality of Syriac translations of the Psalms has been substantiated in the indirect tradition.

Variant readings which have a bearing on (1) will be discussed in Ch.10, except that passages at which P stands alone are discussed later in this Chapter. The indirect tradition is considered in Ch.8.

Subsidiary considerations are:

(1) As S. Jellicoe¹ observes, not the least of the reasons for the "overwhelmingly contrary" consensus of opinion today regarding the Kahle hypothesis in relation to the Septuagint, is the difficulty of supposing an otherwise unknown 'Biblical Commission' which produced the revised official Greek version. The postulate of a similar commission which formulated the Peshitta Psalter is no easier to accept, nor does any external evidence point to it.

(2) If the existence of the commission, and of the 'unofficial' translations on which it drew, be granted, then it is difficult to account for the selection of readings which our present text represents. This text abounds, for example, in omissions of words present in MT and all other authorities, and in renderings which can be explained only by the supposition that the Hebrew Vorlage was (sometimes deliberately) misread. At most points in the text where such a mistake was perpetrated by the particular translator whose rendering now appears in the Peshitta, one may presume that his rivals agreed in renderings based on the correct Hebrew text; it is strange, then, that the commission should have bowed to this minority of one. Some of the renderings in our present text do not even make good sense, and would scarcely have been adopted if the commission had had even one alternative version to hand.

I therefore decided that it was possible to speak, at least provisionally, of a unique Urtext of P' (which one could term the 'Ur-Peshitta'). This served as a starting-point in my search for an interpretation of the map. Barnes too...


2. Vogel, p. 208, states that these number about 200.

3. e.g. at 12:9. MT: צפתי לברון ("like the vile height (=pride? hill-shrine?) of the children of Edom").
began with this aim (p. xlv), but in view of the complexity of the task decided instead to restore in some measure the early Western text, while reserving the Eastern readings for the apparatus criticus. However, the "early Western text" is a somewhat nebulous entity, as the map will confirm, and there are obvious disadvantages in a goal which is not precisely defined. As an example of the anomaly to which this may lead, we find that in ¹ 149:4, Barnes printed in the text the reading אֶלֶחֶד, on the authority of AF alone, placing the majority reading אָליֶשׁ (MT נַעַשׁ) in the apparatus. Yet in ¹ 14:4, where the majority reading is יֶלֶשׁ (MT נַעַשׁ לֶשֶׁד), he chose the majority reading for the text. This inconsistency, which I cannot explain in terms of the character of the readings themselves, seems due primarily to the inexactitude of the term "early Western text". Our aim here, to reach the "Ur-Peshitta", is at least uniquely defined, and it is an aim of which Barnes would have approved, had it not been for practical difficulties; but with the help of modern techniques for processing information, which were not available to Barnes, we may hope to go some way towards attaining it.

As before, our first step is to locate a "best point" at which the original is to be located. Our editorial policy will depend largely on this operation, and we must naturally make every effort to see that $\Omega$ - the original - has been placed in the appropriate region.

I found that three different approaches led to the
same result, viz that \( \Omega \) stands some distance away from the mss, towards the "south-east":

(A) We have already stated that \( P' \) was translated from a Hebrew Vorlage. Now it happens that in many passages where the mss diverge, one of the variants stands closest to MT. In the following examples, the symbol \( > \) means "resembles MT more than":

(a) \( \psi 2:1 \) \( \aleph > \aleph \) \( (MT \ \nu^\nu^\nu) \)

(b) \( \psi 2:3 \) \( \aleph > \aleph \) \( (MT \ \nu^\nu^\nu) \)

(c) \( \psi 2:3 \) \( \aleph > \aleph \) \( (MT \ \nu^\nu^\nu) \)

(d) \( \psi 2:12 \) \( \aleph > \aleph \) \( (MT \ \nu^\nu^\nu) \)

(e) \( \psi 2:12 \) \( \aleph > \aleph \) \( (MT \ \nu^\nu^\nu) \)

(f) \( \psi 3:4 \) \( \aleph > \aleph \) \( (MT \ \nu^\nu^\nu) \)

That a reading resembles MT does not necessarily mean that it is original. The resemblance may sometimes be due, as we have already remarked, to correction after \( G' \) (or after another source); this would, for example, account for passages abcf above (\( \epsilon\phi\nu^\kappa^\kappa \) \( \Delta\iota\nu\gamma\zeta\omicron\rho\iota\gamma\nu\omicron\sigma\nu\kappa\sigma\iota\omicron\nu \) and \( \epsilon\phi\nu^\kappa^\kappa \) \( \alpha\nu\gamma\zeta\omicron\rho\iota\gamma\nu\omicron\sigma\nu\kappa\sigma\iota\omicron\nu \)).

1. I assumed, at this stage of the work, that \( \aleph \) was Aphel (= "they felt"); later, however, its distribution led me to suppose that it was Peal with prosthetic Alaph (p. 10.3).
Again, the resemblance may be accidental; thus in f the Urtext may, for all we know, have had יא, and certain copyists added Waw. Furthermore, the Hebrew text on which we base our comparison may not have been identical with that of the Vorlage of P'. Thus we can hardly be sure in any particular case that the "MT-tending" variant is original. However, we can identify, in about 280 of the 450-odd variant passages, one reading which stands closest to MT; and it is a fair conclusion that these readings are original more often than not. Thus there ought to be a trend for the number of places in which a ms has such a reading, over this large array of independent passages, to reflect - and therefore to serve as a rough index of - the extent of its divergence from the Urtext.

The results of this comparison are given in Table B. 7. 2. The second column gives the number of passages in which each ms was observed not to have the "MT" reading; the third gives the number of passages in which an "MT" reading can be identified and the reading of that ms is known; the fourth gives the second as a percentage of the third. Thus the fourth column is a measure of divergence from О in which the fact that some mss (such as О, S) are available in less passages than are others (like Ua, Le) has been taken into account:
If we choose to judge the mss by the different degrees to which they resemble MT¹, we find that the honours have to be shared more or less equally between: (1) C - our oldest Psalter (6th cent. ?) - and S (12th c.), which closely resembles C in text² (see map); (ii) F (9th c.); (iii) the Nestorian mss K L N O m, of which O is the earliest (12th c.),

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1. We may take 40% as a convenient "pass mark" - but here a candidate must be below it in order to be successful!

2. I have found only 27 passages in which C and S diverge, out of 156 in which there is variation between our 21 "basic" witnesses and the readings of both C and S are known.
and the Nestorian editions UaUc. Other Western witnesses, most of which are earlier than any of the Nestorian group, score rather less well; even A - 7th cent. - comes out badly here. To the fact that F and the Nest. authorities show a surprising degree of resemblance to MT - which creates a presumption of fidelity to the Urtext - we shall presently return. In the meantime we note that if we attempt to locate the Urtext on the map, by applying the third "fragment" technique to the above figures, we find that it comes out at the position marked Ω₁ - away towards the S.E.

(B) The argument above is open to a serious objection. We know that in the majority of the passages considered, the reading which most resembled MT might also be held to have arisen through subsequent revision after G'. It has moreover been shewn that G' did exert considerable influence on the tradition of P'. Surely, it may therefore be urged, this detracts from the value of our figures above; for whereas they are intended to indicate the degree to which each ms resembles MT and by implication the Urtext, they may in fact do little more than measure to what extent each ms embodies corrections after G'.

I therefore searched for passages in which (i) one reading stood closer to MT than did any of its rivals, and (11) this greater resemblance could not be explained by

1. Barnes, pp.xxxiv f; see also his article: "On the influence of the Septuagint on the Peshitta", J.T.S. (1901), pp. 186-197.
assimilation to the G' tradition. In the list which follows, I cite G' in order that the reader can verify this. Thus I obtained a "base" of 37 test passages, in which the "MT" reading had a prima facie claim to be considered original - with a rather greater likelihood than for most of the 280-odd "MT" readings considered in (A). Even here, of course, we cannot be certain that any particular "MT" variant is ipso facto original. However, we have removed the one factor which might have systematically distorted our results. The price that we have had to pay is that the number of passages on which our estimate of Ω's location will be based, has been very much reduced, and a subjective element has necessarily been involved in their selection and treatment.

We thus have a second index of the extent to which a ms deviates from Ω: the number of times it fails to have the "MT" reading, over a selection of test passages, as a percentage of the number of test passages in which that ms is available.

Before proceeding to the list of test passages, I would add some remarks of explanation. The fourth column gives the alternative P' readings, one below the other. The uppermost is that which stands closest to MT; this greater

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1. For this purpose I consulted the Göttingen Septuagint (vol. X, Psalmi cum Odis, ed. A. Rahlfs; 1931). When the variation concerns the construction rather than the meaning (e.g. the use of the construct state as against the Dalath), the Greek text of G' is irrelevant, and I have instead cited the Syrohexaplar, according to Ceriani's reproduction (Milan 1874), for the same purpose.
resemblance is occasionally formal (e.g. the use of the construct state as opposed to Dalath), but usually more substantial. In column 5, I have given the reading of G' (or, when appropriate, Syh.). This has normally been placed in the first line; but if it particularly resembles one of the P' variants, I have placed it in the appropriate line.

Test "passages" I, XVI and XVII are composite, in that I have grouped together a number of clearly inter-related variants, in which the mss vote in a similar (though not identical) fashion. Ia and Ib occur in immediate juxtaposition; XVI represents four places in which MT has

and the mss split between יִרְאוּ and יִרְאוֹ; XVII refers to two parallel verses in י́פִּג. As I have already stated, I do not approve in general of "weighting" variants; but rather than count each member of a set of closely related variants as a test passage in its own right, I have in each of the three cases taken all the n passages (n = 2 for I, XVII and n = 5 for XVI) together, attaching a weight of 1/n to each in the subsequent calculation. Table B. 7. 4. shows my procedure in detail.

The "voting" of the 21 mss in these passages is given in Table B. 7. 4. The reading closest to MT is indicated by a stroke, whereas 1 indicates the alternative reading. (In VII there are two of these, denoted 1 and 2). A blank indicates that the ms in question is defective; a cross, that it offers a peculiar reading not found in any of the other mss. The last three rows give: (i) the number of test passages in which each witness is extant; (ii) the
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**Passages**
- Extant: 36° 35° 37° 27° 37° 37° 37° 37° 33° 39° 34° 30° 23° 33° 23° 14° 31° 37° 37° 37°
- 'Non-MT' Readings: 15° 23° 12° 15° 21° 13° 6° 16° 15° 16° 15° 15° 12° 8° 15° 12° 7° 17° 18° 17° 23°

**Percentage**
- 42 66 32 55 57 35 39 43 41 48 48 46 40 36 47 53 50 57 49 46 62
number of deviations from the MT-type reading: (iii) the latter as a percentage of the former. As the mss vary greatly in the extent of their testimony over these passages, it is again the percentages rather than the absolute numbers of deviations which matter.

We thus obtain a second series of estimates of the divergence of each ms from $\Omega$. Let us compare them with those found under (A):

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The two methods give rather similar results. Here the best scores are attained by (i) C, with the textually similar GHQ not far behind¹; (ii) F; (iii) the Nestorian mss, the best of which are ON. Cod.A is in 8th place, as opposed to 20th under method (A). The worst authorities are all Western (R D E Le B).

If we apply the third "fragment" technique to these figures, we obtain a location for \( \Omega \) at the point marked \( \Omega_2 \) on the map. This is in the same region as \( \Omega_1 \), and for text-critical purposes there is no difference between the two locations.

(C) There is also an intuitive reason for locating \( \Omega \) in that area. We recall that the "East-West" dimension corresponds — however loosely — to the geographical factor of East/West, whereas the "North-South" dimension reflects the date of the mss, the younger ones tending towards the top. Now, we expect \( \Omega \) to be geographically between the Eastern and Western groups (i.e. to the right of the Western and the left of the Eastern), and to be older than any of the mss (i.e. to lie at the bottom of the page). This provides further confirmation for our location of \( \Omega \).

We are now confident that \( \Omega \) has been correctly placed. What does the map now offer to the textual critic who wishes to formulate a policy for discriminating between

1. However S has dropped from 9th place (A) to 15th (B).
rival readings on distributional grounds? An overlay can of course be drawn at any point in the text where the mss vary; but two important guidelines immediately emerge:

1. A high degree of likelihood attaches to a reading attested both by F and by the Nestorian witnesses, cf fig. B. 7. 3.:
If some of the Nestorian witnesses do not agree with F, but there are two or more which do, then this rule still holds. However, if F is supported by only one Nestorian ms, we must consider the possibility that the agreement is coincidental, and that the reading does not go back to the Urtext.

2. If F has little or no Nestorian support, then a considerable likelihood - but less than in (1) - attaches to the reading of F, even if it is unique to F. This is because it is possible that an error has covered the area occupied by all the other mss (fig. B. 7. 4a); it is of course also possible that the error is in F, and this is indeed the likely explanation of the majority of F's unique readings (fig. B. 7. 4b):

Fig. B. 7. 4a

Fig. B. 7. 4b
It is likely that we shall find F's unique readings more often wrong than right. In the special study of these readings, later in this chapter, I found that about one quarter of them seemed to preserve the Urtext.

However, the likelihood to be assigned to a reading of F increases when it has the support of some or all of its neighbours - Z C S Q G H, in that order. The greater this support, the more likely it is that we are dealing not with an error confined to a small region of the map, but with the reading of Ω. I have found very few cases in which a reading attested by F and by as many neighbouring mss as are available cannot be regarded as original. Thus the situation in fig. B. 7. 5a is more likely than that in fig. B. 7. 5b:
On the other hand, the support of Western authorities *not* in the neighbourhood (and this includes A) will add very little to the likelihood attached to the reading of F.

I hasten to make it clear that these rules do not mean that we must always follow F, forsaking all others. The reason that F figures so prominently is that (i) it is one of the closest of the mss to Ω, and (ii) it stands apart from the rest of the Western cluster. We shall in fact more often than not reject a reading found only in F (see discussion below) or only in F and those mss which do not neighbour on it, e.g.

ψ 48:14] μήθερ F = R T, μήθερ rel. (so MT, G')
ψ 50:4] μήθερ F = A, μήθερ rel. (so MT, G')
ψ 67:8] μήθερ F = B R Le, μήθερ rel. (so MT, G')
ψ 116:9] μήθερ F = R Le, μήθερ rel. (so MT, G')
ψ 143:2] μήθερ F = B R (so G'), μήθερ rel. (so MT)

These two rules are not to be followed blindly, but applied in the light of the character of the readings in question. In particular, if there is good reason to suppose that F and the Nestorians have independently made the same change in the text, we must depart from rule (1). However, even as matters of general policy they conflict with the procedures of Barnes and other scholars, and I can hardly expect them to find general acceptance on the sole ground that they follow from the novel technique of textual
The crucial point is of course how we evaluate F. This ms has aroused considerable attention, mainly because it has been observed, not only in ψ but also in other books of the O.T., to depart on occasion from the other F' ms to agree with MT. The conclusion which Barnes drew from his study of the Psalter, as well as an earlier study of Chronicles, was that F had subsequently been accommodated to the Hebrew. Another feature which F has been found to exhibit in the Psalter and elsewhere is a tendency to join the Nestorian authorities — although it shows on the whole greater textual similarity to Jacobite mss, gives the Jacobite spelling, and is written in a hand which in no way suggests a Nestorian origin. Barnes took this to mean that F was a basically Western ms which had undergone Nestorian influence and contained "a considerable Nestorian element".

There are, as we shall presently see, other ways to interpret both these features of F; but it is a fact that Barnes, who had devoted a special study to the Peshitta Psalter, would not have accepted either of our two rules. Given a passage wherein F agrees with MT against all the other mss, we would proceed by virtue of our Rule 2 to investigate the possibility that the original reading survives in F alone. Barnes, however, would attribute F's reading to revision after MT and give priority forthwith to the reading of the

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1. For a detailed discussion, and for references to the studies of the P' text of other books alluded to over the next few pages, see p. 7:71

2. In Isaiah, and also Chronicles (though it is not certain whether s=17e1, the one Eastern ms collated by Barnes, can truly be said to represent a Nestorian text; see Thes., p. 9:47).
majority; his Introduction mentions several places where he put that policy into effect (p. xvii). Again, if we are confronted with a reading supported by F and the Nestorian witnesses against other Western authorities, we shall be very much inclined to follow Rule 1 and to prefer the reading concerned; but Barnes would have suspected it of being a purely Nestorian reading which F had picked up— or, if F agreed with MT as well as with Nestorian authorities, of being the result of revision after MT—and he would have preferred its rival. A notable example is at ψ 68:19, where MT has ἄνσοι and Barnes accepted ἄνεςα (so all the Western authorities collated, except for F) rather than ἄνεςα (F and the Nestorian witnesses). Nor are these the only cases wherein our critical policy differs from his. Barnes would have accepted a reading which was solidly attested by the Nestorian mss together with one or more of the early Jacobite mss (p. xl11); this is not compatible with preferring (as I would at ψ 81:6, for example) the reading of A F H Q S Z (ἕλικονα) to that of B E J K L N T R X m Ua Uc Le (ἵλικονα), in accordance with our Rule 2.

1. The passage is discussed in detail below (pp. 10:23 ff.).
If our rules disagree with Barnes' policy, they must stand or fall on their own merits as established by "intrinsic" approaches to textual criticism. A further investigation is therefore necessary. Let us consider the following questions:

1) What evidence is there that F has been influenced by the Nestorian text?

2) Can we find any passages where there are cogent independent grounds either for accepting or for rejecting a reading attested by F together with the Nestorian witnesses?

3) Do the peculiar readings of F give in themselves any indication whether they are better explained by revision or by unique preservation?

Regarding the first question, I would point out that F shows no greater similarity towards the Nestorian text than do most other Western mss. Indeed, my figures of the number of disagreements\(^1\) between F and the Nestorian witnesses are greater than for several other Western authorities (viz C D E G H J Q S T Le). This is reflected in the map: F does not stand away to the "east" of the other Western mss— as we should have expected had F (like Barhebraeus) undergone considerable Nestorian influence— but to the "south".

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1. as a percentage of the number of variant passages in which the readings in question are extant ("percentage" distances).
Ultimately, however, the existence of a historical link between F and the Nestorians is to be proved or disproved by the criterion of whether they agree \textit{in error}, and not by textual similarity in itself. This brings us to our second question. I have collected those variants attested by F and the Nestorians against most other authorities, to see whether it can be shewn, in any passage where the agreement could not be due to coincidence, \textit{either} that the shared reading was not original (in which case we could conclude with Barnes that F had undergone Nestorian influence) \textit{or} that it was original (which would confirm our first rule). In most cases there seemed to be no conclusive argument either way. Usually this was because MT agreed with G', and a reading which could be recommended for agreeing with the former had to be treated with caution because of its agreement with the latter. However, in three passages it does seem likely that F and the Nestorian mss have the original reading, \textit{viz}:

\[\psi 39:7\] (\textit{= Test passage XIII}).

\begin{verbatim}
\[
\begin{array}{ccccccccc}
MT & \text{אֶלֶד} & \text{יִנְשָׁךְ} & \text{יְהוָה} & \text{עַל} & \text{יְשֵׁבָה} \\
\text{F K L O X} & \text{m U a U c} & \text{A B C D E H J N (Q) R} \\
\text{G'} & \text{יֵשָׁךְ} & \text{וֹאָלָם} & \text{שָׁבָה} & \text{אָרְרָא} & \text{אֹתָא} \\
\end{array}
\end{verbatim}

The reading of F + Nest. is clearly derived from the Hebrew, not the Greek, and is surely original.
Evidently the Hebrew was vocalised ל"ד. The waw of F etc. is to be preferred to beth, in that it was probably in the original translation from the Hebrew; had it come in from G', the rest of the word (אלהים) would surely have been changed too.

Now the reading of F etc. might be, theoretically, either retained from the original translation (cf. the Hebrew), or brought in later from the Greek. However,
Ephraim Syrus (fourth cent.), in his Prose Refutations\(^1\) i 41, lines 9-11, has הלא. Thus F etc. seem to have the original reading.

There seemed to be no cogent intrinsic grounds either for or against the remaining readings which are carried by F and the Nestorian mss with little or no support from the other Western authorities. These I have found at \(\psi\) 17:6; 36:7; 42:9; 45:3; 62:9; 68:19; 73:14; 74:20; 78:6,71; 80:19; 81:13; 104:3,10; 105:8,16; 109:12; 125:5; 128:6; 131:2; 135:9; 144:8,11; 146:5; 147:19.

We now come to the third question, the unique agreements between F and MT. This poses a delicate problem, which has a bearing on the F' text of the O.T. in general. It will be convenient to present that part of the discussion which concerns \(\psi\) in the present chapter, and to reserve the consideration of other books for an Additional Note (pp. 7:71 ff.).

Given the fact that we have agreements between F and MT which cannot be attributed to mere coincidence, we may set up five alternative hypotheses:

(a) F, or a source of F, has been revised after the Hebrew.

(b) F preserves uniquely the reading of the Urtext, which the other mss have lost.

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1. See Thes., p. 8:35.
(c) There existed another old translation of the Psalter from Hebrew into Syriac, independent of that which was adopted by the Church and became known as the Peshitta. There are occasions on which F preserves the reading of this other translation, of which no other trace remains.

(d) There existed in early times more than one "unofficial" rendering of $\psi$ into Syriac; these were later worked into a single "authorised" version\(^1\). From time to time, the majority of the mss present that reading which received "official" recognition, while F preserves a reading derived from another translation of $\psi$ from Hebrew into Syriac\(^2\).

(e) F has been influenced by another ancient version in various passages where that version closely follows MT; both G' and the minor Greek versions ($\lambda \sigma' \theta'$) have been suggested.

Before proceeding to any test passages, we may discuss briefly each of these hypotheses, assessing its intrinsic likelihood and identifying the symptoms whereby it may be recognised.

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1. This is of course the "Kahle view".

2. The difference between (c) and (d) is that (c) implies that the church adopted forthwith as canonical one particular translation, whereas (d) postulates a process of editorial activity whereby an "official" text was compiled eclectically from the differing "unofficial" translations that existed.
The idea that a cent. ix ms such as F should have been corrected after the Hebrew may seem far-fetched, but a parallel can be traced among mediaeval mss of the Vulgate. In a study, based primarily on the Psalterium 1uxta Hebraeos, of readings wherein members of the Theodulfian family depart from the majority to agree with MT, E.Power considered various alternative hypotheses, corresponding roughly to those listed above, and found that the evidence pointed beyond doubt to the conclusion that "the Codex Hubertianus, the oldest (cent. viii/ix) of the Theodulfian Mss., had been extensively corrected from the Hebrew" and moreover that "a number of Theodulfian and allied Mss. of the ninth century had lost some of the Hubertian corrections and at the same time incorporated in the text of the Psalter a number of new corrections from the Hebrew" (p. 234). Thus we have examples of accommodation to the Hebrew, from a number of different hands; and Power's soundings elsewhere in the O.T. suggested that the situation was probably similar in other books (p. 257). To turn now to P', a scribe who was acquainted with Hebrew might well feel justified in correcting away discrepancies between his P' text and whatever Hebrew text he had available. That the belief was widespread among the Syrians that P' was derived from a Hebrew original, may be deduced from the title of Ψ in A and other early mss2, which states

2. The Syriac text is cited on p. 8:26.
that the Psalms were translated "from Hebrew into Greek (!) and from Greek into Syriac". Should it be doubted whether the Syriac-speaking church produced anyone who was sufficiently versed in Hebrew to carry out such a revision, we can at least point to one example, namely Jacob of Edessa (c. 640-708). W. Wright has published 1 two letters, preserved in the ms B.M. Add. 12172, which were written by Jacob and "present him to us as a man of marvellous learning for his age: - an ἀνὴρ πρίγλαντος , who was equally conversant with Syriac, Greek and Hebrew" 2. The further possibility suggests itself that others too within the Syriac-speaking church may have known enough Hebrew to attempt to conform the P text to MT 3.


2. Further evidence of Jacob's knowledge of Hebrew may be drawn from Wright's Catalogue of the Syriac Mss in the British Museum (p.430), and R. Schröter, "Erster Brief Jakob's von Edessa an Johannes den Styliten", ZDMG (1870), pp. 261-300.

3. If it were to be shewn that revision after MT was ever performed on mss of P', it would be tempting to associate such revision with Jacob himself, whom Barhebraeus expressly states (Chron. Eccles., ed. J.B. Abbeles and T.J. Lamy, Louvain 1872, I.291) to have spent nine years correcting (ἐκπίνεν) the text of the O.T. The hypothesis that he consulted the Hebrew in formulating his own version of the O.T. in Syriac could be tested by a study of those extant mss which contain part of it (B.M. Add. 14429 and 14441, covering Samuel and Isaiah). Counting somewhat against the hypothesis is the fact that the colophon of the former ms states that Syriac and Greek sources were employed, but does not mention Hebrew.
Characteristics that would tend to prove that a reading wherein F agreed with MT was due to such a revision are:

1) that it conflicts with the peculiar character of the translation as established from a study of the translation technique throughout the book;

2) that the reading in question and that of the majority each proclaims itself as a rendering made directly from a Hebrew original, and neither can be explained otherwise (e.g. as a corruption of the other).

Furthermore, inasmuch as a reviser may be expected to have worked in a reasonably systematic fashion, one will not expect to find, in the immediate neighbourhood of a passage wherein F has actually been conformed to MT, a passage at which the reading presented by F shows an obvious disagreement with MT.

Hypothesis (b) may also appear improbable; it is natural to doubt whether a single ms is the only one to have preserved the original reading, at any point in the text. Were this a "closed" tradition subject to the laws of Maasian stemmatics, the only situations wherein the supposition of unique preservation in F could be accepted would be (1) if F were the ancestor of all the other extant mss, and (11) if the tradition were two-branched, with F as the sole representative of one of the two branches; but it is clear that neither fits the place of F within this tradition. However, "open" or contaminated traditions have
produced many examples of the survival of an ancient, and in
many cases original, reading in one ms or in a small
minority of the extant mss. G. Pasquali\textsuperscript{2} has collected
many examples, from the traditions of Homer, Eusebius and
several other authors; more recently, R.D. Dawe\textsuperscript{3} has shewn
that unique preservation is frequent among the mss of
Aeschylus. In that the tradition of the Peshitta Psalter
is heavily contaminated, we should not be surprised to meet
the same phenomenon there. The principal characteristic
which would suggest that a reading wherein F agrees with MT
is original, is that it should partake of the peculiar
character of the manner of translation as displayed elsewhere.
A second point which would favour this hypothesis would be the
appearance in F alone of ancient grammatical forms and spellings,
which create a presumption that there may be other ancient
material which F is the only ms to preserve.

1. The reader may gain some insight into this phenomenon
by considering Quentin's model tradition (Thes., p.514)
and supposing that, of the 22 mss, only QSX are extent.
There will be some original readings (such as \path/\textit{passage}, I.5) which Q alone preserves (in that C had produced an error
which was passed on to SX but not to Q); while elsewhere
(//aeque et//, I.3) the truth survives in X uniquely (for
QS depend partially on D, which originated and transmitted
the incorrect \textit{atque}).

2. "Storia della tradizione e critica del testo"
(Florence 1952, 2nd ed.), Ch.VI.

3. "The Collation and Investigation of Manuscripts
of Aeschylus", Cambridge 1964, Ch.V.
In support of the hypothesis (c) that P' was not the only ancient rendering of P' into Syriac, one may appeal to the existence in antiquity of many Latin Psalters (Gallican, iuxta Hebraeos, Roman, Cassinean). Agreements between F and MT which are to be attributed to a second old Syriac version of $\psi$ may be expected to exhibit characteristics (1) and (2) mentioned in the last paragraph but one. In addition, one would wish for (1) proper grounds for assigning the origin of F's alternative renderings to so early a date, and (11) an explanation of the fact that this particular collection of readings, rather than any other, was taken over by F from a Syriac Psalter other than P'.

We have already remarked that the "Kahle hypothesis" (d) in relation to the Peshitta Psalter has little to commend it on general grounds; nor does our study of the unique readings of F do anything to enhance its likelihood. But as it might prove helpful in relation to other books, I have accorded it a place among the alternative hypothesis.

Finally, we cannot overlook the possibility that the unique agreements between F and MT are due to the influence of another ancient version. Assimilation to G' is in itself not unlikely. At $\psi$ 2:12 F gives in the text the reading of the majority of mss (אכ ה), which follows MT (אכ ה), and notes in the margin וו ו, a rendering of G' דַּרְכָּה יָאַה. Now if F (let alone a source of F) introduced readings from G' into the margin, then one cannot rule out the possibility that readings derived from G' found their way into the text. But though
this cause may account for some of the agreements between F and MT, it cannot explain all of them, in that there are some passages in which MT and G' diverge substantially and F departs from all the other mss to agree with the former. It has been suggested instead that F may have made eclectic use of the later Greek versions, mediately through the Syro-hexaplar or otherwise; this could easily have happened, in much the same way as we sometimes cite the margins of the Revised Version. The hypothesis must be tested by careful comparison of the respective renderings; in many passages, of course, nothing can be said, the readings of these Greek versions having now perished.

Almost everyone of these hypotheses has found its advocates. We have already stated that Barnes formed the impression from his Chronicles study that F had been accommodated to MT, and he adhered to the same view for the Psalter. G. Diettrich, however, observing F's comparable behaviour in Isaiah, preferred to suppose that F was often the only ms to preserve the true reading. Certain remarks by P. Kahle1 can be taken as further support for (b), though they may be construed otherwise: "It is clear that we shall have to regard agreements with the Hebrew text in mss in general as belonging to the oldest parts of the Peshitta". He refers expressly to F in this context (pp. 267 f). Now I am not sure what Kahle meant by the phrase "the oldest

parts of the Peshitta". Are we to believe that there is a unique Urtext of the Peshitta, and that F is sometimes the only witness to preserve its reading (b)? Or should we postulate, as he does for the Targum and Septuagint, an initial multitude of "unofficial" translations, which were worked into a single version, and of which certain ancient elements survive in F, while equally ancient elements of other translations may be present in other mss at the same passage (d)? Hypothesis (c) has not been explicitly proposed; the view that other versions of the O.T. into Syriac existed which were of comparable age to F' or even older, may be discerned in Goshen-Gottstein's suggestion that further study may reveal in the Peshitta parallels to the survival "of extra- (or proto-) Septuagint traditions", and in L. Delekat's postulate of a Vetus Syra text of the O.T., based on the Lucianic recension of G'; but these supposed rival versions of the O.T. have not been suspected hitherto of being the source of F's unique agreements with MT. Finally, (e) was proposed in B. Albrektson's study of Lamentations (where G' was thought

1. As Kahle does not support (d) unequivocally, our use of the phrase "Kahle view" is perhaps misleading, but it is by other considerations the most convenient term.


to be the version which influenced F) and in a review, by F.C. Burkitt\(^1\), of Barnes' edition of the Peshitta Psalter (where it is suggested that the later Greek versions are responsible). Scholarly opinion could hardly be more varied; and, due allowance being made for the possibility that the underlying cause of this phenomenon may vary somewhat from one book to another, these divergent views cannot all be correct. This underlines the necessity to check our findings over the Psalter against whatever other material can be gathered from the remainder of the O.T.

In my own attempt to choose between the rival hypotheses over \(\psi\), I collected the fifty-three passages in which F offers a unique reading\(^2\). Less then half of these readings tend towards MT. I then examined them in turn in order to see whether any of them could be regarded as a "crucial" passage, presenting an opportunity of discriminating between the five hypotheses. The most welcome sort of crucial passage is that which points to one of the hypotheses as being more plausible than any of its rivals; but any opportunity for comparison, even if all that is shewn is that one of the hypotheses is rather less likely than the other four, is valuable. The passages which could be employed in such a way turned out to be few, but tended to a reasonably firm conclusion.

2. Fifty from the apparatus itself; three (\(\psi8:9, 22:2, 104:22\)) are mentioned in the introduction only (p.xvii). It is noteworthy that twelve fall within the relatively narrow compass of \(\psi139-148\).
Our plan will be, first to discuss these crucial passages, and to discover which of our hypotheses emerges from that discussion as the most satisfactory; and then to list and, where appropriate, comment on all of F's unique readings in ψ.

What is probably the most important of our crucial passages is at ψ 141:1. There, MT has "הש"ע ש"ע י Warsaw. In most Pesh. mss we find הושע י Warsaw, but F has for the last two words י Warsaw. Now, in seven other Psalm passages in which the root י Warsaw occurs (ψ 22:20, 38:23, 40:14 = 70:2, 55:9, 70:6, 71:12), P' renders it by the root י Warsaw, according to the unanimous testimony of the authorities collated by Barnes. It is a surprising translation, in that י Warsaw means "hasten" and י Warsaw "wait"; indeed the exact meaning of י Warsaw in this context is by no means clear to us ("consider me attentively"? "stay with me"?).¹ The verb י Warsaw occurs in only two other Psalm passages, in which it is rendered differently, perhaps under the influence of G':

ψ 90:12 י Warsaw י Warsaw י Warsaw (G' ק.textBox)
ψ 119:60 י Warsaw י Warsaw י Warsaw (G' היקדרלודיה)

Why did the translator render י Warsaw, at least in the seven passages above, by י Warsaw? One can hardly be confident,

¹. But the meaning must have been clear to the translator himself, because in ψ 22:3 he renders י Warsaw י Warsaw י Warsaw by י Warsaw י Warsaw (the only other occurrence of י Warsaw in ψ).
but it is possible that these forms were mentally associated with \( \sqrt{\text{השנה}} \) ("be silent, inactive"), which comes close to "hesitate, wait" in Jud 18:9 and 2 Kings 7:9. Now an examination of the treatment of \( \sqrt{\text{השנה}} \) by the ancient versions in the 22 passages where it occurs throughout the O.T. shows no parallel to this rendering in any version other than P', and few echoes in P' outside the Psalms. It is fair to say that the rendering of \( \sqrt{\text{השנה}} \) by \( \sqrt{\text{השנה}} \) is a striking characteristic of the Peshitta Psalter, and that F is the only ms which presents that characteristic in this passage.

Within the other ancient versions, "hasten" is the most frequent rendering of \( \sqrt{\text{השנה}} \). There are a number of exceptions, to list all of which would take us too far out of our way; we shall confine ourselves here to listing occurrences in \( \psi \).

G' uses \( \text{κρομμετ} \) for \( \text{נָהָר} \) at \( \psi 22:20, 38:23, 40:14 \) (where some mss have \( \text{אֵשֶׁר} \)), 70:2, 71:12; \( \text{βοσάς} \) at 70:6; \( \text{εἰς ἡμῶν} \) at 141:1. In \( \psi 55:9 \), \( \text{נָהָר} \) is rendered by G' \( \text{κρομμετ} \) \( \deltaιομήν} \), which admittedly comes within sight of \( \sqrt{\text{השנה}} \), but this probably indicates a reading \( \text{נָהָר} \) (so H. Bardiske in BHS), the resemblance to P' being accidental. T' uses \( \sqrt{\text{השנה}} \) "consider" in \( \psi 55:9, 141:1 \), but in all the other passages renders "hasten". H' has "hasten" throughout. The renderings of the minor Greek versions,

1. In all the \( \psi \) passages in question, the verb is cohortative (\( \text{נָהָר} \) or \( \text{נָהָר} \)), and the \( \text{נ} \) may have suggested this etymology.

2. P' there has \( \text{נָהָר} \). There is no need to suppose either that P' also read \( \text{נָהָר} \) or that P' has here been influenced by G'.
throughout the O.T., will be found on p. 715; the sense "hasten" is usual, and "wait" does not appear at all.

In P' outside the Psalter, we usually find the idea of speed:

- Dt 32:35, Is 5:19, Job 31:5
- 1 Sam 20:38
- Is 8:1, 3

Occasionally, we find a guess:

- Is 28:16
- Is 60:22
- Hab 1:8
- Eccles. 2:25

and in Num 32:17, where MT has יָכַּה, P' apparently read (with G') יָכַּה. The two remaining passages wherein יָכַּה occurs are of special interest, because they are rendered in a manner similar to that which we have detected in יִּכְבּ. The first is at Job 20:2:

MT: יִּכְבּ

P': יִּכְָב אֹתְּרֶה

(G' 7al= A, 11f= B-M. Add. 14440)

and the second - in which the similarity is rather less certain - is at Jud 20:37. There most authorities state that the Israelite ambushers approached Gibeah quickly: MT יֵשָׁתֵר בָּקָה, G' וֹקָה (but G merely הֵשָׁתֵר), T' רָחְמָה, V' repente.
In P', however, we find לֹּא הָעָבָרָה הָאָבְרָדָה (so Le, 7al=A, 6h7-B.M. Add. 14430), which may mean: "the ambush came down slowly".  

Whatever our verdict on the Judges passage, it is noteworthy that וַיַּקְבָּר for וַיָּקְבָּר should reappear in Job. Now, it is unlikely that one translator was responsible both for וַיָּקְבָּר and for Job. Firstly, the translator of Job does not seem to have taken offence at anthropomorphisms applied to God, whereas the Peshitta Psalter is sensitive to them and usually tones them down.

Secondly, I have noticed (Thes., pp. C10 ff.) that whereas the Hebrew word יָשָׁב is usually rendered לֹּא in (39:6, 7, 12; 62:10; 94:11; 144:4), and the cognate לֹּא is not found throughout the book, we do find in Job the rendering לֹּא, never לֹּא (7:16; 9:29; 21:34). It seems then that the equation וַיָּקְבָּר -> לֹּא was not a quirk on the part of the Psalter translator alone but a tradition, albeit not a widespread one.

1. This seems to be the same sense of לֹּא in Gen 33:14 (MT יָשָׁב) and Job 37:11. However, a development from "softly" to "stealthily" is suggested by Payne-Smith here and in 1 Sam 24:5.


Before we sum up the implications of this crucial passage, we had better satisfy ourselves that מֵאַמֵּד in F is not merely the result of assimilation to a similarly worded passage. Let us therefore survey all eight יָאַב passages where the root appears. In seven of them, we find the phrase in which it is embodied to be:


The reading מֵאַמֵּד here can hardly be due to the influence of any of these passages; the wording is too dissimilar. In the only other יָאַב place to contain מֵאַמֵּד, viz 70:6, P! does have מֵאַמֵּד, but the surrounding contexts are quite different:

141:1 מֵאַמֵּד מַעֲמַק יָאַב נַחֲלָתָה מִתְקַבֵּל לָמוֹ מֵאַמֵּד

It is hard to see why any copyist should have introduced here the short phrase מֵאַמֵּד from יָאַב 70:6. Hence assimilation to another passage from the Psalter will not account for F's reading here.

Not all five hypotheses will account for מֵאַמֵּד equally well. This reading is unlikely to be due to revision (a); the reviser is most unlikely to have been acquainted with, and to have adhered to, the very uncommon tradition which took מֵאַמֵּד as "wait". To suppose that מֵאַמֵּד goes back to the original translation (b) and on account of its obscurity came to be replaced in most...
texts by מִלָּה, is far more satisfactory. The same cannot be said of (c); if מִלָּה comes from an ancient Syriac Psalter distinct from P', then it is strange that this second translation should possess one of the most arresting peculiarities of P' itself. Again, (d) creates, inter alia, the problem of why those who compiled the "authorised" version rejected מִלָּה here after accepting the same (or essentially the same) phrase on seven other occasions. Finally, Burkitt's hypothesis (e) that the reading may have come in from a minor Greek version cannot be tested directly, for those versions are not extant here; but in those places where מִלָּה occurs and the text of these versions survives, we find nothing which could inspire the translation "wait."

א' has σκέδω at Is 28:16; ψ 58:23, 40:14, 55:9; ξυσσεδω at Is 60:22; κοιματισμ in two places where only the Syriac survives, viz ψ22:20, 119:60; and ἄνηρ (נְעָן) for MT כַּה at ψ 90:10, σ' gives σκέδω at Is 28:16; ψ 58:23; ξυσσεδω at ψ 55:9; ἄνηρ for MT כַּה at ψ 90:10; the Syriac has לָסֶר at ψ 22:20 and 119:60; at ψ 70:6 we find מְשָׁר, which, whether it is sound ("Be poured out for me!" מְשָׁר = besprinkle, moisten) or a phonetic error for מְשָׁר ("Be stirred for me!") does not approach מִלָּה. θ' has σκέδω at Is 28:16; ψ 58:23, 55:9; at ψ 90:10 he joins G' in rendering כַּה by πραπτης. For E', Field gives σκέδω at ψ 22:20, and לָסֶר at ψ 90:10. There can thus be little doubt that any Hexaplaric rendering to which F might have had access would not have given rise to מִלָּה.

In sum the likeliest hypothesis on the showing of this one passage is (b).
A second crucial passage occurs at 104:10. Here, for MT פ"א נא, F alone has ככ, while the other witnesses read ככ. Now, of the three alternative forms of this preposition (in the unsuffixed form), this is our only attestation in the Psalter for ככ, according to Barnes' apparatus, while the other two forms are common. It seems from Payne-Smith that the form ככ without suffixes is rare. However, the phrase ככ is found again at Exod 32:12, where MT has ככ ככ. Apparently the unsuffixed ככ was used at an early period - and in particular within the phrase ככ - but subsequently lost ground to its rivals ככ and ככ. Why then do we find ככ ככ in F alone here? It is unlikely that ככ is a mere scribal error for ככ; assimilation to Exod 32:12 is hardly plausible, there being no clear instance of assimilation to a parallel passage among F's fifty-three unique readings. One may therefore suppose that ככ is an old reading, which cannot be explained by (a) or (e); it may be the original reading of F', which was changed in most Psalm texts to what became the more familiar ככ (b), or it may go back to an ancient Syriac version of פ other than that which supplied the reading ככ of the other mss - so (c) or (d).

1. ככ (note Seyâme) is the reading of the two mss which I have examined (5b1, 7a1) and of Barnes' edition of the Pentateuch in Syriac (British and Foreign Bible Society, London 1914).
Our next crucial passage, at ψ 110:4, allows us to test directly hypothesis (e). F departs markedly from the other mss to agree with the Hebrew, and the renderings of α' and σ' are available (in Syriac only) at that point:

MT

עַל בְּרֵיתֵי פְּלֵךְ-צְדִיקִין

F.

דְּלַהַם הַשִּׁמְשֹׁמַת

rell.

כְּרַמִּים הַשִּׁמְשֹׁמַת

G'

קַדָּה הַשִּׁמְשֹׁמַת פֶּלַחְסָדָא

Syro-H.

םַף הַשִּׁמְשֹׁמַת הַפֶּלַחְסָדָא

(in margin) אֹת הַשִּׁמְשֹׁמַת פֶּלַחְסָדָא

The Syriac rendering preserved in F (with ↓) is even closer to MT than is that of α'. The hypothesis that F (or a source of F) consulted Hexaplar mss can hardly be said to account for F's unique agreement here with MT. The translation represented here in F adheres so closely to the words of MT as to be obscure. On these grounds one might doubt whether it could have come from the original translator. However, if we compare the rendering of the somewhat similar phrase לע זְרוּ at ψ 45:5:

MT

בַּכָּכּ עַל-בֵּכָּרָאָן

F'

כְּרַמִּים דְּלַהַם דְּלַהַם פֶּלַחְסָדָא

we find that F' was indeed capable of such a degree of literalness. The hypotheses admitted by this passage, then, are (a), (b), (c) and (d), but not (e).
The passages to which we now come illustrate a noteworthy feature of F's unique agreements with MT: their patchiness. Let us take first Ἡ 69:11a, where Barnes suggests that F has been directly assimilated to the Hebrew:

MT Ῠαβנὰ Ἰακώβ υἱὸς Ἱωάννης
G' χαὶ συνεξάρσῃ...
Most F' mss Ἰησοῦς Χριστὸς
F Ἰησοῦς

Barnes' supposition seems reasonable enough. Let us look, however, at the second half of the verse:

MT ροῦτης λύτρον ἕως
F' ἱστάσεται ἵνα (so all Barnes' mss, including F)

If a reviser put in ὁμών, then it is strange that he should have overlooked the discrepancy between ἔως and ἵνα in the same verse. Again, at Ἡ 111:1:

MT βοσὸν καὶ θηρὶς ὀξὺν
Most F' mss ἰὸν ἐκ πόπλου ἱππῶν
F ἰὸν ἐκ πόπλου

Here too Barnes supposes accommodation to MT. Verses 7b and 8a of this same Psalm, however, have been transposed by

1. for which some mss have συνεξάρσῃ, which is usually regarded as a corruption.
P' according to all the mss:

MT  פ'  פ"ז  נ"ע  ל"ד  ה"ג  מ"ו  ו"ב  נ"ד  ל"ג

It is difficult to understand how the reviser who was meticulous enough to correct V.1 in this way, could have failed to rectify the far more serious divergence in VV. 7-8. And in general it can be said that for every passage wherein F may be thought to bear the marks of correction after MT, one does not have to look far (certainly not outside the same Psalm) to find a discrepancy between MT and P' which is at least as glaring as that which is alleged to have been corrected away. In order to attribute F's agreements with MT to revision, we must suppose

either that the reviser worked in an utterly haphazard fashion

or that the revision was effected not by F itself but by an ancestor thereof, and was then carried out thoroughly, but that in the intervening time this revised text was gradually assimilated, in most passages but not all, to the prevailing text.

The former hypothesis is hardly feasible, and the latter too complex to commend itself. Thus the evidence of all these passages militates against (a), though it does not favour any particular one of the four remaining hypotheses.

One further argument deserves to be considered. Three of our five hypotheses, namely (a) (c) (d), involve the proposition that there are occasions on which the two
alternative readings - that of F and that of the majority - represent two different translations made directly from the Hebrew. We have already stated (pp. 7:11 f) that for none of the textual variations which appear in the Peshitta Psalter is it necessary to postulate two different translations from a Hebrew source in order to account for the readings attested; and the reader can verify from the following pages that this is true in particular of the fifty-three passages wherein F stands alone. Now if any one of these three hypotheses were correct, we should expect to encounter at least one passage among the fifty-three wherein the conclusion could not be avoided that the two rival readings represent two different translations each made from a Hebrew original; and as no such instance can be found, there is no positive evidence for the second point of direct contact with the Hebrew which is demanded by (a) (c) (d). In this respect, the two remaining hypotheses (b) (e), which involve only one point of contact with a Hebrew text, appear simpler and to that extent preferable.

If we now review our five hypotheses in the light of the above evidence, there can be little doubt that the only one in which no serious flaw has yet been discerned is (b). Let us therefore adopt (b) as a working hypothesis in our examination of all the readings unique to F. Of course by adopting (b) we do not assert that every reading unique to F - even if it tends in the direction of MT - preserves the original text of P'; the various factors which give rise to unique readings in other mss (e.g. scribal error) must
be borne in mind no less in the case of F. Thus the task remains of ascertaining just where unique preservation has occurred.

Our list of F's unique readings begins with the five passages wherein F differs in "content" words (nouns, verbal roots) from the other mss:

(a) \( \psi 68:11 \) MT יָנָה, F לַּתְנָה, rell. לַתְּנָה. The verb לַתְנָה is attested in \( \psi \) here and in the preceding verse only; in either case it is rivalled by a reading which consists of some part of the verb לַתְנָה, while MT has לַתְנָה. Now לַח is rendered by לַתְנָה, according to all the mss, in 11 other passages (\( \psi 8:14; 57:7; 65:10; 74:16; 89:38; 90:17,17; 99:4; 101:7; 102:29; 119:90 \)), and this seems to have been the translator's usual equivalent. I would therefore prefer, in \( \psi 68:10 \), מָהָּ (Le Ua Uc = G J K L N O m bH) to מְהָּ (A B C D E F [H] Q R S Z)\(^1\) which arose by scribal error; and I would accept, in \( \psi 68:11 \), לַּתְנָה on the sole authority of F, regarding לַתְנָה as a corruption.

(b) \( \psi 69:11 \) MT יָנָה יָנָה, F לַתְנָה לַתְנָה rell. ...

\(^1\) According to Rule 2, however, this reading would have been expected to have the advantage, in that it is attested by FZCSQ (though not by G). But Rule 2 gives only a degree of likelihood, not certainty; and it is outweighed here by an intrinsic consideration.
This passage has already been alluded to (p. 7:54). The reading of F may once again be regarded as original, that of the majority being an assimilation to ￥35:13, where MT has יִשָּׁבֶתָה שְׁלֹשׁ and all the P' mss

The graphical change would not have been considerable.

(For a fuller account of the textual evidence, see our discussion above, p. 7:53)

The two P' readings are in themselves quite capable of being regarded as two different renderings of the Hebrew, that of F being literal to the point of obscurity, its rival embodying a particular interpretation. However, it is not essential to postulate two points of contact with the Hebrew; the hypothesis that F alone preserves the original reading of P' will account for the facts equally well. We have shewn above that the degree of literalness of F's rendering here is by no means alien to the mode of translation to be observed in the Peshitta Psalter. The origin of the majority reading can be explained through the fact that the same phrase כֹּל וּלָּתָא אָבָא is found within the Peshitta of the New Testament, not only in the direct quotations of this verse in Hebrews 5:6, 7:17, but
also within the discussion in Heb. 5-7 on the priesthood of Christ (5:10, 6:20, 7:11, where we also find 

Apparently the translator of Hebrews took the phrase κατὰ τὴν τότεν Μελχισεδὲκ to mean that Christ was priest not in the succession but in the likeness of Melchizedek, and there seems no obstacle to the belief that most of the mss have assimilated their Psalter readings to that of Hebrews, F alone preserving the original rendering.

(d) 139:16 MT יְנָעֵשׁ יְנָעֵשׁ

Fvld לְעָנָשׁ לְעָנָשׁ

rell. לְעָנָשׁ "

G' יִמְּרָא קַלָּשֹׁפָנָה

Vogel (p. 211) accepts the reading לְעָנָשׁ, and supposes that Pesh. "misread" יְנָעֵשׁ; he compares the equation רַעָשִׁי רַעָשִׁי in Is 28:20. According to this, F is likely to have been revised after MT. However, the verb לְעָנָשׁ is not used elsewhere in the Psalter, while יְנָעֵשׁ is fairly common, rendering יְנָעֵשׁ in 74:17, 94:9, 104:26. What counts even more heavily against לְעָנָשׁ is that the phrase לְעָנָשׁ לְעָנָשׁ occurs in Mat. 24:22. I therefore suggest that לְעָנָשׁ is original, but that the text was assimilated in most mss to Mat 24:22, all the more easily.

1. He discusses whether "misreading" is the correct term on pp. 213 ff.

2. unless we count the impersonal כָּלָה לְעָנָשׁ, which is found in 69:21.
because of the obscure meaning of רְשִׁיעָה and the slightness of the graphical change.

(e) is our first crucial passage, at יַע 14:1, which has already been discussed.

We now come to those places where F deviates from the other mss with respect to grammatical morphemes only, in the direction of MT. Here there is a complicating factor. There exist about twenty passages where F presents a unique reading which differs from that of the majority in purely "grammatical" respects but does not lie closer to MT and in general has little to recommend it. We may conclude that F (or a source of F) treated grammatical morphemes with less than due care, and quite possibly regarded them as fair game for emendation. Thus in those passages wherein F does stand closer to MT, we must try to decide whether this is due to scribal error, emendation etc., or to unique preservation.

Let us first list those passages in which F's unique reading does not tend in the direction of MT:

2:10  F שָׁבֵעַ, rel. לָאָמָר גָּדָה, MT צֶּרַח שָׁבֵעַ
17:12 F לִשְׁתַּקְיָה, rel. לִשְׁתַּקְיָה, MT לִשְׁתַּקְיָה לִשְׁתַּקְיָה לִשְׁתַּקְיָה
30:12 F אָמַר לֶזַּח מְסֹמֶךְ, rel. לְכָּלַח מְסֹמֶךְ, MT אָמַר לֶזַּח מְסֹמֶךְ אָמַר לֶזַּח מְסֹמֶךְ אָמַר לֶזַּח מְסֹמֶךְ
31:20 F רַבִּים לֹא לָאָמָר, rel. לְכָּלַח לְכָּלַח, MT רַבִּים לֹא לָאָמָר רַבִּים לֹא לָאָמָר רַבִּים לֹא לָאָמָר
The only ones which must have been unintentional are 50:15 (perseveration), 51:3, 80:2 and 96:12.

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1. the existence of which was detected by Barnes (p. xvii)
2. unless the Alaph is prosthetic and has been preserved here alone. The Aphel of this root is hardly attested (P-S).
Now that we have some idea of the sort of changes—deliberate or otherwise—which were introduced into F, let us turn to those 'grammatical' variants in which F stands nearer to MT. (There are a few borderline cases in which F's special proximity to MT may be disputed, e.g. at 104:22, 106:23; these have been included in the following rather than the preceding list.) A symbol is attached to each passage to denote the explanation which seems the most probable: P, unique preservation; C, coincidental agreement with MT; G, assimilation to G'. The last is a real possibility because every P' ms is under suspicion of having been conformed to G', and F is not exempt. It turned out that all these passages were fitted by at least one of these three possibilities; indeed it was not always easy to choose between the three, and on such occasions I have written more than one symbol, in what appears to me to be decreasing order of likelihood. These judgments do not, of course, mean to be dogmatic.

18:9 F? [מרעא סותא], rell. סותא or סותא מִשְׁמָה הַמָּשָּׁל; 3.

22:2 F נַעֲשָׁה לְהוֹ, rell. ... חַלָּב

MT נֶשֶׁר מֵישָׁרִיָּה יִבְרָה לאבנִי

Here F, without the preposition Beth, does not run as smoothly as the alternative reading. PC.

47:4 F לַעֲבָדָה, rell. לַעֲבָדָה, MTبناء; PC

65:10 F בֵּית הָעוֹד הַלְּאָם הַמַּיִם, rell. בֵּית הָעוֹד הַלְּאָם הַמַּיִם, MT בֵּית הָעוֹד הַלְּאָם הַמַּיִם PG equal

G 8 potamos ... MT בֵּית הָעוֹד הַלְּאָם הַמַּיִם PG equal

85:9 F נַעֲשָׁה לְהוֹ, rell. ... כַּעֲבָדָה, MT נַעֲשָׁה לְהוֹ PC
89:20 F [וֹלִ֣דוּ לָהּ] יִשְׂרָאֵ֑ל, ... מִיַּ֣רְעַל הָאֲשֶׁרֲךָ, ... מִיַּ֣רְעַל הָאֲשֶׁרֲךָ [vöv ol du lai yiraal israel, ... miru'al haasherakh lahash deru'al haasherakh], ... miru'al haasherakh deru'al haasherakh

MT ... תֵּלִֽה הַשְּׁלֵמִ֖ים הָאָֽשֶׁרֲךָ ... מִיַּ֣רְעַל הָאֲשֶׁרֲךָ

G' ... תֵּלִֽה הַשְּׁלֵמִ֖ים הָאָֽשֶׁרֲךָ ... מִיַּ֣רְעַל הָאֲשֶׁרֲךָ

PG ... תֵּלִֽה הַשְּׁלֵמִ֖ים הָאָֽשֶׁרֲךָ ... מִיַּ֣רְעַל הָאֲשֶׁרֲךָ

90:17 F ... מִיַּ֣רְעַל הָאֲשֶׁרֲךָ, ... מִיַּ֣רְעַל הָאֲשֶׁרֲךָ

MT ... תֵּלִֽה הַשְּׁלֵמִ֖ים הָאָֽשֶׁרֲךָ ...

PC equal

94:23 F ... יִשְׁכַּ֣בְתִּי נָשָׁ֑יוֹן, ... יִשְׁכַּ֣בְתִּי נָשָׁ֑יוֹן

MT ... יִשְׁכַּ֣בְתִּי נָשָׁ֑יוֹן ...

G' ... יִשְׁכַּ֣בְתִּי נָשָׁ֑יוֹן ...

Both seem due either to emendation or to the influence of G'.

In מַלְאִים, F is closer to MT, but not in מַלְאִים. Both seem due either to emendation or to the influence of G'.

104.22 MT ... נֹלְנָ֣ה הָנָֽאָמָ֖ם נֹלְנָ֣ה הָנָֽאָמָ֖ם

Most P' ms ... נֹלְנָ֣ה הָנָֽאָמָ֖ם

F ... נֹלְנָ֣ה הָנָֽאָמָ֖ם

The phrase יִשְׁכַּ֣בְתִּי נָשָׁ֑יוֹן generally means "in the east" (e.g. 2 Kings 10:33); but the meaning "at sunrise" is more appropriate here, and, although not mentioned in P-S, can be defended on the analogy of the use of לֶבַן נוֹמָ֣ק to mean "at sunset" (e.g. Exod. 12:6) as well as "in the west".

Hence this reading is in itself capable of being original.

That of F may be explained as an emendation, מִיַּ֣רְעַל, being intended as an Aphel participle ("He causes the sun to rise...") the better to harmonise with יִשְׁכַּ֣בְתִּי נָשָׁ֑יוֹן (V.20) - in much the same way as Gunkel advocated the emendation מִיַּ֣רְעַל for the Hebrew.

1. This seems better than taking מִיַּ֣רְעַל as a Peal infinitive, used in the construct state, within an adverbial phrase ("at the rising of the sun"). Such a construction is possible in Arabic (where one would use the term 'nomen actionis' rather than 'infinitive') so that one can say that he came at sunrise; but this would be quite anomalous in Syriac.
This leaves nine peculiar readings in F, of which eight seem to be mere scribal errors or facile emendations, showing no tendency towards MT:

8:9  

MT  עָבָר הַזּוֹ הַמִּקְוֶה, rel. ...
14:6 F נִסָּ֣יָּה, rell. נִסָּ֣יָּה, MT אַהֲרִיָּ֨ה

18:22 F מִתּוֹרֵ֣כָה, rell. מִתּוֹרֵ֣כָה, MT מִתְּמַּעָּ֨ה

50:7 F מְלַֽכְתּוֹ, rell. מְלַֽכְתּוֹ, MT מְלַֽכְתּוֹ

51:19b: F omits the second half of this verse, perhaps by homoioteleuton between לְחַצּוֹנִּים (end of 19a) and לַחָצְנִים (end of 19b).

91:4 F מִשָּׁלְךָ, rell. מִשָּׁלְךָ, MT מִשָּׁלְךָ

131:2 F מְאֹדֶ֥ה בְּלָֽאֹתָ֣ה, rell. מְאֹדֶ֥ה בְּלָֽאֹתָ֣ה MT מְאֹדֶ֥ה בְּלָֽאֹתָ֣ה

139:15 F מְאֹדֶ֥ה בְּלָֽאֹתָ֣ה, rell. מְאֹדֶ֥ה בְּלָֽאֹתָ֣ה MT מְאֹדֶ֥ה בְּלָֽאֹתָ֣ה

The one remaining place where F stands alone is at

ψ 141:5.

MT

G' ἐλάπον δὲ ἀμφοτέροι μὴ λειχαναὶ τὴν κεφαλὴν μου

F

rell

It has been pointed out more than once that G' read ὑψὸν where MT has ὕψος; and on the basis of the majority reading here, it has been supposed ¹ that ὑψὸν was present in the Hebrew Vorlage of P' as well. Barnes, tacitly accepting that view, suggested that the reading of F is

¹. e.g. by Gunkel, and by H. Bardtke in EHS.
"perhaps due to a taste for emendation on the part of the scribe" (p. xvii).

Now, if the longer reading is original, then the source used by the translator had the verb before the object, whether that source was a Hebrew Vorlage reading יָשֵׁר or a Greek text to which (according to Baethgen and others) the translator referred on occasion. This word order is usual in Syriac; and it is curious, then, that the translator chose to put the verb after the object. Admittedly, this order is sometimes found in P' where it was present in the Hebrew:

132:15 חיָה בְּרֵשֵׁי אָבְּרָהָם נַעֲרָב

137:8 אֲשֶׁר גְּרִידָה יָדָיו נַעֲרָב

1. I have chosen my examples from the vicinity of יָשֵׁר.141:5.
but there are also cases in which the Hebrew had the object before the verb, and the translator moved it, presumably in order to improve the Syriac style:

The opposite change of order, which must be postulated if the longer reading is accepted, seems inexplicable.

However, I believe that a case can be made out in favour of F's text as the original rendering of the translator from a Vorlage resembling the difficult MT (i.e. with \( \psi x \_7 \) not \( \nu y v \)). It was pointed out by Baethgen (p.433) that there was a powerful tendency for the translator to abbreviate his Hebrew text, he lists several examples on pp. 433 ff., and indeed in \( \psi 141:5 \) itself there is nothing to represent either \( \nu y v \) or \( \nu y v \) in any of the Pesh. mss. The reading of F, which carries this abbreviation farther by rendering the two forms \( \psi x \_7 \) and \( \psi x \_7 \) by the single \( \nu y v \), has many parallels, e.g.

\[
\begin{align*}
27:8 & \quad \text{סֶנֵסְכָּהָה רֶשֶּׁׁנֶּךָ רְבֵּּשׁ} \\
102:27 & \quad \text{זַעְלַגָּה רָּתַּלְוָּה רָתַּלְוָּה}
\end{align*}
\]
and Baethgen's list yields many more examples. Nor is the omission of the negative particle *ם* out of character. I have found several passages in the Psalter in which the translator apparently reversed the meaning of the Hebrew by adding or omitting a negative particle. This phenomenon is not mentioned explicitly in the studies of Baethgen and Vogel¹, and so I take this opportunity of listing passages in which I have observed it:

ψ 16:2

ψ 37:33

ψ 56:3

ψ 60:6

ψ 68:19

1. They do refer to the conversion of interrogative sentences into positive or negative statements (Baethgen, p. 428; Vogel, p. 48), and this sometimes results in an opposite sense. In this connection Baethgen notes ψ 88:11 and ψ 89:48
Coming back to \( \psi 141:5 \), we see that the peculiar reading of F could well have been that of the Ur-Peshitta.

1. On this and other renderings of \( \psi 141:5 \), see R. Loewe, "Jerome's treatment of an anthropopathism" in VT (1952), pp. 261-272; especially p. 271.

2. \( \text{דָּלָסָא} \) for \( \text{נָדָה} \) may represent a form \( \text{רָלָה} \) from \( \text{רָלָה} \), cf. \( \text{רָלָה} \) \( \rightarrow \text{דָּלָסָא} \) in 106:20. \text{סָא} here: \( \text{וָ} \) \( \text{סָא} \) \( \text{יָ} \) \( \text{טָ} \) \( \text{עָ} \) \( \text{טָ} \).
If so, the word order in the other mss is readily explained; from G' were inserted the words אַלְמָנָה and בָּשָׂר, while the order of the words in the earlier text was left undisturbed.

This concludes our study of the readings peculiar to Cod. F. There seems to be no evidence of a second translation from the Hebrew, and our choice of hypothesis (b) is thereby justified. There are occasions on which F alone preserves the original text of P', and the foregoing discussion has set out to identify where this has occurred.

So far, then, we may claim that the policy suggested by the map for discriminating between variants does not conflict seriously with the lessons of traditional textual criticism.
THE BEHAVIOUR OF CODEX F OUTSIDE THE PSALTER

§1. Introduction

References:


G.Dietrich, "Ein Apparatus criticus zur Pesitto zum Propheten Jesaia" [= Beihfte zur Zeitschrift für die alttestamentliche Wissenschaft VIII], Giessen 1905.


As we have already remarked, the Psalter is not the only book wherein F departs from all other mss to agree with MT, and it is of obvious importance to ascertain the cause of these agreements in other O.T. books.

It should be stated from the outset that the scope of the brief investigation which follows is in many respects limited. I have not examined the ms at first hand, and therefore rely on the published material listed above, which is all that I find accessible at the moment; it is of course by no means sufficient for the sort of full-scale and definitive treatment which will be possible after the Leyden project is completed. Furthermore, we shall confine our aim to choosing between the rival hypotheses which

1. Of Voöbus in particular, H. Schneider warns that some of his collations "should be treated with caution because they seem to be at variance with the actual readings of the MSS" (Leyden Pesh. Rd.).
might account for F's unique agreements with MT; the far less certain task of explaining how the situations which we shall postulate came about, is reserved for a later stage in our work (pp. 9:54 ff.). Despite these limitations, nevertheless, this study leads to some interesting conclusions.

To judge from this material, one can state that F has been found, in most of the books wherein it has been collated, to exhibit some unique agreements with MT, but that the extent of this phenomenon varies greatly between one book and another. Those books in which such agreements are relatively frequent are:

**Numbers** ("I have observed that F in Numbers differs from all other MSS, and follows the Hebrew on 29 occasions, most of them being important variants" - so Pinkerton, p.16).

**Deuteronomy**, wherein F shows similar tendencies (loc. cit.); some evidence, to be discussed, is available in Vööbus' collations of Ch. 32.

**Isaiah**, where Diettrich (p. xxxi) lists 47 unique agreements between F and MT (out of a total of 228 readings peculiar to F).

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1. Whether a reading is unique to F depends, of course, on the number and character of the other MSS used in the investigation. It should be pointed out that in some books, certain MSS have been found to be apographs of F, namely 17a7.8.9 in Judges, 17a6.11 [-y,0] in Isaiah, 17a6 in Lamentations, and 17a6.8.9 [-l,m,d] in Chronicles. A reading which is attested by one or more apographs in addition to F but by no other MS is treated in our discussion as being unique to F.
Lamentations, for which Albrektson lists (pp. 27 f.) thirty-nine readings unique to F; of these, no less than twenty appear to tend in the direction of MT.

Chronicles ("The text of Cod. F is peculiar. While resembling that of Cod. A in many striking instances, it frequently departs from A (and from all other MSS which I have examined) in other instances equally striking to agree with the Massoretic text" - Barnes, p. xxx).

In other books, however, this tendency is far less marked:

Judges, over which Dirksen states (p. 105) that F (with or without its apographs) stands alone on 142 occasions, on eight of which it agrees with MT.

Ezekiel, where Goshen-Gottstein reports (pp. 48 f.) only 5 unique agreements between F and MT, over 95 places where F stands alone.

The only books over which a detailed collation of F was available to me are Chronicles, Isaiah and Lamentations. We shall therefore discuss each of these in turn, and then (in §5) make whatever observations our material allows regarding other books.

§2. CHRONICLES

As in our treatment of ψ, several rival hypotheses regarding F must be borne in mind. The one which now holds the field is Barnes' theory "that in Chronicles at least its text has been so freely conformed to the..."
Massoretic, that its value as a witness to the text of the Peshitta is seriously lessened" (p. xxx). In the following pages, we shall try to discriminate between this and other possibilities, using the same methods as for \( \Psi \). The extent of variation among the mss is remarkably great in comparison with other O.T. books\(^1\), a fact which tends to complicate our investigation. Nevertheless, we can point to a fair number of crucial passages which take us some way towards our goal.

\[ \text{1.9.1 Here F alone has the form } \gamma\alpha\lambda\omega, \text{ while the other mss have } \chi\omega\nu. \text{ In Nöldeke's Grammar, } \gamma\alpha\lambda\omega \text{ is said to appear "only in very old writings" (p.47); apart from the present instance, the only documents in which I am aware of its occurrence are the Old Syriac Gospels (both Cur. and Sin.). Evidently } \gamma\alpha\lambda\omega \text{ is an ancient form (cf Bibl. Aram. } \gamma\lambda\nu\gamma\nu\text{), obsolete in classical Syriac; and the fact that F is here alone in presenting it demands explanation. } \gamma\alpha\lambda\omega \text{ can scarcely have been deliberately substituted, in this one passage, for } \chi\omega\nu; \text{ it seems rather that the original translation employed } \gamma\alpha\lambda\omega \text{ regularly, and that the text was subsequently brought into line with later usage, with only rare occasions remaining for us to catch a glimpse of that earlier state of the text. The likelihood is thus enhanced that there exist other readings unique to F which are at least as old as their rivals.} \]

\[ \text{1. cf Goshen-Gottstein, p. 35n.} \]
We may mention here the fact that prosthetic Alaph is common in F (Barnes, p. xxix). This is perhaps to be regarded as a further archaic feature, in that it is common in the Old Syriac Gospels¹ and in old mss generally². Goshen-Gottstein however asserts (p. 55) that it is the later Jacobite mss which are especially prone to add a prosthetic Alaph.

1.11.20

MT

הַשַּׁלֹשׁ (Q יִלֵּךְ) רַב

G' καὶ οὗτος ἰδίως δυναστείᾳ ἐν τοῖς πρώτοις

Most P' mss have יִלֵּךְ וַיְרָאָה, except for F יִלֵּךְ וַיִּרְאוּ; none has anything to represent תַּשְּׁלֹשָׁה. Now a rendering יִלֵּךְ יְרָאָה for

 말씀 is not only closer than its rival to MT but also consistent with the free and often expansive mode of translation exhibited throughout Chronicles³. Moreover it is not difficult to explain יִלֵּךְ as either a corruption of or a more explicit substitution for יִלֵּךְ יְרָאָה. Thus the hypothesis that F here is alone in preserving the (unique) original reading is admitted by this evidence.

The theory that revision after MT is responsible seems less satisfactory; from a reviser we would expect יְרָאָה followed not by an adjective but by יִלֵּךְ.⁴

2. Nöeldeke 851. Moreover, Diettrich found that, in Isaiah, prosthetic Alaph was a particular feature of his three oldest mss (p. xxv1).
4. The reading of F cannot be explained by assimilation to the parallel at 2 Sam 23:18, whether we read there יִשְׂרָאֵל יִלֵּךְ וַיִּרְאוּ (edd. Walton, Lec) or - far more probably - omit the phrase altogether (with 614, 714, ed. Mosul).
In the majority of P' mss we find מַלְאָכָּה יֶלֶקֶטָּה יָמָה, in agreement with G' (פֹּזֶרְשָׁה) and with the P' reading of all the mss reported by Barnes at the parallel in 96:9 (said). F however reads ...סָמָה. If this reading is due to revision, then it is surprising that in the preceding verse the expansive rendering of מַלְאָכָּה יֶלֶקֶטָּה יָמָה by מַלְאָכָּה יֶלֶקֶטָּה יָמָה (so all the mss) was allowed to stand in F. More probably F preserves the original reading, while the other mss have assimilated to the familiar parallel in 9.

Most P' mss

F

The majority reading follows MT and there is no reason to suspect it; but to regard F's reading as a corruption thereof seems rather far-fetched. Is it possible that F has a second translation made from a Hebrew text, Hiph. יִתְלוֹנָה having been construed in the sense "offer up", and הַרְבוֹנּוֹ having been mistaken for כַּרְבוֹנָה or the like? For other possible instances of F presenting a different translation, see II.22.3; 23.1.
Most P' mss have ַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַַָ
readings, in fact, must depend on the Kings passage; but how are they related to one another? If we consider in particular the second and third, it does not seem satisfactory to regard either as derived from the other; they seem instead to be two different attempts at defining the connection by marriage which the term יִנָּה suggests. Cod. A makes Ahaziah into Ahab's brother-in-law, while F states that he was married to Ahab's niece. Perhaps the majority reading has arisen from that of A through a misguided "correction" which based itself on the description of Athaliah (who was in fact Omri's grand-daughter) as יִנָּה at II Chron. 22:2, whence it was supposed that she was Ahab's sister. At all events it does seem that, whatever the relationship between the first two readings, they on the one hand and F on the other go back to two quite different treatments of a Hebrew text.

II.23.1 MT יִנָּה G' ... מְשַׁתַּאי (or the like)

Most F' mss לַמְשַׁתַּאי, F לַמְשַׁתַּאי.

The reading of F can scarcely be due to revision after either G' or MT; note that in y.3 the rendering עַכְּבָד יִנָּה מַ for עַכְּבָד עַכְּבָד יִנָּה יִנָּה appears in all the mss, including F.

If, on the other hand, לַמְשַׁתַּאי is original, then it is hard to understand how it came to be changed to לַמְשַׁתַּאי, a name which (in Syriac guise) bears no great resemblance thereto and is not at all common. We have a possible explanation if we refer לַמְשַׁתַּאי to one translation from

1. This is what I understand by the expression מְשַׁתַּאי לַמְשַׁתַּאי.
a Hebrew text like MT, and to a second translation in the execution of which the word, by an easy displacement of the ו, either appeared corruptly in the translator's Hebrew Vorlage, or was misread by him, as שמשיור.

II.26.3 Here MT has רֵעֵי (ך), רֵעֵי (ן), while the P' mss exhibit remarkable divergence. Most read רֵעֵי; A has רֵעֵי, agreeing with the P' text at the parallel in II Kings 15:2 (according to the three mss which Barnes consulted); F, without support, gives רֵעֵי. Now this last reading looks like a rendering which has been affected by the well-known tendency to "Syriacise" Hebrew proper names of the /yqtl/ pattern by substituting Nun for Yodh. Instances occur within Chronicles, such as

I.3.18 MT רֵעֵי P' לְשֵׁנֵא 1
I.23.19 MT רֵעֵי P' לְשֵׁנֵא 1

and elsewhere (e.g. מַשְׁבִּית in Judges x1). Thus the substitution of Nun under these circumstances may be regarded as a characteristic, albeit not unique, of the translator. If לְשֵׁנֵא is due to revision, then we must suppose either that the reviser imitated this characteristic, or that he wrote לְשֵׁנֵא, with initial Yodh, which was later corrupted to Nun. It seems simpler to consider לְשֵׁנֵא to be at least as ancient a reading as its two rivals.

1. This reading has been checked in Lee and in 7a1.
As for the two remaining readings, \( \text{דנ} \) can be attributed to the influence of the parallel in Kings, but \( \text{כד} \) is difficult to explain on any theory. It could scarcely be a corruption of \( \text{כד} \) or \( \text{כד} \); and to assign it to an independent translation, from a Hebrew Vorlage reading \( \text{נהי} \) - a name which occurs four times in I Chron, but not at all in II Chron., and in every case denotes a man, not a woman - involves an equally implausible corruption in the Hebrew tradition from \( \text{ס} \) to \( \text{נהי} \). All that can be said with some confidence is that the reading of \( \text{F} \) appears to go back to an ancient translation from the Hebrew.

Consideration of these passages yields the following results:

1) There are unique agreements between \( \text{F} \) and \( \text{MT} \) which cannot be satisfactorily attributed to revision after \( \text{MT} \) (see I.11.20; 16.30; II 13.17; 23.1; 26.3).

2) There are indications that some of the material which \( \text{F} \) alone attests is ancient (see I.9.1; II 26.3).

3) In some places, the text of \( \text{F} \) can be considered as the original reading in contrast to that of the majority (I.9.1; 11.20; 16.30; II 13.17).

4) There are however a few passages in which \( \text{F} \) on the one hand, and the majority on the other, seem to represent two different renderings of the Hebrew (II 3.14; 22.3; 23.1).

In the main, then, the behaviour of \( \text{F} \) in Chronicles is similar to its behaviour in Psalms: there are passages in which it alone preserves the one original reading. In view of (4), however, it may be suggested that the
translator issued more than one edition of his work, or that he occasionally offered two alternative renderings between which he had not made the final choice; this would explain those few places in which two different translations of the Hebrew seem to be attested.  

§3. Isaiah

As we have already noted, Diettrich held that many of F's unique agreements with MT preserved the original text of the Peshitta. To appreciate his arguments, we must first consider another of the conclusions at which he arrives, namely that his three earliest Jacobite codices (7a1=A, 6h5=D, 9a1=F) emerge as a small group possessing the property that one, two or all three of its members often depart from all other mss to present a reading which there are grounds for regarding as original, in contrast to that of the majority. The grounds which Diettrich offers are:

1) The three mss (which we may term the 'triad') show various orthographic features which are shared by Nestorian mss but not at all by other Western mss. This suggests that the triad embodies "ein altes Erbstück aus der Zeit vor der nestorianischen Sezession" (p. xxv11).

1. Such an explanation has frequently been proposed in relation to classical texts; see Pasquali, Chap. VII (entitled: "Edizioni originali e varianti di autore")
2) Of those readings which are attested within the triad but in no other ms of P', no less than 44 are supported by the citations made in the commentary attributed to Ephraim (cent. iv) on Isaiah. Admittedly, even a text as early as Ephraim must have contained some errors¹, and the editions available to us must be used with caution; nevertheless, Dietrich maintains, these agreements between Ephraim and members of the triad must surely, at least in the majority of the 44 instances, represent the original reading of P'².

Among the many readings which were attested within the triad (by one, two or all three members) but by no other P' manuscript, Dietrich assembled 106 which were closer than their rivals to MT³. In view of the above arguments, he saw no obstacle to the belief that such readings were older than those of the majority. Unique agreement between F and MT was viewed by Dietrich as no more than a particular case hereof, and was accordingly to be explained by supposing that F alone preserved on such occasions the original reading.

¹. On the other hand, Dietrich points to some passages where Ephraim's citation gives the true reading, while all the P' mss have gone astray (p. xxix).

². But, as we shall soon see, the criterion of agreement with our present text of the commentary attributed to Ephraim, may not be an adequate foundation for Dietrich's conclusions.

³. The distribution of these 106 is: 11(A)+7(D)+47(F)+10(AD)+4(AF)+2(DF)+25(ADF).
Yet even in relation to the triad, F occupies a special position. Of the 106 readings just mentioned, nearly half (47) are due to F alone; the corresponding figures for A and D, which are at least two centuries older, are 11 and 8. In order to account for F's disproportionate share, it is hardly satisfactory to state merely that F is just like the other members of the triad, only more so. The suspicion therefore arises that though F may contain up to a dozen unique preservations by virtue of its membership of the triad, the majority of its unique agreements with MT are due to revision. Diettrich attempts (pp. xxxi f.) to quell that suspicion, but there are some who have not been convinced by his arguments¹, which are far from being unassailable:

1) He observes that F has a much greater number (228) of unique readings than either A or D, and that the proportion of unique agreements with MT to unique readings altogether, is approximately the same (20%) for all three mss. However, what matters is the absolute figure, not the proportion; it is all too easy for a scribe to increase the number of unique readings in his ms (by errors, false corrections, and so on) but not the number of agreements with MT - unless he is carrying out a systematic revision.

2) If one were to suspect in F revision after MT, Diettrich urges, one would be inconsistent not to suspect

¹ e.g. Albrektson, p. 28.
A and D equally; this would lead to the implausible proposition, that readings which follow MT and are supported by our oldest F' mss (albeit against the later mss) should regularly be discarded on the grounds that they result from revision after the Hebrew. The flaw in this argument is that the number of unique agreements between F and MT is far in excess of what one might have expected strictly on the basis of F's membership of the triad, so that it is not unreasonable to postulate some additional reason, peculiar to F, for this high total.

3) In nine of the places where F agrees uniquely with MT, it is supported by the citations in Ephraim's commentary. But as Burkitt observes¹, this commentary in its present form has been extracted from a Catena Patrum compiled by one Severus, a monk of Edessa, in 861 A.D., and it is doubtful whether a quotation occurring therein goes back to the text with which Ephraim himself was familiar. It follows that agreement between F and "Ephraim" may be due to work on the F' text which was executed at any time down to cent. ix, and is hardly a certain guarantee of antiquity².

1. "S. Ephraim's Quotations from the Gospel" [= Texts and Studies VII.2 ], Cambridge 1901, pp. 87 ff.
2. In one place (14:10), the reading common to F and "Ephraim" is clearly an error:
   MT יָּֽלִֽעְצָּֽמָּה, F Eph. יָּֽלִֽעְצָּֽמָּה, rel. יָּֽלִֽעְצָּֽמָּה.
I therefore felt that room was left for further treatment of this problem, on the same lines as our earlier discussions. In particular, we — unlike Diettrich — shall examine certain passages in detail, in order to decide whether the evidence points to unique preservation or to revision.

We may begin by observing that some orthographic features which F alone presents may be considered archaic:

1) As in Chronicles, F is sometimes alone in writing prosthetic Alaph before Yodh or Resh (p. xxvi); see however p.7:75.

2) Vocalic Waw is sometimes omitted, as in certain old Syriac inscriptions and documents¹. We find .defineProperty(display:block) for \(\text{יִשׁוֹעַ} (30:28)\), .defineProperty(display:block) for \(\text{יְמוֹן} (45:23)\), and .defineProperty(display:block) for \(\text{יְמֹן} (19:1)\) — the last being reminiscent of the spellings \(\text{אָשֻׁנֶה} \) and \(\text{אָשּׁנָה} \) which appear in Codex Sinaiticus (Lk 12:1, 13:3).

3) At 65:11, where the other mss have .defineProperty(display:block) (MT .defineProperty(display:block) ), F has .defineProperty(display:block). In the plural of nouns derived from a root mediae geminatae (as apparently here), to write all three radicals is regarded as archaic (Beyer, p. 245).

Next, there are several instances of F agreeing uniquely with MT, while there exists in the immediate neighbourhood another passage where F joins the other mss in departing markedly from MT. As we have stated above (p. 7.40), such a phenomenon can hardly be explained on the basis of revision. Ten such passages are listed in Table B.7.6; the last column records the said nearby discrepancy between F and MT, which occurs – unless otherwise stated – in the same verse.

### Table B.7.6

<table>
<thead>
<tr>
<th>Ref</th>
<th>MT</th>
<th>Most Pesh. mss</th>
<th>F</th>
<th>Remarks</th>
<th>Neighbouring discrepancy between F and MT</th>
</tr>
</thead>
<tbody>
<tr>
<td>10.6</td>
<td>מָיָה</td>
<td>לֵילָה</td>
<td>לֵילָה</td>
<td>Could δαlé be due to emendation?</td>
<td>at end of v.5</td>
</tr>
<tr>
<td>19.11</td>
<td>בַּגְדָּד</td>
<td>לְשֹׁנִי</td>
<td>לְשֹׁנִי</td>
<td>Loose rendering of v.10</td>
<td></td>
</tr>
<tr>
<td>37.21</td>
<td>יְנֶה לֵילָה</td>
<td>לֵילָה</td>
<td>לֵילָה</td>
<td>ℓαρ ῥούμαρ</td>
<td>ℓαρ ῥούμαρ</td>
</tr>
<tr>
<td>46.2</td>
<td>מִן</td>
<td>לֵילָה</td>
<td>לֵילָה</td>
<td>Perhaps F has an archaic spelling rather than a different reading</td>
<td>V.1 is very loosely rendered</td>
</tr>
<tr>
<td>47.6</td>
<td>מְצֹל</td>
<td>אֶלְקָל</td>
<td>אֶלְקָל</td>
<td></td>
<td>ℓαρ ῥούμαר(v.9)</td>
</tr>
<tr>
<td>48.15</td>
<td>מְצֹל</td>
<td>אֶלְקָל</td>
<td>אֶלְקָל</td>
<td></td>
<td>ℓαρ ῥούμαר</td>
</tr>
<tr>
<td>49.8</td>
<td>לְבָרִיק</td>
<td>אֶלְקָל</td>
<td>אֶלְקָל</td>
<td>no addition</td>
<td>For the majority reading, of 42 כ</td>
</tr>
<tr>
<td>51:12</td>
<td>מְצֹל</td>
<td>אֶלְקָל</td>
<td>אֶלְקָל</td>
<td>no addition</td>
<td>ℓαρ ῥούμαר</td>
</tr>
<tr>
<td>53.1</td>
<td>אֶלְקָל</td>
<td>אֶלְקָל</td>
<td>אֶלְקָל</td>
<td></td>
<td>ℓαρ ῥούμαר</td>
</tr>
<tr>
<td>65.14</td>
<td>מְצֹל</td>
<td>אֶלְקָל</td>
<td>אֶלְקָל</td>
<td></td>
<td>ℓαρ ῥούμαר(v.2)</td>
</tr>
</tbody>
</table>
We now come to two places where F's unique agreement with MT does not admit the possibility of revision, while the origin of the majority reading can be explained in terms of the theology of the copyists:

25:4 MT יָדֹה הַנָּשָׁן, F יָדֹה הַנָּשָׁן כָּלָּא נַפְּלָה. The root יָדֹה found here in F, is the translator's usual equivalent for יָדוֹת (3:3, 59:18, etc.); moreover, a change from יָדוֹת to יָדֹה could easily have been made by a Christian scribe. Thus unique preservation is admitted by the evidence, and is preferable to revision, which would probably have yielded יָדוֹת טַכְנָבָה or the like.

49:4 MT יָדוֹת לְעֵינֵי הַרְבִּיִּים שֶׁנִּשְׁאֵר. F יָדוֹת לְעֵינֵי הַרְבִּיִּים שֶׁנִּשְׁאֵר. The other mss add יָדוֹת after יָדוֹת.

The initial יָדוֹת rules out the possibility that the absence of יָדוֹת in F is due to the influence of MT or G (כִּי יָדוֹת ... ). It is conceivable that יָדוֹת was added by the translator, and omitted in F by accident; but it is far more likely that the words lacking in F did not form part of the original text of P'. The possibility should perhaps be contemplated that they form a tendentious interpolation made in order to suggest that the Servant, who speaks here in the first person, is distinct from the seed of Jacob.

Finally we come to a variant at 21:2. Here MT offers וַיָּעָל אֶלְם וַיָּעָל מֵדְמַדְּרַד "Go up, Elam! Lay siege, Media!" G' gives a very different sense, which however pre-supposes a similar consonantal text: יָעָל אֶלְם לָשֵׁם מֵדְמַדְּרַד, יָעָל מֵדְמַדְּרַד מֵרְפֵּד, representing יָעָל אֶלְם מֵדְמַדְּרַד. Now most P' mss read וַיָּעָל אֶלְם מֵדְמַדְּרַד "Go up, O Elam and mountains of Media". F has וַיָּעָל אֶלְם מֵדְמַדְּרַד, against all the other mss (apart from its own apographs); the "Ephraim" commentary has וַיָּעָל אֶלְם מֵדְמַדְּרַד, without any Seyāmē.

One might suppose at first that the P' translator too misread the Hebrew, יָעָל אֶלְם being mistaken for יָעָל אֶלְם, and that the forms attested by F and Eph. are each derived from יָעָל "mountain" and therefore constitute different corruptions of an original rendering יָעָל. This involves one quite unexpected misreading and two independent scribal errors - not corrections, because both יָעָל and יָעָל are if anything more awkward than יָעָל. There is, however, an altogether different way of viewing the evidence. Syriac has a verb יָעָל "to fly, move swiftly";¹ it is rather less common than its derivative יָעָל "avis, volucris imprīmis rapax" (P-S), which is used several times to render the Hebrew יָעָל. Arabic has a cognate verb יָעָל (ג) "to fly". If we were to postulate a cognate root in Hebrew, that root would be יָעָל.² It is

¹. sometimes used figuratively, of the eyes or mind.
². Cf יָעָל - (ג)
possible, then, that the translator had a consonantal text "םיראש(1) (cf G' above), and recognised here - rightly or wrongly - a Hebrew fly which, as far as I am aware, has not been identified by modern writers hitherto, either in this passage or elsewhere. On this hypothesis, the translator employed the Syriac cognate and wrote "re' "fly!", which survives in Eph.; F re', with the silent Yodh omitted, is merely a different way of writing the same, not a real variant (cf. re' for re' immediately before); while re' is a very easy corruption due to the far greater familiarity of the noun re'. That "Ephraim" construed his text as we have suggested, may be conjectured on the basis of his comment on re':

that in which he refers to a mountain and seems to take re' to be roughly equivalent to re' 2. It would follow that here again F is closer than any other P' ms to the original rendering.

Whether P' was right to have understood the Hebrew text thus is a separate question, but an interesting one. The text "besiege!" is supported by sa', V', Saad., and all modern authorities known to me; T' ירי, though it may be based on re' -rock, also tends to confirm re'. Yet re' is attested by G' and P',

2. So the Roman edition renders re': "Advola".

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quite independently; and I feel that a case can be made out in its favour. If שָׁלַג be read, then the advancing armies of Elam and Media are here represented in the figure of a predatory bird. It is then desirable to take צָמָה as "rise up in flight", as at Jer 49:22

and to suppose that צָמָה shares with its cognate צָמָה a special association with birds of prey. We may then render the phrase: "Soar, O Elam! Swoop, O Media!", with a sense not unlike that of the Jeremiah passage just cited. As צָמָה does not apparently occur elsewhere, we can readily understand how the reading צָמָה arose.

Such an interpretation will remove two difficulties posed by our present text:

1) If צָמָה is taken in the traditional sense "go up", then, as has often been pointed out, it is a singularly unsuitable word to use of the Persians marching against Babylon; for they would descend from the highlands of Persia to the valley of the Euphrates. Against this argument it has been urged that צָמָה yields a fine assonance with צָמָה, and that being the appropriate word for approach to Jerusalem it was transferred by the prophet to an advance on Babylon. But the fact remains that "topographical distinctions are always carefully observed by the Hebrew writers".

   There is the further point that צָמָה may have been intended to mean "mount an expedition, attack". We note, however, that when צָמָה is so used, the context is made clear by the addition of צָמָה or the like, or by an explicit mention of the enemy after a preposition such as צָמָה (see examples in BDB, p. 748, col. ii, lines 14-20); there seems to be no evidence that צָמָה used absolutely - as here - would have been understood as a technical military term.

a dictum which applies particularly to the distinction between יִשְׂרָאֵל and יִשְׂרָאֵל - so that one is most reluctant to accept a literal interpretation of יִשְׂרָאֵל here. If however we construe יִשְׂרָאֵל in terms of our "bird" metaphor, the problem disappears.

2) There is nothing else in this oracle (21:1-10) to suggest a siege, which is by definition a protracted affair. On the contrary, the simile of the stormwind (V.1) - which can be more naturally taken to refer to the advancing armies (T', Ra, etc.) than to the vision received by the prophet (Gray) - suggests an unimpeded advance, and the banqueting scene of V.5 also indicates that the prophet expected Babylon to be taken unawares and - as in fact happened - without any effective resistance. Thus the reading יִשְׂרָאֵל removes the one indication that the enemies of Babylon did not meet with total and immediate success.

It is noteworthy that the stormwind figure of V.1. now appears to give way to that of a bird of prey in V.2. Such a progression of ideas is not without parallel in the ancient Near East. We may refer to י 18:11b יֹעַר וּלְאָם יָדַע "he swooped on the wings of the wind" (NEB). Of particular relevance is the external form which was attributed to the Sumerian deity Imdugud, who was later venerated as Ninurta/Ningirsu: "Imdugud is the thunder-cloud personified. The mythopoetic imagination saw it as an enormous vulture floating with outstretched wings in the sky"¹. It

¹. T. Jacobsen and S.N. Kramer in Journal of Near Eastern Studies, vol. 12 (1953), p. 167, n.27. A magnificent copper panel of Imdugud (ca 2500 B.C.), wherein the god is represented by an eagle with the head of a lion (to account for the roar of the thunder), is to be seen in the Babylonian Room of the British Museum (exhibit no. 114308). Despite a later tendency to represent the god in human form, the bird form persisted for many centuries and can be detected on Assyrian reliefs; see Jacobsen, "Formative Tendencies in Sumerian Religion" in "The Bible and the Ancient Near East" (ed. G.E. Wright, London 1961), pp 267-278, especially p. 270.
Two serious faults in the above discussion on the text of Is. 21:2, were pointed out at the oral examination by Prof. L. Warszawski; and Mr. Loewe and I have agreed that a note should be added (although Prof. Emerton did not demand it formally) in order to ensure that the reader is not left unaware of these and of their implications.

1) Having added the Syriac ܐܝܗ and the Arabic چا (y), both meaning 'fly', I continued (p. 7.88): "If we were to postulate a cognate root in Hebrew, that root would be יִּעַ. That sentence cannot stand; for the regular sound correspondence is

Arab. چا = Arab. چا = Heb. יִּעַ (not יִּעַ)

so that the expected form of any putative cognate of this verb in Hebrew is יִּעַ, not יִּעַ.

2) In attempting to explain the P' readings (ܗזא most ms, ܗזא P, ܗזא Eph.) where MT has יִּעַ, I neglected the possibility that P' vocalized the text as יִּעַ, and, in accordance with its occasional practice of rendering יִּעַ by שִּׂמְרָא, offered the rendering ܗזא.

This would imply that the majority reading was original, while the texts of P and Eph could easily be explained in terms of scribal error (substitution of singular for plural, and omission of סָיָּמָא, respectively).

I do of course accept Prof. Emerton's criticisms, and acknowledge that they count heavily against both the main hypotheses advanced in the last few pages,

(a) that the original Hebrew text was יִּעַ, being an imperative form of a יִּעַ 'fly';
(b) that the original text of P' was ܗזא, with the same meaning: "And fly!"

Nevertheless, I may be allowed to remark - not, I hope, out of sheer αναγκαιοτητα - that it is still not impossible to defend these conclusions, at least in part. The letters י and מ, which are homorganic, can interchange within Hebrew²: a well-known indication of this fact is the existence of pairs of related roots such as יִּעַ and יִּעַ, or מָעַ and מַעַ. Now in particular, we may suppose that there were cases in which an original י became מ (though no doubt there were also cases in which the commutation worked in the opposite direction). A likely example is furnished by the pair מָעַ/מָעַ when we consider the second radical of cognate roots in other Semitic languages (Arab. מ, Arab. מ, As. מ), we expect to find מ in Hebrew, and indeed מ is the usual and presumably original form - but there nevertheless occurs, at Is. 5:28, a noun מָעַ. It follows that the appearance in Hebrew of מ where the regular sound correspondences point instead to מ, is not in itself fatal to our arguments, indeed, the Hebrew root מָעַ 'cut' (at Es. 40:22*) appears to find cognates in Syriac ܐܝܗ and Arabic چا (not چا), and thus to partake in precisely the same "illicit relationship" as is implied by ܐܝܗ ~ מ ( ג י ו) ~ מָעַ 4.

My present reluctance to abandon the proposed יִּעַ is further sustained by two points (of which I was not aware when the thesis was submitted) tending to favor יִּעַ rather than יִּעַ as the original Hebrew text (though they do not touch the question of what יִּעַ might have meant)

But this is not the place to take the matter any further; and so, for the meantime at any rate, cavet lector.

1. put forward by L. Warszawski, "Die Peschitta zu Jesaja (Kap. 1-39)", Berlin 1897, pp. 189, 207. I note Warszawski compares תִּּיָּגә יִּעַ (so most ms).
2. Both in Isaiah (54:1) and elsewhere (Num. 23:9, Jer. 18:14; Nah. 1:16; Job 18:4). These are, I believe, the only examples, out of 11 occurrences of the noun יִּעַ in Isaiah, and over 70 in the whole of the Tanakh.
4. If, then, the existence of יִּעַ 'fly' were admitted, it would be a separate question as to whether מ, recognized it (and rendered ܐܝܗ) or rendered the text as if it were יִּעַ 'whence ܐܝܗ'. A further point is worth noting with regard to hypothesis (b): even if the existence of the proposed Hebrew root were denied, it would still be possible for the last-ditch defender of the priority of ܐ伝え to argue that P' had learnt that he could often translate a Hebrew word into Syriac by substituting מ for מ (e.g. at Is. 16:3, where he renders מָעַ by מ in לְמַעַ and מָעַ by מ in מָעַ), and here look that course comes to an end. In the passage under discussion, such a possibility is more likely to entertain the reader than to be entertained by him, but it is perhaps applicable at Jer. 2:20, where MT has מָעַ מ, but P' ܐ伝え מ

5. Those points are:

(1) It is almost certain that יִּעַ is the reading of the Quranic scroll Q115a; this was first pointed out by J. Barthélemy, in Revue Biblique (1960) p. 540, and now seems to be generally accepted (as in the apparatus added by Lasselfeld to T4). The reading יִּעַ leads to a felicitous assimilation between the words which in v.2 refer to the onrush of the invader (יִּעַ יִּעַ מָעַ מ) and those which portray in v.3 the terror which thereupon overwhelms the poet (יִּעַ מָעַ מָעַ מָעַ מ); the terms denoting his reaction are made to seem like "intensive" forms (the first reduplicated, the second plural) of the terms denoting the attack. (It is of course assumed here that the similarity in pronunciation between the two gutturals מ and מ was so noticeable as to warrant being exploited by the poet in this way.)
is however uncertain whether the succession of metaphors in the Isaiah passage involves a mythological element, rather than a long-established association of ideas. A similar development seems to have befallen the word פֹּן. This word is found extensively — in slightly varying forms — within the literature of Near Eastern peoples as the name of a weather- or storm-god\(^1\). In the O.T. it appears seven times, apparently meaning "flame, fire-bolt"; yet in each of these passages it is rendered by at least one of the ancient versions in the sense of "bird(s)"\(^2\) with an occasional suggestion that birds of prey are meant\(^3\).

In sum, the hypothesis that revision is the cause of the unique agreements of F with MT, is no more acceptable in Isaiah than in Psalms or Chronicles. As for the possibility which suggested itself in §2, that F sometimes

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1. in Assyrian, Ugaritic, Old Aramaic (Zenjirli), Phoenician (especially Cyprus), and Egyptian.

2. viz. Dt 32:24 (G‘T‘V‘a‘, and probably P‘); Hab 3:5 (P‘a‘q‘t‘, and also E‘); ψ 76:14 (σ‘); ψ 78:48 (α‘σ‘); Job 5:7 (G‘F‘V‘a‘σ‘); Ct 8:6 (G‘ απεικόνισα); and Ecclesiasticus 43:17 (G‘). Some hold that in the last passage the Hebrew itself means "birds"; note that it stands in parallel with נֵבֶר. Compare further the comment of the Midrash (Exod. R. 12.6):

מֶה צֵּאָכַה לְאָרְשָׁי לְהָלִים וְעָשָׂה מְכֵה: מִתָא אֵלֶּה הָעֲרוֹפָה כְּדָא (אֲרָבָה תֹּא) בְּנֵבֶר רֵשֵׁה גַּבֵּרָה עָרָה

preserves a second translation of the Hebrew, I can find no evidence in its favour; occasionally, it is true, F has a different word (lexical item) from all the other mss, but my impression is that we are dealing with a substitution in F rather than a different translation (an interesting example being at 60:16, MT יָפָר, most mss יָפָר, F יָפָר 1). Our own conclusions therefore agree with, and (I hope) render more firm, Diettrich's claim that F is on occasion the only ms to preserve the true reading of the Peshitta.

§4. Lamentations

Over the short book of Lamentations, F exhibits no less than 39 unique readings, of which 20 tend in the direction of MT. Albrektson considers the rival explanations of revision and unique preservation, and he opts for the former. He is by no means convinced, however, that the source employed for the revision was the Hebrew itself:

1. יָפָר is used to render סִינֶך in the N.T. (Luke 1:47 Sin. Pesh. etc); see Burkitt, "Early Christianity outside the Roman Empire", Cambridge 1899, p.22.

2. These are listed in Albrektson, pp. 27 f. Many of them are indeed unique to F in relation to the mss which Albrektson cites in the body of his edition, but reappear in Par. Syr. 8 (=17a6), which, as Albrektson states, is probably derived from F. Some of the readings have found their way into the polyglots and Lee, for Sionita apparently drew from 17a6 his text of Lam 2-4. We shall therefore treat 17a6 as an apograph of F for the purposes of Thes., p. 7:72, n. 1.
"The possibility that it may in fact be a revision according to LXX should perhaps be considered. At least in the Book of Lamentations the peculiar readings of F may equally well be explained in this way - there are as many agreements with LXX as with MT" (p. 28).

Now there are 17 readings ¹ unique to F which stand closer than their rivals to both MT and G', and could therefore be explained by Albrektson's hypothesis - though other explanations are also admitted. We may also point, in support of his view, to one passage wherein F agrees with G' while it is the majority who adhere to MT, namely:

1:3) MT יִֽצְרֵכָה, G' אוּדָּלַתָּוָּו אָבְּרֵה Most mss סָמַע אֱלֹהִים, F מִצְרוּאֵה נ How ever, this one passage is outweighed by the following three, in which F agrees with MT against G'.

1:18) MT נַּעַרְתֶּשׁ, G' אָדָם שֵׁם δῆ Most F' mss לוֹ, F וְלוֹ This passage will be discussed below; for the moment we need only say that פ is more likely to go back directly to the Hebrew נ than to Greek δῆ .

¹. at 1:7,17; 2:1,5,6,7,7 (but not דנא), 8; 3:2,4,14,16,18; 4:3,8,10; 5:9.
2:7) MT כינום מֶנָּח, G' וְנַעֲמֶרָה וֹאָרְתוֹכֶה. Most P' mss have אָבָב אֶלֶּכֶת חַָּנָךְ (cf G), but F אֶלֶּכֶת אֵתֶנִּים (cf MT).

4:12) MT וַיִּתְנַהֲרֵנִי וְלַמְלֹךְ כִּי וַיִּתְנַהֲרֵנִי. Most P' mss agree with G' in reading שְׁלֹחַנֵי שְׁלֹחַנֵי, but F has שְׁלֹחָנָנוּ.

These three agreements between F and MT against G', cannot all be attributed to coincidence, whereas F's one agreement with G' against MT can. It therefore seems that revision after G' does not account fully for F's behaviour, and that we must turn once more to the rival hypotheses of revision after MT and unique preservation. Two of the three passages just mentioned take us some way towards deciding which hypothesis is appropriate.

1:18 F is the only ms to present after נָחָל the Syriac particle יַעֲמֹר (sometimes spelt יַעֲמֶר), cognate with Heb. יַעְמֵר. There are two reasons not to attribute its presence in F to revision: first, in V. 19 the rendering יָעִין has not been accommodated to MT יָעִין (G' וְנַעֲמֶרָה תּוֹלֵע); second, there are grounds for the belief that יַעֲמֹר became extremely rare, if not obsolete, some time about cent. IV, so that it is unlikely that the supposed reviser whose work is represented in F should have inserted it here.
The evidence on this last point may be summarised thus.

In the Peshitta, נ is occasionally rendered ר (e.g. Gen 21:21) but far more often simply omitted. Within the Psalter it does not appear at all, despite the frequency of נ in the Hebrew. Aphraates is apparently another who made no use of יָוֵש (though some of his citations of biblical passages contain י). However, a work attributed to Ephraim (the Commentary on Kings) offers at least one example (טוש "look, I pray" in Ed. Rom. vv 491 F). Over the subsequent centuries, P-S gives no occurrence of י until we come to Barhebraeus. Of particular importance is a discussion on the word Hosanna, written in 701 by Jacob of Edessa, to whose breadth of scholarship we have already referred (p. 7:39). In this lengthy (ca. 450 words) note, he shows an acquaintance with the Hebrew נ, but makes no mention of its Syriac cognate; he could scarcely have omitted to refer to the Syriac י if it had

1. according to Techin.
2. See the lexicon at the end of Parisot's edition. I am sorry to report, however, that this lexicon is not as exhaustive as it is claimed to be, so that an argumentum ex silentio is not completely safe.
3. e.g. Wright, p. 73 (= Mal. 1:8)
5. Jacob identifies Hosanna with נ יָוֵשָׁת פ' 118:25 (G' מָשַׁה, נ' מָשַׁה). He asserts that P' provides the correct translation, and that נ יָוֵשָׁת should be construed as one word, not two, נ יָוֵשָׁת being a suffix which means "he" ('). The rendering of G' (ג' מָשַׁה), he states, presupposes erroneously that נ יָוֵשָׁת is a separate word. (Hence we may infer that he was aware of the usual meaning of נ in Hebrew.) His argument runs:

ן יָוֵשָׁת ג' מָשַׁה הַיְּהֹוָה מַעְלֵה מִזְמָרוֹ, כִּי יִזְמָנֵה מְלֹא הַיְּהֹוָה מַעְלֵה הָעִיר, הַיְּהֹוָה אֲלֵהוּ יָשָׁע.
enjoyed general currency in his day. The native lexicographers (Bar-Al, Bar-Bahlul, etc.; see P-S) explain as equivalent to יד; this is presumably a mere guess\(^1\), and the fact that the original meaning was forgotten reinforces the impression that יד had long fallen out of general usage.

Against this view it may be urged that יד is used — in much the same way as in P' — by Barhebraeus\(^2\), and that in Modern Syriac this same particle can be attached to a far greater variety of verbal forms (e.g. ידבכ "he finishes")\(^3\). These facts, however, do not prove uninterrupted usage of יד from the time of the Peshitta translation until the present day; it is far more likely that יד in the later period is what J. Barr\(^4\) — who had in mind the Hebrew Bible, not the Syriac — termed a 'restoration', i.e. that the word was taken up after centuries of desuetude, and renewed in later literature, on the basis of whatever meaning it was believed to possess. Later Hebrew provides two instructive analogies. The expressions יד תּוּכִּים and ידָתָה חֵרוּתִין each occur only once in the O.T. (1 Sam 19:20; י113:9), and cannot

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1. Whether it is at all connected with Jacob's exegesis above may be doubted.

2. Carmina, ed. A. Scebab (Rome 1877), p. 46 1.11 (יְדָ תּוּכִּים), p. 158 1.8 (יְדָ תּוּכִּים)


be traced thereafter until the tenth or eleventh century. 
Since then, however, both have entered into general usage, and 
today they bear the meanings "company, assembly" and "mistress 
of the household" respectively. The later usage of הכהה is probably close to the earlier; that of הקדשה is certainly not; but we would not be justified in asserting 
that either expression has remained in general usage continuously 
from the time of the O.T. onward.

We may therefore suppose that נא formed part of 
the original P' text, but was omitted in the majority of 
mss because it was no longer familiar to the scribes.

4:12 In order to decide between the two rival readings 
of P', let us consider how the words רע and ארי are 
translated elsewhere in Lam:

- רע is rendered by הָעַל 7 x (1:5,5,7,10,17; 2:17)
  and by הָעַל 1 x (2:4)

- ארי is rendered by הָעַל 1 x (2:4)
  and by הָעַל 13 x (1:2,5,9,16,21;
   2:3,5,7,16,17,22;
   3:46,52)

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1. In writings earlier than that date, we do find occasional 
citations of and exegetical comments on these two passages, 
but no independent usage of either expression.
2. According to the great Thesaurus of Ben-Yehuda, the earliest 
post-biblical occurrences are in the writings of Saadiah 
(cent. x) and Rashi (cent. xi) respectively.
3. This explanation first appears, I believe, in the Midrash 
(Num. R. 14.20):
It is clear that the usual policy of the translator was to distinguish these two Hebrew words; the rendering \( \text{שָׁפַר} \) for \( \text{שָׁפָר} \) underlines the derivation from \( \text{שָׁפָר} \) 'oppress'. Although 2:4 shows that the rule is not invariable, the balance of probabilities is nevertheless decidedly in favour of the originality of \( \text{שָׁפַר} \). The majority reading can then be explained by the influence of \( \text{G}' \); note that \( \text{שָׁפַר} \) \( \text{שָׁפָר} \) is actually the reading of Syrohex.

We may conclude that the limited evidence afforded by the Book of Lamentations can best be accounted for by the hypothesis that here too the original reading of \( \text{P}' \) is on occasion preserved in \( \text{F} \) alone.

§5. Other Books of the O.T.

The first biblical book over which information on \( \text{F} \)'s text is available to me is Numbers\(^1\); Pinkerton states (p.16) that \( \text{F} \) presents 29 unique agreements with \( \text{MT} \), but he does not give collations. For Deuteronomy, in which \( \text{F} \) is said (loc. cit.) to behave similarly, we have Vööbus' collations of Ch. 32 (see pp. 80 ff.), which are based on six mss\(^2\). There\(^3\) \( \text{F} \) presents nine

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1. Admittedly, \( \text{F} \) contains the text of Ex 15:1-21 as a Canticle, placed after the Psalms; we shall refer to this text below.
2. namely 5b1, 6b1, 6h6, 7a1, 8a1, 9a1.
3. Deut 32 also appears in \( \text{F} \) among the Canticles, but with a rather different text, as we shall see.
unique readings, of which three tend towards MT:

7) MT יבג נ, F יבג נ, rel. יבג נ

27) MT כל-ל, F כל-ל, rel. יא (G' רוני פָּנוּ)

30) MT נב ידוה, F נב ידוה, rel. יא ג'77)

G' פָּנוּ שָׂדוּת

These three agreements between MT and F alone, are consistent with the hypothesis of unique preservation, though they could theoretically be equally well explained otherwise, viz by coincidence (in the first two cases) or by accommodation to MT¹. Thus we may recognise the possibility that, in the third passage at least, the reading of F is original.

This third passage raises some points of linguistic interest.

For verses 29-30, MT has

לא תמכ ישבויה זא תביכי לָטיאלָט: יنبيיה הפרח פָּנָה גָּבְרוֹת קָנָה חֲלָץ.

According to the usual interpretation, V.30a refers to an ignominious defeat inflicted upon the Israelites. Now the apparatus by Vööbus gives the text of F as follows:

According to the usual interpretation, V.30a refers to an ignominious defeat inflicted upon the Israelites. Now the apparatus by Vööbus gives the text of F as follows:

1. but hardly to G'.
while the majority differ from F solely in not having וָּאֵל. If the text of F is original, it appears that P' took וָּאֵל to express a condition (as did Targ. Onk). But construed the whole of V.29 as the protasis, with V.30a as apodosis. We may then translate: "If they had been wise..., how could one have pursued a thousand (Israelites)? .... Yet their mighty one delivered them up ...." - a rendering which, albeit awkward, can be related to MT. One can also understand how a copyist could have been puzzled by an apodosis in the form of a question, and could therefore have omitted וָּאֵל, to obtain the rather different sense offered by the majority of P' ms: "If they had been wise..., then one (Israelite) would have pursued a thousand.... Yet their mighty one delivered them up" - wherein V.30a now refers to a defeat which the Israelites might have inflicted on their enemies.

If the priority of F's text be granted, then one may tentatively consider the possibility that the original meaning of P' did not cut across the traditional verse-division. V.29 looks, as we have said, like one long protasis; but Burkitt found that in the Old Syriac Gospels, Waw was sometimes used to introduce the apodosis of a conditional sentence. Burkitt was able to prove that this thoroughly Semitic idiom "was really used in the earlier stages of Syriac literature" (p.74),

1. G' understands וָּאֵל; V' renders /utimam/, so AV RV NEB.
even though we do not find it in the ordinary Edessene Syriac, 
as known to us in writings which date from cent. Iv onward. 
Accordingly it is possible to take V.29 as a complete conditional 
sentence, with uncompounded perfect in both protasis and apodosis; 
vv. 29 f. according to F can then be translated more smoothly, and 
in general agreement with many other authorities (Targ. Onk., Rashî, 
Ibn Ezra, RSV): "If they were wise, they could understand this... 
How could one have pursued a thousand (Israelites)....unless their 
mighty one had delivered them up? ...." When this idiom fell out 
of use, however, the text of F would have been understood in the 
manner in which we first translated it, whence the omission of 
in the majority could be explained as before.

Thus the presence or absence of Waw, in which lie a high 
proportion of the variations among P' mss, may on occasion deserve 
closer attention than has been considered necessary hitherto.

In the book of Judges, Dirksen notes (p. 105 f.) that 
F has 142 unique readings, of which 8 tend towards MT. He 
does not appear to regard any of these readings as original. 
The only passage on which I think it worth while to comment 
at this stage is at 1:10. There MT has שׁוֹנֵאָנָה יַעֲשַּׂנָה יָשׁוּפְּנָה 
in most P' mss, these names are followed by the phrase 
"(cf G' γεννήματα τοῦ 'Εβαστ ) which in F 
however is lacking. Dirksen's proposed explanation is:

1. Cf Judges 8:19 יָשׁוּפְּנָה יַעֲשַּׂנָה יַעֲשַּׂנָה
"The Greek and the Syriac (of all other mss) might well reflect the original text (cf. vs. 20¹), in which case the two words were later accidentally omitted in the Hebrew and in 9a1 fam [i.e. F and its apographs]." In view, however, of our findings elsewhere in the O.T., and of the lack of any trace of the extra phrase among the mss of Targ. and Vulg., we may suggest that דַּעַת וּבָלי did not appear in the original text of P', which is here presented by F alone, and that the two words came into the other mss from V.20, or from Num 13:22, where the same names occur in MT and are followed by נֶאֶה נֶאֶה (P' נֶאֶה נֶאֶה). Although the critical edition of Judges is not yet published, it is already clear that there are much fewer unique agreements between F and MT in Judges than in the other books so far discussed.

The situation in Ezekiel seems to be similar, though here again no list of F's unique readings has yet appeared. The collations of Goshen-Gottstein are based on ten mss² (two of them massoretic). In this company, F was found (pp. 48 f.) to stand alone in 95 places. In thirty of these we are told that the reading unique to F is "possible per se". Only in five places is F closer than the other mss to MT, and Goshen-Gottstein sees no cause to invoke either revision or unique preservation in order to explain

1. where the three are called
²

(P' כֶּנֶת נֵנֶק

2. namely 6h15, 7a1, 7h2, 8h2, 9a1, 9m1, 10m1, 12a1, 17a3(?), 17a4.
them: "None of these cases is of so striking a character as to support the contention that F is especially close to MT....Only the fact that other early manuscripts were not examined sufficiently could create this impression" (n. 108).

Of the Canticles which have been placed in F after the Psalter, Vööbus has provided collations of Ex 15:1-21 and Deut 32:1-43. F has three unique readings in the former (VV. 7, 21, 21) and five in the latter (VV. 7, 25, 31, 36, 43), all of which tend away from MT. Thus F seems to behave quite differently here. Moreover, on the basis of Vööbus' collations it appears that this text of the Deut. passage departs on no less than 40 occasions from that which F presents in Deut. itself. One may deduce that these Canticles go back to a manuscript source other than that which supplied these same passages in the body of the text, and that the text of F here is of quite a different character.

Here we may also mention Ezra, which is lacking in the first hand of F but has been supplied by a scribe of cent xvi. C. Moss has studied the text of this later hand, and found no place where it agrees uniquely with MT.

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1. This last sentence gravely underestimates the extent of F's tendency to agree uniquely with MT elsewhere in the O.T., and is hardly a fair comment on the painstaking work of Barnes and Dietrich.

2. Over fifty mss have been collated.

Before concluding this investigation it seems convenient to assemble whatever instances have so far been detected of a ms other than F departing frequently from its fellows to agree with MT. Our starting-point must be the well-known study of C.H. Cornill on the text of Ezekiel, wherein 7al was collated against printed editions alone, and found to exhibit many unique agreements with MT. Cornill's verdict was that 7al "nach dem massorethischen Texte, wenn auch nicht gerade systematisch überarbeitet, so doch in ausgedehnter Weise corrigiert und geändert ist, wodurch sein Werth für die Herstellung des ursprünglichen Textes von [P'] und damit zugleich für die alttestamentliche Textkritik so ziemlich auf Null reduziert wird" (p. 145) — a view which he changed when the textual evidence was more fully examined and it was shewn that most of these so-called unique agreements in fact enjoyed solid support from other witnesses to the text.

Real enough, however, is the isolation of B.M. Add. 14425 (5bl, D), of which Barnes stated, with regard to its text in Genesis and Exodus: "(1) that it differs from that of all other mss, (2) that in these differences it agrees with the Massoretic text". To Barnes himself it appeared that the text which D represents "has


possibly been accommodated to the Massoretic text"; he therefore based his edition on other mss. Pinkerton, however, argued (pp. 34 ff.) that many of D's readings, albeit unique, were older than their rivals. In the Book of Ezra, Moss¹ found that B.M. Or. 8732 (8h5, C) often departed from all the other mss which he had collated, to agree with MT; his conclusion was that, on many occasions, this ms "alone has kept the original reading of the Peshitta" (p. 81). Finally, Barnes' study² of 2 Kings 14-25 shewed that the text of the Polyglots and Lee (the 'Receptus', as he terms it) differed from that of the other mss collated³ "by a closer approximation to the Hebrew Massoretic text" (p. 534). He concluded: "The general impression which the character of the Receptus makes upon the student is that of a Revised Version, i.e. of a version revised to bring it into closer agreement with the Original". Since all the representatives of the Receptus go back to the Paris Polyglot, it may be supposed that these agreements with MT are due to revision at the hands of Gabriel Sionita; but there exists another possibility. Barnes invites us to consider whether the primary source of the Polyglot text of 2 Kings may have been Par. Syr. 7 (= 17a6), which contains a note asserting that it was used for the Paris Polyglot. This possibility was denied by Emerton⁴ in 1959, but deserves to be re-considered in view of the

¹. op. cit., pp. 76-87. He collated 7al, 8h5, 16/9al, 17al, 17el, and - partially and with great difficulty - 12al.
³. viz 7al, 9ml, 10ml, 11cl, 12al, 14cl, 17al.
⁴. "The Peshitta of the Wisdom of Solomon" (=Studia Post-Biblica II), Leyden 1959, p.xxiv. His grounds - of which Barnes himself was aware - are that the greater part of the text of 2 Kings in 17a6 is unpointed, and that the ms is free of printer's marks.
cogency of Albrektson's arguments (pp. 2 ff.) that it was this
ms which supplied the Polyglot text of Lam 2-4. Now 17a6 has
been found in other books (Isaiah, Lamentations, Chronicles)
to be derived from F; and if the Polyglot text was indeed drawn
from 17a6, then the tendency of the Receptus to join MT may lead
back to yet another instance of the phenomenon of unique
agreement between MT and Cod. F - which raises the ironic
possibility that some readings presented by our printed editions
are older than their rivals, in spite of the fact that the latter
are supported by all the mss which Barnes collated.

In conclusion, we may warn against two possible
misconceptions. Our attention to the superiority of many
of F's readings is not meant to imply that F is consistently
the closest to the original; the places in which F alone
preserves the truth are far outnumbered by those in which it
has gone astray while other mss present the original text.
Nor is it suggested that unique preservation is a monopoly
of F; the same phenomenon occurs in other mss, not only
where F is lacking - as we saw in the previous paragraph -
but also where F is extant (e.g. in Judges¹ and Isaiah²).
What is striking, however, is that F exhibits this

1. Dirksen (p. 105) records unique agreements with MT
on the part of 6h7 (four times) and 7a1 (six times).
Two of the agreements between 7a1 and MT are said
to be "noteworthy", and Dirksen wonders "whether in
these places 7a1 has been incidentally adapted to
the Hebrew".

2. In 6h5 (D) and 7a1 (A).
characteristic more than any other ms over so many of the Books of the O.T., and in the company of mss which are up to four centuries more ancient. Enough, I trust, has been said to shew that instances of unique preservation do abound in F, and that no editor of P' would be wise to neglect a reading peculiar to F on the sole pretext that it is not attested elsewhere.
8. THE INDIRECT TRADITION

To complement the picture given by the mss themselves, we must now turn to quotations from the Peshitta Psalter in ecclesiastical writers. In any ms tradition the indirect evidence is important, but here it is particularly so. This importance lies in two central questions:

1) Do these quotations preserve P' readings which cannot be traced in our mss?

2) When there are two or more rival readings, one of which is supported by an ecclesiastical author, what are we to infer?

To begin with (1): In the opinion of C. Peters¹ (p. 283), our mss offer a text which has been extensively worked over since the translation was first made, and the indirect tradition may enable us to recover an earlier stage of the text. "Um möglichwerweise eines älteren Textes, als die handschriftliche Üeberlieferung ihn bietet, habhaft zu werden, wird man die Psalmenzitate der ältesten syrischen Original- und Übersetzungsliteratur zu Rate zu ziehen haben." This must obviously be followed up.

But the question takes on particular importance in the light of what may be termed the "Kahle view" (Thes., p. 7:11)

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of the origin of Bible versions. According to Kahle, the need for a translation of a biblical book could produce initially a number of different attempts, out of which an "authorised" text was eventually formulated. A similar opinion is that of K. Beyer¹ (pp. 252 ff.): As Hebrew came to be replaced by Aramaic (from the sixth cent. B.C. onwards), the need arose for Aramaic versions of the Old Testament. Little is known of the early history of the translations prepared over the Aramaic speech-area to meet this need, but we can observe them in certain later forms into which they developed – Babylonian Targums, Peshitta, Palestinian Targums. All these versions have been coloured by the theology of those who came to use them, and have been adapted to different dialects of later Aramaic; but they are nevertheless based on older Jewish Aramaic versions, and "durch viele inhaltliche Uebereinstimmungen miteinander verbunden." According to these views, much of the variation among our authorities for the text may be due to their going back to several different translations from the Hebrew, and in such a case it would be nonsensical to speak of an 'Urtext' from which all the variant readings are derived. Much of Kahle's evidence (in his study of G') was gathered from the text of quotations, and thus we have an additional reason for studying the indirect tradition of P'. It is naturally of the greatest importance to the textual critic that he should know whether his aim is to reconstruct the text of a single translation, or whether

he is to recover what he can of a multitude of earlier versions.

Before proceeding to our second question, I would add some remarks on method; these points are obvious, but they need to be stated, because some work in this field has tended to neglect them. If we find a citation whose wording differs from that of our mss, it does not immediately follow that the author in question had a different P' text. He may have recollected the text imperfectly; he may have adapted it deliberately, to suit the context or (if he wrote in verse) the metre; we must also reckon with the possibility of corruption in the text of the patristic work itself. Again, a writer may have had Jewish contacts who acquainted him with the translations of certain passages of Scripture with which they were familiar; and he may have read - and even had in his library - many of the writings produced by the Western church, which contained scriptural quotations whose wording differed from that of P'. Nor have we exhausted the possibilities even yet. Thus some supplementary indication is required to make it appear likely that a divergence in the author's ψ text is responsible. Nevertheless, it is a valuable result if we can shew, with some confidence, what were the readings to be found in the P' text of an ecclesiastical figure. It is valuable, whether a reading found in all our mss is thereby confirmed, whether one out of a number of rival readings is supported, or whether a variant not attested at all among the mss is thus identified - not least because some of the
works embodying ψ quotations are represented by mss as early as, or earlier than, our oldest Psalter (C—probably cent. vi).

How, then, do we regard a reading attested by an ecclesiastical writer at a point where the mss diverge? This will depend on the date of the work in which the quotation appears. An author who wrote before say 425 A.D., i.e. before the Syriac-speaking Church was torn by schism, has preserved an ancient reading. Indeed, if we are to speak of a unique Urtext, then the relative antiquity of its attestation will tend to indicate that that reading, rather than any of its rivals, is the original one. However, the case is by no means proved thereby; for different forms of text may—indeed must—have begun to evolve before the schism, and even a cent. iv author may already be found to incline to a particular form of text. Nevertheless, a reading that is attested by Aphraates or Ephraim enjoys thereby a considerable advantage.

Later authors are of interest for a different reason: if we find an association between an ecclesiastical figure and a certain group of mss, this provides us with a point of reference when we come to unravel the history of the text. This association can be put on a relatively firm basis if we have enough variant passages to locate the author on the map using a "fragment" technique. This has been possible for Aphraates (cent. iv), on the basis of 15 passages; Philoxenus of Mabbog (cent. v-vi, 17 passages); Daniel of Salak (cent. vi, 16 passages); and an anonymous Jacobite introduction to the Psalter¹ (cent. x-xii, 34 passages). We have moreover

located Barhebraeus already (cent. xiii, 266 passages).

It goes without saying that the more passages are available to establish a location, the more confidence it will command.

Much relevant material has already been gathered by Barnes and Peters. In general, Barnes was principally concerned with the second of the two aspects, whereas Peters confined himself to the first.

Following Peters, I shall deal with the sources in the following order: (i) native Syriac literature; (ii) translations into Syriac from Greek; (iii) translations into Arabic from Syriac. Clearly, anything like an exhaustive study of even one of these branches of literature was out of the question. One cannot in all humanity expect one man to search our entire store of Syriac (and Syriac-based) literature. Thus for the most part I have contented myself with following up the investigation in the directions pointed out by my two predecessors, though I have wandered a little farther afield. I have devoted particular attention to those authors who antedate the fifth century (Aphraates, Ephraim). The witnesses are treated in chronological order within each of the three branches.

Our earliest native Syriac authority is Aphraates (flor. 337-345). His homilies were edited by W. Wright and are also available in the edition of J. Parisot. An index of

scriptural quotations is provided in both editions; Parisot's is the more extensive, with reminiscences as well as quotations. The text attested for Aphrates agrees virtually throughout with that printed by Barnes. I list below the more substantial variants\(^1\), with comments where necessary:

\[\psi 14:2\] \(\text{om. Aph.}\) [This is incorrectly indexed as \(\psi 53:3\)].

\[\psi 19:5\]

\[\psi 22:21\] \(\text{P', Aph}\) 342

\[\psi 22:23\] \(\text{P', Aph}\) 342.

Barnes explains (p. xxiv) that this verse appears in Heb. 2:12, where \(\text{P'}\) too has \(\text{P'}\) as well.

\[\psi 37:35\] This is a remarkable case, because in every other \(\psi\) passage Aph agrees closely with the text of the \(\text{P'}\) mss, with only such minor variations as are found elsewhere in this list; here, however, he departs from \(\text{P'}\) throughout the verse to agree with \(G'\):

\[\text{1. I have not however counted variants in only one of Wright's authorities for Aph, if the other agrees with \(\text{P'}\) according to the mss.}\]
The case has given rise to much discussion. The first view we may mention is that נַעֲרָה נַעֲרָה נַעֲרָה is the true text of P', from a Vorlage reading נִנְנָה נִנְנָה נִנְנָה. The difficulty then is that we can hardly explain how נַעֲרָה נַעֲרָה נַעֲרָה arose, or why Aph and the mss diverge in the earlier part of the verse as well. Neither Baethgen's tentative suggestion (p. 445) that the change from the supposed true text of Aph to that of the P' mss arose "aus einer, wenn auch nur vereinzelten Korrektur nach dem hebräischen Original," nor F.X. Wutz's theory that נַעֲרָה נַעֲרָה נַעֲרָה is an inner-Syriac corruption from נַעֲרָה, offers a satisfactory explanation. A further argument against the view that Aph here preserves the true text of P' is that נַעֲרָה נַעֲרָה נַעֲרָה is not found in the Peshitta Psalter, whereas all the words in the text of the P' mss are relatively common there.

We must conclude that Aph has been influenced by the Septuagint, but this too demands an explanation. None of his other ψ quotations betray G' influence, except for a

2. So Barnes (p. xxiv) and Vogel (p. 360).
few instances in which a verse is quoted in the New Testament; and that is not the case here. We may compare the investigation of Baumann (1898, pp. 331 ff.), who examined Aph's quotations from Job; he found that although Aph showed a few striking coincidences with G', a careful appraisal of the evidence suggested that both had taken material independently from the store of Jewish tradition, and not that Aph was directly influenced by G'. Why then did Aph quote just this verse according to G'? The best explanation seems to be that of Burkitt, who observed that the series of quotations adduced by Aph in that context (Jer 9:23, 1 Cor 1:31, Ps 37:35) are also to be found in Clement's Epistle to the Corinthians (xii), which was composed ca. 96 A.D. and of course quotes the Bible from G'. It is reasonable to suppose, therefore, that Aph was influenced by some Greek work - though we cannot identify it with certainty - in which Ps 37:35 appeared in its G' form.

ψ 41:4] Ἀπόκρισις τῆς π', ἡ δὲ τῆς Ἀπίας Ἀφ 79
ψ 50:14] Ἀπόκρισις τῆς π', ἡ δὲ τῆς Ἀπίας Ἀφ 76
ψ 79:1] Ἄπαστροτική π', ἡ δὲ τῆς Ἀπίας Ἀφ 95
ψ 82:6] Στιχοὶ τῆς π', ἡ δὲ τῆς Ἀπίας Ἀφ 334 π' John 10:1
ψ 90:4] Ἰερώνυμος τῆς π', ἡ δὲ τῆς Ἀπίας Ἀφ 36

Here Aph shows an interesting divergence from P':

\[\psi 119:99\]  

The verb here is in MT 'הוֹדָלְךָ, G' טְהַרָה, T' וָהַרְוָה, H' eruditus sum. Aph stands closer than does P' to MT and the other versions.

We first note that Aph's reading is the more suitable in the context in which he quotes the verse, viz the encouragement of disciples:

But where did Aph get this rendering from? We may entertain two possibilities:

(a) Aph gives the original text of P', and the ms text arose through assimilation to a neighbouring passage; הָרִים (for MT 'רִים') occurs in verses 34, 125 and 144 of this ψ. Admittedly, √כַּלָּא is not found elsewhere in
ψ to render ἐσκάλιν 1, but it may have been used for the sake of variety instead of ἀνάμαξα, which occurs twice in the vicinity (verses 95 and 100, to render ἠγαθοποιήματα).

(b) The mss give the true text of F', and Aph has been influenced by another source. This source is unlikely to have been G', for ἀνάμαξα is not an exact equivalent of συνήκα (thus Syro-Hex. has ἀναμαξα), and the general meaning of G' (ἐπεξεργάζεται τοῦ διδακτόντως με συνήκα) is rather different from that which seems to be intended by Aph. It seems more likely that Aph here depended on a contemporary Jewish source. According to J. Neusner 2, the arguments of Aph in his critique of Judaism and his defence of Christianity show that he had Jewish contacts. These Jews, however, did not represent rabbinical Judaism; "Aphraates knew Jews who quoted scripture, pretty much following its plain meaning" (p. 160) The rendering ἀνάμαξα for τεκμερίωσα is "plain" enough; and we know that this verse, under the interpretation which Aph seems to favour, was well-known among the Jews, cf. Aboth 4:1. עֵבֶר חוֹמֵם אָנוֹמֶה בֵּית מֵלֵם אָמוֹן שֵבַע הָאָם מִלְם מִלְם. It is therefore quite possible that the Jews with whom Aph associated were able to provide him with this rendering.

The choice between (a) and (b) is left to the reader.

We might also consider the possibility that the readings of F' and Aph represent two different ancient translations from

1. We find ἀνάμαξα (4x), ἀναμαξα (2x), ἀναμαξα (2x) and, as guesses, ἀναμαξα (ψ 47:8) and ἀναμαξα (ψ 101:2). It is omitted in ψ 36:4.
Hebrew into Syriac, and that this case is evidence for a "Kahle view"; but as it is only an isolated instance, I am hardly impressed by this explanation.

Most of Aph's variants are not in the direction of MT, and seem to be due to his having quoted the Peshitta "merely from memory" (Wright, p. 16). This is confirmed by the fact that a verse quoted more than once is sometimes found in different forms. In general, Aph had the same text as our P' mss, and as our ms attestation for Aph is older than that for the Psalter itself, our evidence for this same Psalter text is carried back thereby into the fifth century.

The testimony of Aph when the mss diverge is available in the following passages; in all but one (137:7) it is recorded by Barnes in his apparatus. In my list, "Ø" denotes that Aph's reading supports that of F in company with Nestorian authorities, and "f" that he has the reading of F together with as many of the "neighbouring" Jacobite witnesses as are available:

1. B.M. Add. 17182 (foll. 1-99) is dated 474 A.D. It contains the first 10 homilies. Ibid., foll. 100-175, containing the last 12, is dated 512 A.D. Another cent. vi ms (Add. 14619) contains all 23.
ψ 16:10 (f); 18:46 (Ø); 22:19 (Ø); 33:6 (Ø); 41:2
(here Aph has the reading not attested by F and the Nestorians); 41:3 (Ø); 51:6 (Ø); 51:13 (Aph's reading is only in CHQS); 51:19 (F is defective but the map gives Aph's reading); 81:6 (the Aph mss are themselves divided); 87:6 (f); 89:3 (Ø); 90:4 (Ø); 137:7 (here Aph goes against the sole testimony of F); 143:2 (Aph against EFR - our rules do not decide the choice either way).

On the basis of these passages, Aph can be tentatively located on the map. I was surprised to find that he does not come out in the vicinity of folio - as we might have expected for such an early authority - but in the area occupied by the Western mss, close to our earliest Psalter C (cent. vi ?). This result is based on only a few passages, and we must treat it with caution; but it does suggest that a text similar to that of the cluster CGHQS was attested in certain mss of cent. iv. There seems to be only one instance where Aph seems to have an error which he shares with some Western mss:

ψ 41:2

MT קְּשֵׁרִי נַּכְּבֵי אַל-ךְּלֵי בַּר יִשָּׁע לָנוּ וִיהָ נָה

P' (most mss) קְּשֵׁרִי נַּכְּבֵי אַל-ךְּלֵי בַּר יִשָּׁע לָנוּ וִיהָ נָה

1. For the figures on which this location is based, see Table B.8.1.
Before כְּפַר, a number of Western mss add Dalath (כָּפַר), as does Aph1; see the shaded area of fig. B.8.1:

Neither MT nor G' has a conjunction, and it seems that כְַפַר is an error. However, we cannot make much of this, because Aph may have added Dalath independently of the Western mss which bear it.

In another passage, ג 51:13, he agrees with the Western mss CHQS in reading כְַפַר: all the other authorities have כְַפַר. Despite Rule 1, the reading כְַפַר is to be preferred; for כְַפַּר is feminine elsewhere in the Peshitta Psalter (e.g. ג 91:6), and the wish to use masc. instead of fem. of the Holy Spirit could easily have brought about the same change in many different mss independently.

1. T has כְַפַר.
In all the other variant passages in which Aph is available, both the intrinsic criteria and our two Rules suggest that he has the reading of the Urtext; and to that extent, his testimony confirms the policy arrived at in Chap. 7.

Before leaving Aph, we look at one place (ψ 16:10) where the P' mss offer two alternative readings:

AEFGH, and C (perhaps a second hand)  
LeUaUc = BDJKNPT m

The verse is quoted in Acts 2:27, where P' has ψ. Both this fact and Rule 2 suggest that ψ is original. Now, both Aph and Ephraim (see below) quote the verse, and both tell us expressly that they are citing ψ (not Acts). Aph has ψ, but Ephraim ψ. It seems that Aph has the true reading, but Ephraim was influenced by Acts.

We now come to the Psalter quotations of Ephraim Syrus († ca. 373). They are not mentioned at all by Barnes, perhaps because we have no commentary by Ephraim on the Psalter. Some examples however are given by Peters (pp. 284f.), who believed that Ephraim's ψ text diverged considerably from that of the P' mss. He mentioned one passage (ψ 50:16) in which Ephraim's text showed a "targumische Breite der Wiedergabe." This he took to be a valuable relic of the old western Aramaic version which in his submission underlies the Peshitta Psalter.
Peters' first item of evidence to show "dass jedenfalls der Psalmentext der Zeit Aphrems nicht schlechthin mit demjenigen des überliefernten Ps-Ephraim-Psalter identisch war" is a quotation of 81:14-15, followed immediately by 2:5 and 83:17a, from the homily Adversus Judaeos (Ed. Rom. VI, 216f):

Certainly it would appear that Ephraim had a considerably different text. However, two important points of method are involved here.

The first is that not all works ascribed to Ephraim can be accepted immediately as authentic. This problem was encountered by F. C. Burkitt in his book: "S. Ephraim's Quotations from the Gospel" (= Texts and Studies VII.2), Cambridge 1901. His procedure for recognising genuine writings will be discussed presently; for the present we need only say that not one of Peters' quotations is taken from an undoubtedly authentic work. It is of course
desirable that our investigation should at least start out from writings which are under no suspicion of being spurious. All this does not, of course, lessen the fact that some author, who may have been Ephraim, apparently had a Peshitta text very different from our own.

The second methodological point is that this homily - like all the works of Ephraim which Peters cites here - is metrical, i.e. the number of syllables in each line follows a set pattern. This places a constraint on the author, who may have to modify his text. Thus we must investigate the possibility that divergence from the ms text in a metrical work may be due to the demands of metre, before we are entitled to suppose a divergent form of text.

In the light of this, most - though not all - of the variation in this example may be explained. Adversus Judaeos is written in stanzas of four lines each containing seven syllables. Once the author had decided to use these Psalm passages, he found that 81:15, in its Peshitta form, was too long (11 syllables in a, 8 in b). We therefore find \( ἡς ἰη \) for \( ἡς ἰη \), and the singular ending \( ἴη- \) for the plural \( ἴη- \). He then wanted to make a whole stanza out of 2:5 and 83:17a, but here found the Peshitta text too short; so he added \( ἱρα ἤ ἦ \). Not all the variation can be explained on this basis, viz:
In all these remaining variants, the P' ms stand nearer to MT. Eph's readings seem to be caused not by a different P text but by a wish to keep the past tense throughout after (thus (and) by imperfect recollection of the text of the Peshitta Psalter. Thus when we consider the quotation in the context of the work in which it occurs, we find that it presents no solid evidence that this author's text diverged from our own.

The reader can easily verify that similar considerations will explain the variants noted by Peters in \( \psi 45:11 \) and \( \psi 143:2 \). His last example ( \( \psi 50:16 \)), from the homily beginning (is to be found in Ed. Rom vi 413. This work is in stanzas of which the lines apparently follow the pattern: \( 14 + 14 + 14 + 14 + 7 \). I write "apparently" because Syriac prosody can be a complicated business, especially when the lines are relatively long.

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1. A good introduction to the subject is in G. Bickell, "S. Ephraimi Syri Carmina Nisibena," Leipzig 1866, pp. 31-35. My examples are from his edition.
This is partly because the punctuation marks in the Roman edition are sometimes wrongly placed, and partly because words can sometimes be reckoned as having more than their "face value" of syllables by diaeresis (e.g. Carmina Nis. 9:35 as three syllables, but normally as two), or as having less by synaeresis (e.g. Carm. Nis. 1:53 as one syllable, normally ). Thus it may be difficult to recognise the metre in the first place. As far as I know, the metrical scheme of this homily has never been fully investigated, but after examining the whole work I am reasonably sure of this scheme.

In this passage, then, we have:

The quotation in Ephraim is longer, but I believe this to be due to the need to get two 14-syllable lines out of it; the Peshitta form has only 17 syllables. I give the whole stanza with the quotation in square brackets:

1. diaeresis 2. synaeresis
"Niemand wird die hier zu Tage tretende targumische Breite der Wiedergabe verkennen können," says Peters of this passage; but the expansion can be adequately explained in terms of Ephraim's own need to fill up two lines of 14 syllables each.

Peters' examples, then, do not take us far in the study of the ψ text of Ephraim, because they come from metrical works of doubtful authenticity, no account being taken of the constraints imposed by the metre. Thus room was left for a further investigation.

In order to ensure that I confined myself to authentic works of Ephraim, I followed Burkitt, who drew up a list of genuine works (pp. 24f.) on the principle of admitting only those which were extant in mss earlier than the Mohammedan invasions. Thus every work in his list is attested by at least one ms not later than cent. vii. There have been a number of developments since that list was drawn up. Critical editions of several of Ephraim's works have appeared in the Louvain series "Corpus Scriptorum Christianorum Orientalium". All that is extant of Ephraim's Prose Refutations was transcribed by C. W. Mitchell from the ms B.M. Add. 14574, of which about five-sixths had been palimpsested; and the task was completed, after Mitchell's heroic death in 1917, by his teachers, A. A. Bevan and F. C. Burkitt. More recently, there has come into the hands of Sir Alfred Chester Beatty a

Syriac ms of Ephraim's commentary on the Diatessaron, with which scholars had been familiar only in an Armenian translation; the ms (Chester Beatty 709) is assigned to a date not later than the early 6th cent. The text has been edited and translated by Dom Louis Leloir. Both the Prose Refutations and the Commentary on the Diatessaron may of course be counted without hesitation among the authentic works. I have tried generally to admit only quotations which are introduced by some formula such as ܡܳܪܝܐ ܕܐܒܪܳܐܢܐ ܕܡܳܫܳܚܳܘܬܐ, or are otherwise recognisable as direct quotations rather than reminiscences. This yielded about forty passages. Unfortunately, even the undoubtedly genuine works of Ephraim are too voluminous for me to be sure that I have not missed relevant material.

The quotations follow. Where I have not made any comment, Ephraim agrees with all Barnes' witnesses.

\[ \text{Eph} \]

\[ \text{Psi} 2:7 \ ( = \text{Acts 13:33}; \text{Heb 1:5, 5:5}) = \text{Ed. Rom v 516} \]

\[ \text{Psi} 2:7 \]

An abbreviated text, to fit into a 10-syllable line.

ψ 2:9 = Lamy 1 667.

Eph. λόγος καταλύω, P' Κατάλυσθαι...

ψ 16:10 ( = Acts 2:27) = Diat. 11:3

Eph's text, beginning ἀπέκτησα, differs from that of Barnes only in that Eph supports the reading λάτασα (with many mss, and with P' in Acts 2:27) against λάτσα, which has the support of Aphraates. See Thes., p. 8.14.


ψ 38:12b= Diat. 21:8

ψ 45:7 ( = Heb 1:8) = Lamy 1 667.

Eph. ἡ ὡλὴ καταλύω, P' Κατάλυσθαι

Thus Eph confirms P's somewhat unexpected rendering of ἀπέκτησα. (G' [NT] have τάξησα εὐθύτητος).


Eph. ἡ ὡλὴ καταλύω, P' Κατάλυσθαι ῥόη

This seems to be a loose paraphrase.
ψ 50:10b = Ed. Rom. iv 18.

The work in which this quotation occurs, viz the Commentary on
Genesis, is to be found not only in the Roman edition but also in the
critical edition of R-M Tonneau ("Sancti Ephraemi Syri in Genesim et
in Exodum Commentarii", published as CSCO vol. 252, Louvain 1955),
wherein this ψ citation will be found on p. 22. It is perhaps the most
interesting of Eph's ψ quotations:

MT: 

G': κτήνη ἐν τοῖς ὀρεσίν καὶ βόσκο (ἐκ τοῦ being understood
as "cattle")

So P': 

On ψλαμ in Gen 1:21, Ephraim comments, according to
the codex unicus (Cod. Vat. Syr. cx, 6th cent):

There is a textual difficulty here. δύλως cannot
be right, and is emended to ἄλλας in Payne-Smith col.
2773, 1.8. This emendation is accepted by Tonneau; we
shall return to this point presently. But whatever we make of סלָד, the general sense is clear. Eph believed that סלָד referred to Leviathan and Behemoth, but he was concerned to reconcile the statement in Genesis that the סלָד were creatures of the water, with indications elsewhere (Job 40: 15f, and our passage י 50:10b) that the Behemoth dwelt on dry land. Thus Eph apparently knew a rendering of this passage which differed from P', in that והנה was construed as "thousand", and יהיו was taken as a nome proprium.

Before proceeding to the implications of this rendering for the P' text, let us compare other ancient authorities. H' has: pecudes in montibus millium (כוהו "thousand," but יבנה a as common noun). T', which in י has been handed down only in a late recension, seems to depend here on the Talmud and Midrash, to which we now turn. In Bab. Talm., Baba Bathra 74b, we find the תִּהוּ of י 50:10 taken to refer to the Behemoth, which had occasion to regret its great size:

The Behemoth was seen in י 50:10 also by Midrash Rabbah (Lev. R. 22:7).
What is recorded here agrees remarkably with Eph, in that this verse is taken to mean that the Behemoth feeds (גְּבוֹץ) and lies (נָפָל) on a thousand mountains.

We now come to T' (ed. Lagarde):

The plural indicates that does not represent Behemoth (as it does in Job 40:15). But the reference to the righteous feasting in the hereafter on the recalls the Baba Bathra passage, while the "der wilde Ochs", Levy is apparently connected with the tradition recorded in Levit. R. of a beast which feeds upon a thousand mountains.

What does all this signify for the P' text? Two possibilities suggest themselves. Either Eph here preserves the true P' reading, which finds a remarkable parallel in Rabbinic sources, whereas the text offered by the P' mss is a later adaptation to G'. In that case, we have important evidence on a celebrated controversy:

1. This problem is discussed further in Chap. 9.
Are the numerous agreements of P' with G' against MT and all the other versions due to a later revision of the P' text after G', or to the influence of G' on the translators of P' themselves? This quotation in Eph would seem to preserve a fragment of an older P' text, which was effaced through assimilation to G'. On the text of the mss and that of Ephraim represent two different translations from Hebrew into Syriac, and we have presumptive evidence for a "Kahle view".

Against the latter argument we may state that there is little confirmation elsewhere for a "Kahle view". In fact the only other cases where it can be regarded as a possibility, viz ψ 119:99 in Aph and ψ 143:10 in Eph (see below), also admit other explanations. It would seem that the former explanation were correct, that Eph here preserved the true P' text, and that P' had undergone an early revision wherein many readings which reflected Jewish exegesis and stood close to T' were replaced by material from G'.

But one point remains: the textual anomaly in this Eph passage. In Payne-Smith we are told to read מְזָכַר for מְזָכַר; in support of this MT is adduced. The trouble is that if we adopt מְזָכַר, which supposedly rendered מְזָכַר, then the relevance of the quotation is lost. It would be implied that Eph understood the verse to refer to animals in general, and not to the Behemoth, as the context demands.¹

¹. I think it unlikely that מְזָכַר, like מְזָכַר in T' to Job 40:15, means Behemoth. Payne-Smith does not give any passage which might confirm this. In Job 40:15, P' has מְזָכַר (מְזָכַר, A)
I was unable to account for גוונא without resorting to violent emendation (e.g. כונן), until I showed the passage to Mr. Loewe, who conjectured גוונא "juxta Hebraeos."

The construction גוונא ר'ו "David according to the Hebrews" may be justified on the analogy of a note in some of our early Jacobite mss. These preface the Peshitta Psalter with the following title, which Barnes reports (p. 11) as it appears in Cod. A:

There are some variations in the other mss, but they need not detain us here. The title may be translated: "The Psalms of David of the Separated Ones (?), from the language of Palestine, which were translated by them from Hebrew into Greek, and from Greek into Syriac."

The phrase גוונא ר'ו stands — according to Barnes' reports — in ACDEF. What does it mean? In an earlier article (J.T.S. 1901, p. 191), Barnes pointed out this title as evidence that these copyists laboured under the astounding belief that P' had been translated from G'. He therefore suggested that גוונא meant 'the Separated Ones', viz "the Seventy who worked according to an often repeated tradition in separate cells." It would then appear that the scribes "believed that the Seventy had translated their own Greek into the Syriac of the

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1. R (cent. x) has גוונא( 'the Psalms of David...one by one'?), but this can hardly be original.
Peshitta." I am inclined to accept that ܡܐܕܝܐ ܕܐܝܐ means "David according to the Greek", although I am not entirely convinced by Barnes' explanation (which, it is fair to add, was only tentative). In particular, the title ܡܐܕܝܐ ܕܐܝܐ ("Gospel of the Separated Ones", as opposed to the Harmony or Diatessaron) for the Old Syriac Gospels shows a resemblance which looks as if it should be more than coincidental; but I cannot see where - and indeed whether - it fits in. This does not however concern us now. What matters is the phrase ܡܐܕܝܐ ܕܐܝܐ. If this - as seems likely - meant "David according to the Greek", then surely ܡܐܕܝܐ ܕܐܝܐ could have stood in Eph. The corruption to ܡܠܐ was especially easy because ܡܠܐ is used frequently by Eph in this part of his commentary on Genesis, in the sense "created beings."

If this conjecture be correct, we have here a parallel to other passages in which Eph quotes the ܡܠܐ (elsewhere singular). These citations were collected, I am told, by G. L. Spohn1; but as Spohn's work is practically unobtainable, I would add that they are also listed by J. Perles.2 Eph introduces them with formulae such as ܡܠܐ ܐܠܐ ܐܠܐ 3 or ܡܠܐ ܐܠܐ ܐܠܐ 4 The Ebhraya.

1. Collatio versionis Syriacae, quam Peshito vocant, cum fragmentis in commentariiis Ephraemi Syri obvius, instituta a G. L. Spohn. Leipzig 1785-90. I have been unable to find a copy in London.
and his relationship (if any) to the interpreter quoted by Greek fathers as 'Eβραῖος (or the like), are discussed by Field.\(^1\) Moreover, in a study of the Armenian translation of Ephraim's comments on Genesis 1-38, Lagarde discovered several references to the Ebhrayā which are not to be found in the Roman edition of the Syriac text.\(^2\) I do not know of any exhaustive investigation of the Ebhrayā, but as Perles pointed out, we do know that much that is attributed to him is paralleled in Jewish tradition, e.g.

Gen 24:63  יְהֹוָה  MT, אַדְּנֶא מְשַׁיֵּר Pesh.,

אֱבֹרְנָה  Ebhrayā apud Eph. (Ed. Rom, IV 173),

יְהֹוָה  Onkelos.

Gen 36:24  יְהֹוָה  MT, אַדְּנֶא מְשַׁיֵּר,

אֱבֹרְנָה  Ebhrayā apud Eph (Ed. Rom, IV 184),

יְהֹוָה  Onkelos.

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2. "Über den Hebräer Ephraims von Edessa"; this may be found in his "Orientialia" (Gottingen 1879-1880), Zweites Heft, pp. 43-63.
Thus there is no difficulty in supposing that Eph's interpretation of Ps. 50:10b, which seems to depend on a Jewish tradition, could also be introduced as from the Ebhrâyā.

Apart from restoring the Syriac text of Ephraim, this emendation leads to two interesting conclusions.

First, none of the references to the Ebhrâyā hitherto discovered appear among the undoubtedly genuine works of Ephraim. Most are to be found in those Commentaries on the Old Testament which are not taken direct from Ephraim's works, but excerpted from a \textit{Catena Patrum}, compiled by one Severus of Edessa in 861 A.D. Of that work Burkitt (p. 87) says: "The Catena is made up of extracts and abstracts from many writers... It is often impossible to discover where the passages taken from S. Ephraim really begin or end..." Thus we could not tell whether references to the Ebhrâyā went back to Eph himself. But now we have for the first time a citation of the Ebhrâyā within a work which must be regarded as authentic; and this enhances the possibility that references to the Ebhrâyā within the Catena also go back to Ephraim.

Second, Eph shows here that he is familiar with a rendering other than the ms text of the Peshitta; but from the fact that he specifies that this interpretation is taken from the Ebhrâyā, we may deduce that it was \textit{not} to be found in his Peshitta text.\footnote{Thus in Ed. Rom iv 380, in a comment on 1 Sam 24:4, Ephraim (or Ps.-Eph.) refers to \( Ps' \) - which has the euphemistic (?) rendering \( רָחַבָּנוּרָאִּים \) as \( חַיָּה \); in contrast to the Ebhrâyā, which has \( רָחַבָּנוּרָאִּים \) as \( חַיָּה \).} Indeed, there is no
reason to suppose that the Peshitta translation of 50:10b, as it was known to Ephraim, was at all different from our own.

We now resume our discussion of Ephraim's Psalter quotations.

\[ \psi 55:7-9 \] = Overbeck 114, lines 8 ff.

This quotation, from \( \text{HA} \) in verse 7, agrees exactly with P'.

\[ \psi 69:12a \] = Prose Ref. ii 73:40-41.

\[ \psi 77:17 \] = Diat. 12:9

Eph quotes the last two stichs (from the second \( \text{PS} \)). For \( \text{HA} \), in all P' mss, the Ephraim ms has \( \text{HA} \).

\[ \psi 77:20 \] = Diat. 12:9

The quotation begins at \( \text{HA} \) (the third word of the verse).

\[ \psi 78:20 \text{ab} = \text{Lamy i 242.} \]

Ephraim omits \( \text{HA} \), against all other authorities, presumably through quotation by memory.

\[ \psi 78:24b = \text{Diat. 12:10} \]

For \( \text{HA} \), Ephraim has \( \text{HA} \) (MT \( \text{HA} \), versions have ordinary genitive construction).

1. Eph. has \( \text{HA} \) in 7 and \( \text{HA} \) in 8.
The third stitch alone is quoted: ἐὰν τὴν ἀναποκρίνεται τοῦτο ἐκ τῶν ἀνθρώπων.

The Peshitta has:

Ephraim's omission of לְהָעֵד has no support in other authorities. Eph has בְּ (with BCEFNRTLe) against (AGHJKLOSm Ua Uc). This case is not covered specifically by our Rules, for F does not have the whole-hearted support either of the Nestorian authorities or of the neighbouring Western mss. Nevertheless, as F enjoys some support from both quarters (N;C), the map suggests that Eph's reading is original (MT יִרְאֶה, כְּֽלָּכָה).
ψ 87:6 = Ed. Rom v 396

Here we have a recasting in Eph, giving two 7-syllable lines:

P': ἐδέξατο τά σπώματα, καὶ ἠκολούθησεν ἡμῖν

Eph: ἐδέξατο τά σπώματα, καὶ ἠκολούθησεν ἡμῖν

For ἡμῖν, we find ἡμῖν in Nest. mss and in R; but Eph's reading is to be preferred, by Rule 2. Aph also quotes this verse, and has ἡμῖν.

ψ 89:10 = Diat. 12:8.

The second half of the verse is shorter in Eph:

P': ἡμῖν ἀνενεχθεῖ ἐκάθεν (MT, G' are similar)

Eph: ἡμῖν ἀνενεχθεῖ

Once again, the explanation seems to lie in Eph's memory.
\[ \psi 91:7a = \text{Diat. 10:12} = \text{Prose Ref. ii 115, lines 43-5. Only the first stitch (} \psi \text{) is quoted, on both occasions.} \]

\[ \psi 95:10 (= \text{Heb. 3:10}) = \text{Diat. 17:6.} \]

Eph construes \( \text{y} \) with the succeeding words (so MT), whereas Heb. 3:10 takes it with the preceding verse. The punctuation in Barnes' edition, which presumably represents the consensus of the mss, agrees with Hebrews against Eph.

For \( \text{y} \) (P' in \( \psi \) and Heb.), Eph has \( \psi \).

\[ \psi 99:8c = \text{Carmina Nisibena no. 71, init.} \]

This hymn, on the Resurrection, begins (in Bickell's ed.):

Eph regarded the phrase "I shall recompense Moses, Aaron and Samuel" (cf. v.6) as a hint of their resurrection. Our P' mss, however, have not \( \text{y} \) (fut.) but \( \psi \) (imperative). I quote the whole verse:
The last phrase corresponds to רכשו על עלייתם in MT.

We must now make sure that our text of Eph is reliable. Turning to the critical edition of E. Beck (=CS目标 240, Louvain 1963), we find that two of his mss, including our oldest authority for Eph (viz B.M. Add. 14571, dated 519 A.D.; D in Beck) agree with the P' mss in that they have לְָוָָו in the second line (but both have אָוָָו in the fifth). Despite this, the interpretation which Eph places on the phrase must imply that he had the future form, not the imperative. We may compare the curiously parallel argument recorded in the Bab. Talmud, Sanh. 91b:

אמר רבי מאיר מנין להתרות המทานין מן התורה שנאמר (שמות טו, ז) אָזֶה מֵאוֹשֵׁב יִשְׂרָאֵל הוא השורר הוזא את לה, רָא אָזֶה מֵאוֹשֵׁב יִשְׂרָאֵל הוא השורר מַן התורה.

How then are we to account for the reading לְָוָָו in Eph? It is certainly inferior as a P' reading; the rest of the verse is addressed to the Lord, and a change to 1st person would be most awkward. I would therefore suggest the following explanation: We know (Nölderke § 51) that an Alaph with a vowel is sometimes prefixed (sc. in writing) to an initial consonant which has not a full vowel, and that prosthetic Alaph is more frequent in our older mss. This phenomenon of spelling tells us something about contemporary pronunciation, viz that some words which began with a cluster of two consonants had a "helping" sound attached to them, at least at an early stage in the
development of the Syriac language. We know furthermore that the prosthetic vowel in Syriac was /ə/ in many cases, thus "tqatāl → etqatāl". Is it possible, then, that in Eph's day the word ḍПрав could be mistaken by the ear for ḍRaus?

With regard to the particular initial consonant cluster P6Fσσ, evidence that such a helping vowel was sometimes prefixed may be drawn from the Syriac forms (attested in the Thesaurus Syriacus) of certain words and names of Greek or Latin origin. Thus we have

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<th>Syriac</th>
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<td>ḍProveedor</td>
<td>ḍProton</td>
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<tr>
<td>ḍPrusias</td>
<td>ḍPrues</td>
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Furthermore, Dr. S.P. Brock tells me that he has observed a prosthetic Aleph written before ḍProveedor (προκατακαί) we may add the medical term ḍProveedor (ὄρυγμα). It is tempting to go further and to explain in terms of such a prosthetic Aleph two puzzling forms which appear in the Peshitta to Judges xii, viz. ḍProton (Q."?) and ḍPrusias (V. 13.15), where the initial has ḍPrusias and ḍPrusias. The additional Aleph in P cannot be due to dittography, for in none of the three places is the letter preceding the name an Aleph, either in MT (K) or in P (Q). We may imagine that the P translator vocalised ḍPrusias, in more or less the same way as the Greek translator represented in ḍProton (Ὅρυγμα). This form was carried over into the Syriac, being pronounced with a prosthetic vowel, so that an Aleph was prefixed, either by the translator himself or by an early scribe (who would have worked certain, not later, and probably much earlier, than cent xii, to which our earliest ms of the Syriac Judges are assigned).

In this ḍ passage too, then, we need not suppose that the P text familiar to Eph differed from that of our ms. His ear having apparently confused ḍПрав with ḍProton, the phrase ḍRaus ḍProton - as Eph imagined it to be - remained in his mind, and eventually he embodied it into his hymn.

θ 109 1ab = Prose Ref. 141, lines 9-11.

Here Eph has ḍRaus, which supports ḍRaus (F + Nest.) against ḍRaus (all other authorities), and thus Rule 1 receives striking confirmation. See Thes., p. 747.

θ 109 16 = Overbeck 115 6 ff.

θ 109 20 = Dist. 11 7.

θ 109 24 = Prose Ref. 11 73, 11. 32-34.

The Ephraim ms has not Seyane (PerPixel) unlike P (PerPixel).

θ 110 1 (= Lk 20 42, Acts 2 34, Heb 1 13)

Lamy 1 669.

Eph has ḍProveedor ḍProveedor in an allusion rather than a quotation (P').

2. These are the forms found in Ice and in 7α. I have not consulted any other P. B. Drfe'rov's detailed study, "The Transmission of the Text in the Peshitt", manuscript of the 1 century of Juda "Jewen 1957", does not mention any difference among the ms in either of the two verses quoted.
\[\psi 115:5b, 6a = \text{Lamy i 163}.\]

In 6a,  כָּל בְּלִים is omitted by Ephraim (but not in 5b).

\[\psi 116:10 = \text{Diat. 1:12}.\]

Ephraim has: יִתְּנָה שֵׁלֹשָׁה יָדוֹ וַיַּשְׁמֹא לְהוֹיָה שָׁבַע.

F': יִתְּנָה שֵׁלֹשָׁה, MT יִתְּנָה יִשְּׁמַע נַע

G': ἐπίκειεται, διὸ οὐλάρεα.

Is ἡ ἡρια Ephraim's expansion, or was he influenced by G'?

\[\psi 118:20 = \text{Diat. 21:21}. \text{The Ephraim ms has} \quad \text{ךָּנָּה} \quad \text{ךָּנָּה} \quad \text{ךָּנָּה}.

(P' mss. ךָּנָּה ךָּנָּה).\]

\[\psi 118:22a = \text{M} 21:42, \text{Mk} 12:10, \text{Lk} 20:17, \text{1 Pet} 2:7 = \text{Diat. 21:21}.\]

\[\psi 121:1 = \text{Überbeck 123, II. 19f.}\]

For מְנֹשֵׁה (MT 1 עָשָׁה, G' מְנֹשֵׁה), Ephraim has מְנֹשֵׁה. This suits the context of the quotation rather better, but is further from MT.

\[\psi 125:4a = \text{Diat. 15:11} \quad \text{(Leloir wrongly cites this as 18:26)}.\]

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1. Some Lucianic witnesses have מְנֹשֵׁה; but the agreement with Ephraim seems to be accidental.
Verse 3 is quoted in an abridged form, presumably for metrical reasons:

The usual equivalent for יִנְּהַ שְׁלֹשָׁה is יִנְּהַ שְׁלֹשָׁה , as in Eph., but יִנְּהַ שְׁלֹשָׁה is found in 9:21 (where the object is יִנְּהַ שְׁלֹשָׁה , not unlike this passage), and may be retained here.

יִנְּהַ שְׁלֹשָׁה = Diat. 15:9.

Ephraim quotes only: יִנְּהַ שְׁלֹשָׁה

P' mss: יִנְּהַ שְׁלֹשָׁה

MT: יִנְּהַ שְׁלֹשָׁה

Elsewhere the adjective יִנְּהַ שְׁלֹשָׁה is rendered virtually always by יִנְּהַ שְׁלֹשָׁה and never by יִנְּהַ שְׁלֹשָׁה. I am inclined to regard Ephraim's reading as original, and that of the P' mss as an attempt to improve on what may have seemed an unduly plain phrase. Perhaps, however, the mss are original, and Ephraim was influenced by G' (τῷ πνεύματι σου τῷ ἁγιασθέν).
Our study, far from confirming the assertions of Peters, agrees with Burkitt's opinion¹: "...S. Ephraim is a witness for a text of the Syriac Old Testament almost exactly agreeing with that of our mss and printed editions. In several instances he quotes with the slight inaccuracies of a man who has not verified his allusions." There seems to be no prima facie case of a different Peshitta reading in Ephraim, although in 50:10b he knows of an "extra-Peshitta" rendering.

There are only four places where the mss diverge and Eph's reading is available, viz 16:10; 85:9; 87:6; 104:1. Thus we cannot find a meaningful location for him on the map.

We now come to native Syriac authorities later than the Nestorian schism. It turns out - not surprisingly - that I have not found in any of these a reading which tends to prove that the writer had a P' text which differed substantially from that offered by the direct tradition. Nevertheless, the behaviour of these authors at points where the P' mss diverge gives us signposts, whereby we may associate a region of the map with a figure in the Church.

Our first is Isaac of Antioch († ca. 460), a monophysite². I know of no index of the scriptural quotations in Isaac's homilies, and have therefore perused Bickell's

¹ Prose Ref., vol. ii, p. cl1.
² All these witnesses are monophysite unless I expressly state otherwise.
edition of 1873-7 in search of $\psi$ references. As this edition contains only 37 out of about 200 works, and it is moreover likely that I have missed some material even there, my evidence is far from complete. Nevertheless, Isaac has the characteristic renderings of $P'$, e.g.

$\psi 2:12 = 11,8$  
\textit{Isaac,}  
$\textit{ibid. (sine add.)}$  
\textit{Isaac,}$^3$  
$\psi 73:3 = 11,228$  
\textit{Isaac, AFLQS}$^4$

1. We note that Isaac, like Ephraim (Prose Ref. i 41), uses the numbering of MT, not that of G'.
2. For an explanation of the symbols $\phi$ and $f$, see p. 3:11; the sign $>$ means 'against'.
3. The line in Isaac runs: $\textit{Isaac,}$  
\textit{whence it is likely that he did not have} $\textit{Isaac,}$
4. This is not covered by our two Rules, but may be accepted because it is in $F$ and has some support from neighbouring (QS) and Nest. (L) mss.
ψ 73:6 = 11, 228  ינשנשא  ע"יאכ, 0 > ינשנשא
ψ 73:11 = 11, 230  ינשנשא, BDJRTLe > ינשנשא(0)
ψ 73:25 = 11, 232 (sine add.)  ינשנשא, f > ל"כ ינשנשא

Except for ψ 73:11, Isaac has the variant which is preferable on distributional grounds, and cannot therefore be associated with any particular mss on the grounds that he has an error in common with them. As for ψ 73:11, ינשנשא is found only in Western mss, but the agreement may well be coincidental: ינשנשא ינשנשא[7]... ינשנשא. Thus we can say little of Isaac's text-type.

Our next writer is Philoxenus, Bishop of Mabbogh (485-519). Much of his work has appeared in the editions of Budge2, Vaschalde3 and De Halleux4, though much remains unedited. Barnes consulted Budge's edition, and reported (p. xxv) that the affinity of Philoxenus' text could be judged from a number of passages, "in all of which Philoxenus gives, as we should expect, the Jacobite reading." We may supplement Barnes' report by saying that in one passage (ψ 6:8), Ph has ינשנשא, in agreement with the Malkite PT,

1. The stich in Isaac runs ינשנשא ינשנשא. Here ינשנשא is superfluous (cf context in Isaac) and has been added to fill up the line. This would not have happened if Isaac had had ינשנשא.
4. CSCO, Syr. 98 (1963).
against 'םיינֵי in all the other mss (see Budge, p. 207); and in יִתְמֹר, Ph has 'ֶֽצְרָם, which is the Nestorian reading, whereas all Jacobite authorities (except for Barhebraeus) have יִתְמֹר (Vaschalde, p. 107, l. 8). But he has a Jacobite reading in the other passages, viz

יִתְמֹר 6:7, 7; 26:6; 33:8; 38:9; 39:3, 3; 69:11; 71:16; 73:25, 25 (Budge 92'םיינֵי' וּלְבָשָׁנָת); 78:71; 89:37 (de Halleux, p. 59, l. 12 'םיינֵי' וּלְבָשָׁנָת'); 143:2; 148:5 (Vaschalde p. 156 l. 11 'םיינֵי' וּלְבָשָׁנָת'). On the basis of these 17 passages, he may be assigned to a location on the map close to the cluster CGHQS. These in turn may be tentatively regarded as representatives of an "early Monophysite" type of text.

Turning now to cent. vi, we note the greater Commentary on the Psalter by Daniel of Salah (542). Daniel's Discourses on the first two Psalms only were published by Diettrich. Barnes took over this material, and also consulted BM. Add. 17125 in some other passages. This gave 16 places where the mss diverge and Daniel's reading is given, viz יִתְמֹר 2:1, 3, 3, 10, 12, 12; 16:10; 45:2 (apud Barheb.), 5, 9, 9, 9; 48:13, 13; 60:3; 110:2. From these, a location is obtained within the Western group, not far from J, but tending somewhat towards the East.

1. Most of these readings are noted in Barnes' apparatus (see also Addenda thereto, pp. lvi ff.). Only when they are not in Barnes do I go into detail.
3. as an appendix to B. Z. A. W. 5.
Thomas of Marga (ca. 840) is our first Nestorian authority. In his "Book of Governors," he cites about twenty-five verses (or parts of verses). He occasionally diverges from the P[mss, but his unique readings have little to recommend them (e.g. the omission of יִנָּה in פ 89:2). There are only three occasions on which he takes sides in a variant passage. In פ 7:15, he supports the Nest. texts in reading מִשְׁלֵי, against the Western מִשְׁלֵי. He testifies in פ 29:9 to מִשְׁלֵי, against מִשְׁלֵי (only in Polyglots and Le). Finally he gives a paraphrase of פ 68:23 which seems to agree with certain Western texts:

This has no Nest. support, and Rule 1 indicates that it is not original. Let us now compare Thomas, p. 204:

This tends to support מִשְׁלֵי, but the evidence is far from conclusive. It is possible that Thomas fell victim independently to the same phonetic error which gave rise to the variant מִשְׁלֵי in the direct tradition.

At about the same time, Isho-dadh of Merw, also Nestorian, wrote a commentary on all the books of the Nest. canon of the Old Testament. This work is preserved in B.M. Or. 4524. Diettrich published the commentary on Psalms 16,

22, 45, 68 and 69; Barnes used Dietrich's work, and consulted the ms itself in a few other passages. This yielded 19 readings, on which I base the following remarks. In almost every case, Ish agrees with all the Nest. authorities available. The latter are usually accompanied by some Western witnesses (e.g. C in 8:7, GJLe in 68:10), but the only mss to side consistently with Ish are Nestorian. In 40:10, both Eastern and Western texts vary between כְּשָׂרךְ and אַבְרָהָם (MT וַיהוָה). Ish has כְּשָׂרךְ, with K and with Barheb.'s testimony that this is the reading of the Nestorians; however, LmO Ua Uc have אַבְרָהָם (NX 'abstain'). To sum up: Although we may confidently associate Ish with the Nestorian cluster, the 19 readings are not enough for us to locate Ish in any particular part of it. But the ms is available, and a special study would doubtless yield a more specific result.

The anonymous Jacobite introduction to the Psalter published by Diettrich (B.Z.A.W. 5) may be assigned to a date between the 10th and 12th centuries (p. xliii). He quotes a great many passages, and over 30 of these cover points where the P' mss vary among themselves. (Only 17 of these readings are in Barnes' apparatus.) The passages where Anon is available are:

1. 8:7; 22:3; 36:7, 7; 39:4; 40:7, 7, 18; 45:2, 4/5, 7, 7, 15; 51:12, 13; 68:10, 23, 26, 35.
On the map, Anon appears within the Western cluster, towards the top. It lies within the area covered by our latest Western authorities (E, P, R, T, Le, Barheb.). We may conclude that this upper region of the map is occupied by a loosely-knit group of "late Western" witnesses.

The citations of Barhebraeus (1226-1286) are too numerous to be discussed here in detail, but numerous enough to lend confidence to our map location. Whereas the Jacobite mss lie within a wedge-shaped region which radiates outwards from Ω in a "north-westerly" direction (see Fig. B.9.2), Barhebraeus lies further east, and hence nearer to the Nestorian
cluster. The reason may be, as Barnes suggested (p. xxvi), that "a considerable Nestorian element had intruded itself into Jacobite codices by the beginning of the xiith century". On the other hand, the Western mss of about the same time or later, viz BPT, do not show this affinity to the Eastern text; and I wonder whether an alternative explanation is that Barhebraeus, who had "a minute knowledge of the Nestorian and Jacobite texts of his day" (p. xxv), adopted an eclectic approach. Thus the fact that his text shows Nestorian influence does not necessarily imply that the Jacobite codices of his time in general had been affected.

Before leaving the native Syriac writers, I give the following table of the relation of certain writers to the P' mss. Under each writer's name are three columns; the first gives the number of times he diverges from each P' ms; the second, the number of passages in which both his reading and that of the P' ms in question are available; the third gives the first as a percentage of the second.

1. Barhebraeus was appointed in 1264 to the office of Maphrian, whereby he became the Jacobite Patriarch of the East. He then took up residence at the monastery of Larkaltai near Mosul, which lay in predominantly Nestorian territory. These facts go some way towards accountin for his Nestorian connections.
<table>
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<tr>
<th>A</th>
<th>Aphraates</th>
<th>Philoxenus</th>
<th>Daniel</th>
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TABLE B. 8.1
We now come to Syriac translations from the Greek. One might have thought that these could tell us only about the Greek Vorlage, not about the Peshitta text in the time of the translator. However, A. Baumstark shewed\(^1\) that a translator who encountered a biblical quotation in his Greek text sometimes allowed the Peshitta to influence the wording of his translation, and sometimes did not translate the Greek at all but substituted the Peshitta version. According to Baumstark, when a Syriac translation diverges from the Greek original, it is reasonable to suppose that \(P'\) is being quoted\(^2\). However, this indirect evidence must be used with caution. We cannot determine the form of the Greek text from which the translation was made; the mode of translation itself may give rise to variations; the Peshitta may have been imperfectly recollected by the translator; textual corruption in the Syriac version of the work remains a possibility. Thus Barnes (p. xxvi) attached little importance to such evidence; however, Peters (pp. 286 ff.) believed that translations from the Greek deserved investigation, and he claimed to have found therein two "alte Pešittā-Varianten".

Our earliest translation is of course the New Testament. An exhaustive study was not possible, but some "spot checks" suggest that in places where an NT author quotes the Psalter,

\begin{enumerate}
\item "Das Problem der Bibelzitate in der syrischen Übersetzungsliteratur"; in Oriens Christianus 3, ser. VIII, pp. 208-225.
\item "Ein etwa vom syrischen Übersetzer an Stelle desselben eingesetzter ihm geläufiger Text in der eigenen Sprache könnte füglich nur derjenige der Pešittā gewesen sein" (p. 209).
\end{enumerate}
the P' text in NT has sometimes been influenced by the P' text in \( \psi \). For instance:

Heb. 1:7 Gk ξυρὸς φόνγα, \( P^{NT} \) - \( \psi 104:4 \) (MT

Heb. 3:10 Gk ἐδιπλασίων τῆς ξυροῖς \( P^{NT} \) - \( \psi 95:10 \) (MT

so \( \psi 95:10 \) (MT

But I have found no evidence that this Syriac Old Testament text was any different from that of the P' mss. Indeed, when we consider the complexity of the situation - the textual problems of the New Testament Peshitta, the possibility of influence between Psalter and New Testament in either direction and at several stages, and so on - it seems that these citations can offer little help in our study of the Peshitta Psalter.

Peters' first variant (p. 288) is from Heb. 1:13, where Gk has ἰποκόδιον τῶν κόσμων κατο, \( P^{Heb} \) - \( \psi 110:1 \) (MT 

He found the former reading in other Syriac documents (including the Sinaitic text of Luke 20:43) and held it to be an old Peshitta (Psalter) variant. However, the use of ἰκαπεί may be an attempt to render the element ἰκαπεί in ἰποκόδιον, cf. the treatment of περι- in Lk 3:3.

ἐν τὰς τὴν περὶχωρον τοῦ Ἰορδάνου

Or perhaps the influence of Mt 22:44 and Mk 12:36 is responsible, for there the same phrase ( \( \psi 110:1 \) ) is quoted
in the form ἐπιτύμβῳ (not ἐπιτύμβῳν) τῶν κοιμῶν σου. This too would account for the appearance of ἔπιστεφα elsewhere. An old variant in the P' Psalter is not the only explanation.

It has been pointed out by Peters that the Ecclesiastical History of Eusebius in Syriac\(^1\) is rich in ψ quotations, many of which agree with P' against the Greek original – as far as we can imagine the latter to be, from our Greek ms evidence. The Syriac mss are ancient (one dated 462 A.D., another assigned to cent. vi), and there is also an Armenian version (collated by A. Merx) whose origin antedates both mss. In I. iii. 14 Peters claimed to find his second P' variant. Here Eus ends a quotation of ψ 45:8 with καὶ τῶν μετέχοντων σου, so G'; P' η λείψανον αὐτοῦ. However, the Syriac translator of Eus wrote αὐτὸν ἐπιθέου (so the two Syriac mss).

Yet here too, an old P' variant is not the only possible explanation. The translation accords exactly with Eusebius' own exegesis on this phrase (cf. the subsequent paragraphs), that the anointing of Christ was wholly different from that of any who had gone before Him. It is possible that the translator – whom we know (p. ix) to have allowed himself considerable freedom – followed Eusebius' interpretation in translating the verse itself.

I have examined the fifty-odd verses quoted in the Syriac Eus, and found them to be a resultant of the Greek Vorlage and a P' text which cannot be proved to differ substantially from that of our mss. A few passages are of interest:

\[\psi 2:2 = \text{I.iii.6}\] Eus Syr ὄνομα Ἱωάννην, but Gk (=G') καὶ τοῦ Ἰωάννην

Thus Eus Syr agrees with the Polyglots and Le, which also have \( \text{\textit{LX}} \), against all the other witnesses, which have \( \text{\textit{P}} \). But the agreement may be coincidental.

\[ \psi 37:35 f = X. i. 7 \]  Here Eus Syr (at least in the two Syriac mss) departs from his Vorlage to quote the verse according to \( P' \), whose attestation is thus carried back to cent. v. Aphraates, it will be remembered, quoted the verse in \( G' \) form.

\[ \psi 110:4 = I. ii. 16 \]  Eus Syr \( \text{\textit{LX}} \) in \( \text{\textit{P}} \)

\[ \psi 148:5 = I. ii. 5 \]  Eus Syr \( \text{\textit{LX}} \) in \( \text{\textit{P}} \)

These pilot studies hold out little hope that a search for \( \psi \) quotations among Syriac translations from the Greek will repay the effort involved.

Of more interest are quotations by Arabic writers dependent on the Syriac Bible. We shall examine here the two authors considered by Peters: 'Ali Tabari' (839-923) and Ibn Djawzi (cent. xii). According to Peters, the latter took over biblical quotations from Ibn Kuṭayba (828-871).

Ali Tabari's work is a defence of Islam in which he attacks Judaism and Christianity by claiming to find in the Bible not only allusions to Mohammed, but also many

explicit occurrences of his very name.¹ He was familiar with Syriac (see e.g. E. Tr., pp. 129 ff.) and, according to Mingana, he translated for himself some at least of his biblical citations (see pp. xix ff., and my preceding note). Mingana himself recognised that his source was a form of P', though we cannot determine its text-form specifically.²

Peters pointed out (pp. 292 ff.) that Tab's text is often longer than that of the P' mss, in contrast with which it shows a "targumartige Breite und Freiheit". This he illustrates with a series of examples, e.g. \( \psi 45:5 \) fin:

\[
\begin{align*}
\text{Peters:} & \quad \text{He is familiar with Syriac.} \\
\text{Tab:} & \quad \text{He translated for himself.}
\end{align*}
\]

"for thy law and thy prescriptions are joined with the awe of thy right hand"

and \( \psi 72:11 b, 12 \)

\[
\begin{align*}
\text{Peters:} & \quad \text{He is familiar with Syriac.} \\
\text{Tab:} & \quad \text{He found many passages containing the word.}
\end{align*}
\]

"and all nations shall serve him with obedience and submissiveness, for he shall deliver ....."

---

1. Should the reader wonder how Tab arrived at this conclusion, I would explain that he regarded Ar. \( \text{אֲדֹנָי} \) as equivalent to Syr. \( \text{си} \). This Syriac root is exceedingly common in P', being a "drudge-word" used by the translators to render a great number of Hebrew roots (nearly four columns in Tchern's glossary). So Tab found many passages containing the word, which he rendered \( \text{אֲדֹנָי} \), or containing cognate words such as \( \text{אְדֹנָי} \), which he rendered \( \text{אֲדֹנָי} \) and took as equivalent to \( \text{אֲדֹנָי} \).

2. In quoting \( \psi 45:4 f. \) (E. Tr., p. 89), Tab read twice, like most authorities of both East and West. Le however (and T) have it only once. We cannot therefore make much of Mingana's note that Tab's Vorlage "is more in accordance with the East Syrian version, which repeats twice the world "glory"; the repetition is as much a feature of Western as of Eastern texts."
Such renderings are certainly reminiscent of the translation technique of Targumim, and raise the question: Did Tab use a P’ text which preserved old targumic elements that can no longer be traced in the text of our $\Psi$? If so, Tab becomes a witness of first-rate importance. Or are these expansions due merely to Tab’s own mode of translation? Peters prefers the former explanation, without giving any reason; but there is a simple test whereby we may decide between the two. Tabari also quotes the New Testament. Do we find expansive renderings there too? If so, we shall have to conclude that this was a feature of his own translation, and suffices to explain the expansions in $\Psi$ (and other books of the Old Testament). In fact we do find such renderings:

Mt 4:19

Gk: καὶ ποιήσω δομινικά ἄλεσις ἀνθρώπων

P’:

Tab., p.148 “and I will make you after this day fishers of men”

Mt 4:21

Gk: καὶ ἐκάλεσεν αὐτούς

P’:

Tab., p.148 he called them to his faith

1. I give Mingana’s English translation to which the page number refers. Only the words underlined are shown in Arabic too.
Mt 12:25

Gk Πίσις βασιλεία μερισθεί σαθ' ἐαυτῆς ἐρημοῦται, καὶ πάσα κόλις
η ὀλίγα μερισθεί σαθ' ἐαυτῆς οὐ σταθήσεται.

P' דל מָלְאָה וַתִּדְמוּ לוֹ נְבֵינִית, וַתִּדְמוּ לְנָשִׁים.

Tab., p. 59. Every kingdom which is divided against itself shall perish, and shall not stand (לא תִּקְּנַ),
and every city in which there is disunion and disagreement
shall not last and shall not be firm (לא תִּקְּנַ ולא תִּזְקַּנְּי).

I cite other examples more briefly:

Mt 21:23 = Tab. 150 "By what authority doest thou what we see?" (יָאִישׁ , וַיִּתְּמָה)

Mt 27:40 = Tab. 151 "Come down from the cross, that we may believe in thee" ( > Gk, Syr)

Lk 22:35 = Tab. 143 "Were ye harmed and lacked ye in anything?" ( > Gk, Syr).

Jn 16:13 = Tab. 140 "and will announce to you events and hidden things" (τὰ ἑρξάμενα , אָסִים)

Gal 4:23 = Tab. 144 "but he of the free woman was by promise from God" ( > Gk, Syr).

So Tab did tend to expand his Vorlage, and despite the
"targum-like" quality of his renderings, we have no reason to attribute them to anything beyond his own methods of translation.

The other Arabic author adduced by Peters is Ibn al-Djawzi, cent. xii. Ibn Djawzi too offers three "targum-like" renderings (ψ 45:5 fin., 72:11b, 149:7a - see Peters, pp. 291 f.). He saw no reason to ascribe them to the technique of the Arabic translator, and concluded: "ihre Ursache dürfte vielmehr in einer entsprechend freieren und breiteren Fassung der syrischen Vorlage zu suchen sein."

But we have already seen one Arabic translator producing "targum-like" renderings, and we should not immediately discount the possibility here. In fact it turns out that the quotations from ψ 45:5 and 72:11 are also in Tab, with virtually identical wording (p. 296); this indicates, in my submission, that Ibn Djawzi is dependent there on Tab.

As for ψ 149:7a:

Ibn Dj. (p. 289): "that God take vengeance from the nations who serve him not"

the expansion in this 12th cent. Arabic author admits many explanations, e.g. an accretion to the text between the time of the translation and that of Ibn Dj, an explanatory addition by Ibn Dj himself, and so on. We are far from

1. Peters gives the year of his death as 597 A.H.
2. perhaps mediatey through Ibn Kutayba, if the latter is the immediate source used by Ibn Dj.
having proved that old targumic elements in the Syriac Vorlage are responsible.

Since this chapter was first typed, Dr. S.P. Brock has drawn my attention to another article by Peters, entitled "Arabische Psalmenzitate bei Abu nu'aym", in Biblica (1939), pp. 1-9. Here Peters makes for Abu nu'aym (1038) roughly similar claims to those which he made for Tabari and Ibn Bjawzi. He presents citations of (i) 1.1-3, (ii) 1.1-3, (iii) 34.12-14,16. The text of (i) differs considerably from that of (i), but in all three the translation exhibits a great degree of freedom which is said to be "typically targumic". A.N. also gives, as what purports to be a further quotation, the line:

لا تنتمى من الطائر بالطائر ثم تنتمى من الطائر حبيما

"that I may wreak vengeance on the hypocrite through the hypocrite, whereupon I shall wreak vengeance on all hypocrites"

which Peters could not of course identify as a verse from ס, and regards as a "riddle".

How are we to account for the free renderings evinced in (i)-(iii)? Peters argues that A.N. must have drawn his ס citations from certain old Arabic versions of the Psalter, "deren freie Textgestaltung es nahelegt, ihren Ursprung in der Sphäre targumischer Überlieferung zu suchen" (p.7). Thus Peters invites us to believe that A.N. scrupulously followed an old Arabic version of ס - or rather, in view of the many divergences between (i) and (ii), two different such versions - and that these versions faithfully reproduce various ancient Aramaic/Syriac paraphrases of ס, which have left no other trace. To my mind, however, a likelier explanation is that A.N. had encountered some Psalms in Arabic, but recollected them very imperfectly. Hence the differences between his text and that of all other known authorities are due in the main to his inaccuracy of quotation. Twice he alluded to the same passage (ס 1.1-3), drawing, when memory failed, on imagination, the latter was volatile enough, and was assigned sufficient scope, to yield on the two occasions two substantially different texts. As for "" - which, on Peters' hypothesis, does indeed constitute a riddle - it seems that A.N.'s imagination supplied either its attribution to the Psalms or almost the whole of its text. Hence I submit that the supposition that A.N. has preserved fragments of an old Aramaic/Syriac paraphrase of the Psalter is completely unwarranted.

The field over which one could search for citations going back to the Peshitta Psalter is of course enormous, and only a small part of it has been explored in this chapter. Had we been able to conclude that such an investigation would reveal a good number of ancient P' readings, or evidence of multiple origin of P', etc., it would of course have been interesting to carry it further. Thus Goshen-Gottstein suggests (p. 61, n. 165) that a study of the Syrohexaplar and other later Syriac versions may provide a parallel to "the problem of later Greek versions preserving extra- (or proto-) Septuagint traditions". But our conclusions from this Chapter, hold out little hope of advantage from an extended study. To sum up our results:

(i) We have not a single instance in which the reading of the P' was on the one hand, and that of a citation on the other, can only be explained as going back to two different attempts at rendering ס from Hebrew into Syriac. Thus no cogent evidence is evinced for a "Kubile view" of the origin of the Peshitta Psalter.
(?) I find ro e v i e n c e to s t a t e the i'ψ is the resultant of two or more different translations. Is or are there any grounds for the supposition that the Syrians ever knew any other extensive version made directly from a Hebrew text of ψ? Indeed, it seems that until at least cent. iv, the Syrians virtually always quoted the Psalter according to P'. They did, however, have access to contact's and to literary works in which the Bible was quoted in other forms, and these seem occasionally to have influenced Syriac writers. Thus Aphraates quoted ψ 37 35 according to G', under the influence of a Greek work (so Burkitt), and ψ 119 99 in a form which was current among Jews (though here Aph may have the true P' reading - see 3 below). The Ebhēyā in Ephraim is an extra-Peshitta tradition and is specifically recorded as such, as this authority is referred to only once, it too seems to denote a piece of Jewish tradition, which Ephraim happened to pick up, rather than a complete Syriac version of ψ. Apart from these references, we have found nothing to suggest that any predecessor or early rival of P' - probably until cent. vi, which is the earliest time at which we have evidence that rival versions (all based on G') appeared2 - ever existed. In other books of the Bible, the case may be different. In particular, L. Delekat3 has suggested that we may discern a "Vetus Syra" text for Isaiah, out, I repeat, such traces are absent from the indirect evidence I have examined for the Psalter. . .

(3) Occasionally, the original text of the Peshitta - a term which we may use in view of our first conclusion - may have been lost in the ms text but preserved in a citation. So perhaps in ψ 119 99, 1;11; in the mss, 1;11, in Aph[142 (MT יהבננ); and in ψ 143 10 ἡλικον in mss, κυρια in Eph. Dist. 15·9 (MT κυρια). However, the assertion that a whole layer of early Peshitta renderings, mainly of targumic character, may be recovered from citations, is unjustified. Expansive and free renderings which seem at first sight to bear a strong resemblance to Targumim may be due to other causes. A critical editor of the Peshitta - if we may extrapolate from our results for the Psalter - would

1. Thus we have found no certain instance of either Aphraates or Ephraim being influenced directly by G' (as opposed to being influenced mediately through the L.T. or an early patristic work). Indeed there is little evidence that they were familiar with the text of the G' version even of other O.T. books. To be sure, the Bible commentary attributed to Ephraim occasionally cites the ψ. - a list of the passages concerned is given c. J.Perles, "Elektomata Pechithoniana", Breslau 1899, p.42. - but all these instances appear in parts of the commentary which cannot be accepted, on Burkitt's criterion, as undoubtedly genuine writings of Ephraim.

2. The best-known of these later versions is the Syrohexaplar, the work of Paul of Tella (636/7). However, it is believed to have had two cent. va predecessors. A version of the Psalms and the L.T. is attributed to Polyceph (A.B.508, Duval, p.64), some fragments survive, but nothing of ψ. Also Mar Aba, the Historian Patriarch (596-602), is said to have produced a Bible version (Duval, p.67), of which nothing is extant. Both versions are stated in the Syriac sources to have been based on G', we may perhaps speculate - for we could hardly ever be certain either way - whether some variant readings in or over in mss of the Peshitta Psalter written before 616, and which seeks to betray the influence of G', were taken over from those versions. An example of such a reading is and("א" and, υ') at ψ 29 7, in 40 (and many later mss).

be well advised to use the testimony of Aphraates, the undoubted works of Ephraim, and Barhebraeus. This material is easily accessible and most of it has been critically edited. But a wider examination of Syriac and Arabic literature in search of readings lost in the mss but preserved in quotations, is unlikely to be worth while.

(4) Citations from Aphraates and Ephraim, when they cover a point where the P' mss diverge, may help us to discriminate between rival readings. The rules set up in Chap. 7 are generally confirmed in such passages.

(5) Later authorities can sometimes be associated with certain text-forms, cf. our identification of an "early Monophysite" and a "late Western" group of authorities.

(6) Most important of all, I believe that as the Peshitta Psalter may now be assumed to have been from the start one translation, scholars need no longer feel that it would be somehow naïve to attempt to recover the unique original text of the Peshitta Psalter. The practical difficulties may be considerable; but the aim itself is surely valid.
To suggest a policy for discriminating between variants is only part of our task. We must also supply an outline of the textual history which will explain how the situation presupposed by that policy came about - how, to take a specific instance, there were occasions on which F alone preserved the true reading. This outline should also give us a general idea of the transmission process.

The question of the origin of the Peshitta - whether it is the work of Jews, Christians, etc - is not directly relevant to the study of the text itself. Nevertheless, it is of great interest for its own sake; and as I have found in the course of my study of the text certain features which seem to have some bearing on the problem of its origin, I put forward my findings at this point. I would state immediately that I make these observations tentatively, and purely as a student of the biblical text, not as one of statistics. They are not connected with the methods of textual criticism proposed in this thesis, nor do my choices between rival readings (in Chap. 10 and elsewhere) depend on them.

In what circles, then, did the Peshitta Psalter originate? On this question the last century has seen a remarkable change of opinion. During the nineteenth century, the view was dominant that the Peshitta Psalter was a Christian work. Thus Nöldeke¹ (p. 262): "Manche Stellen zeigen in ihr (sc. the

¹. "Die alttestamentliche Literatur", Leipzig 1898.
Peshitta) eine entscheiden christliche Auffassung, zum Theil in Widerspruch mit allen sonstigen alten Uebersetzungen und in einer Weise, die nicht durch nachtragliche Interpolation erklärt werden kann; namentlich finden sich solche Stellen im syrischen Psalter." (my italics). Unfortunately, Nöldeke did not go on to give any details of these alleged Christian tendencies in the Peshitta Psalter. He was not the first scholar to put the case for Christian authorship of Psalms in particular; this had already been advocated by J. A. Dathe¹, A. Oliver² and J. F. Berg³.

Even at that time there were a few scholars who believed that the Peshitta Psalter was of Jewish origin⁴; but not until the present century did this view — usually in conjunction with an earlier dating of the translation — become predominant. The acquaintance of the translators with Hebrew and with Jewish tradition convinced Burkitt⁵ that the Peshitta of the entire O.T. was the work of Jewish scholars. A. Baumstark⁶ believed

1. Psalterium Syriacum, recensuit et Latine vertit Thomas Erpenius. Notas philologas et criticas addidit Johannes Augustus Dathe. Halle 1768. Dathe believed that the translator was a Christian of Jewish extraction (pp. xxiv f.)
4. such as I. Prager, "De veteris Testamenti versione quam Peshitto vocant quaestiones criticae", Göttingen 1875.
that the evidence tended "den noch jüdischen eher als auch nur jüdisch-christlichen Ursprung der Übersetzung zu erhärten".

Wutz, writing in 1925, assigned the Peshitta Psalter to cent. II B.C. (p. xvi). Peters too, as we have already seen, maintained in his article of 1939 "das der Psalmentext der Peshitta ursprünglich in der aramäischen Targumüberlieferung wurzelt" (p. 283). One scholar who advocated caution was F. Rosenthal, who pointed out (p. 206) that the problems involved in the histories and inter-relations of Bible versions were exceedingly complex. The argument most frequently advanced in favour of Jewish authorship had been that P' and T' showed many parallels; but because there were so many possible currents which could have brought elements of Jewish tradition into any ancient version of the O.T., Rosenthal did not accept that P' had been proved to have grown out of the targumic tradition. But this is, of course, not the same as arguing that P' to any part of the Old Testament is in fact the work of Christians. Indeed, my impression is that neither Rosenthal himself nor any of the successors of Nöldeke whose work he reviews would regard the Peshitta to the Psalter, or to any other Old Testament book, as more likely Christian than Jewish. The position does not seem to have changed in more recent years. Thus we have Kahle's conclusion that "the Syriac translation of the Old Testament is of Jewish origin" (p. 269), and M.H. Goshen-Gottstein's statement (p. 266 n.)


that he knew of no indication against the hypothesis that
the P' of the Old Testament (with the possible exception of
Proverbs) was a Jewish targum.

In order to clarify the discussion which now follows,
I shall state my own conclusion at the outset. I believe
that the hypothesis of the Jewish origin of the Peshitta
Psalter - whatever we may say of other books of the O.T. -
has not been conclusively proved, and that a case can be
made out in favour of Christian origin.

We begin with two propositions with which most modern
scholars, I believe, would agree. First, it can hardly be
doubted that the Syriac Pentateuch is nothing but a Jewish
targum. This has been convincingly shewn in a number of
studies; among the most recent are those of A. Voöbus¹,
J. A. Emerton², and S. R. Isenberg³. Second, the Peshitta
of the rest of the O.T. is not the work of the same translator,
or school of translators, as that of the Pentateuch. Barnes⁴
has demonstrated in particular that the translators of ψ are
characterised by a "dread of anthropomorphisms, of which the
translators of the Pentateuch were free." (p.197) Thus "it
is difficult to believe that the same school of translators

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1. "Peschitta und Targumim des Pentateuchs: Neues Licht
   zur Frage der Herkunft der Peschitta aus dem alt-
   palästinischen Targum", Stockholm 1958 (= Papers of the
   Estonian Theological Society in Exile, ix).
2. "Unclean birds and the origin of the Peshitta", Journal
   of Semitic Studies (1962), pp. 204-211.
3. "On the Jewish-Palestinian origins of the Peshitta to
4. "On the influence of the Septuagint on the Peshitta,
   in JTS (1901), pp. 186-197."
rendered into Syriac both the Law and the Psalter" (p. 187).
It follows that the problem of authorship has to be considered separately for the Peshitta to each book (or group of books) of the Bible. Nevertheless, the general conviction that the Syriac Pentateuch is a Jewish work creates a presumption—and nothing more—that the same is true of other O.T. books.

Most of the arguments adduced in favour of the supposition that the Peshitta Psalter is a Jewish work are based on the presence of characteristics which are usually regarded as Jewish. The Vorlage was a Hebrew text; the translators were acquainted with Jewish tradition, and there are striking parallels between T' and P' to the Psalms; moreover, they show a distaste for anthropomorphisms, as we mentioned above, and this is a well-known characteristic of Targumim. I doubt,

1. The only other argument I have seen in favour of Jewish origin is that of Prager (pp. 47 ff.), which reappeared in B. Oppenheim, "Die syrische Uebersetzung des fünften Buches der Psalmen", Leipzig 1891 (p. 4), viz that the titles of the Psalms in the Peshitta prove that it is a Jewish translation. This can be allowed little weight, in that it is generally acknowledged that the titles—which differ greatly in the different mss and editions—do not go back to the original translation (at least in the form in which they were used by Prager and Oppenheim). See discussion in W. Bloemendaal, "The headings of the Psalms in the East Syrian church", Leyden 1960.

2. Peters lists eleven such agreements (pp. 277 f.), but a much fuller list had already been compiled by Baethgen (p. 448).

3. It is also found in G', but cannot be explained in terms of assimilation to the G' text. Thus in ὑ 84:12 G' and P' treat the figure differently:

MT דִּבְרֵי ה' בְּכֵן יַעֲבָדֶנָּיו
G' ἐλεον καὶ ἡμήρειαν ἐκούσα ὁ Κύριος ὁ θεός
P' ἐκούσα ἡμήρειαν τοῦ θεοῦ, ἀνελεον

Again, in ὑ 31:6, where MT הַיְמָנָה יְקַטְּרַף שְׁתֵּי P' has ...

... וַאֲנַחַת, whereas G' tolerates the figure (εἰς χειράς οὖν).
however, whether the conclusion is inevitable that the translators were Jews; for it is possible to reconcile these Jewish elements with the hypothesis of Christian origin by supposing with Nöeldeke (p.263) that the translators "getaufte Juden waren, welche die fest ausgeprägte exegetische Tradition des Judenthums mit in's Christentum herübernehmen". Although the origins of the Syriac-speaking Church remain uncertain, it seems intrinsically likely that converted Jews formed some proportion of the Christian community in its early stages, so Burkitt, Kahle and more recently Neusner. It is against this background that the Jewish characteristics may be viewed. A community of partially Jewish extraction would retain for some time an acquaintance with Jewish tradition, and - at least for a generation or two - with the Hebrew text of the O.T. Nor would conversion to Christianity banish their distaste for anthropomorphisms. Thus, although

1. This seems preferable to Burkitt's hypothesis that the translation was made by Jews for a Christian community. There are some passages, as I hope to demonstrate, in which the Hebrew has been rendered in a manner which lends itself to Christological interpretation, this can hardly be due to the Jews to whom the translation was supposedly entrusted, no matter how obliging they may have been.

2. "Early Eastern Christianity", London 1904, p.34.

3. op. cit., pp. 260 ff. It would be prudent not to rely as heavily as Kahle does (p.275) on the support which this supposition receives from the Chronicle of Arbela, because the authenticity of that document is now hotly disputed. J.N.Fley, "Auteur et date de la Chronique d'Arbèles" in L'Orient Syrien (1967) pp. 265-302, goes so far as to suspect that it is a modern forgery.


5. To infer Jewish origin from acquaintance with Jewish tradition is particularly dangerous. Thus the fact that P' occasionally renders הֶלִל by מַכָּה (P 3.9, 21:10, 66:7), מַכָּה (P 4.13, 66:4), מַכָּה (P 55:20), in agreement with T' מַכָּה, proves ("sichert") in Peters' opinion (p.279) that the Peshitta Psalter is based on Aramaic Targumim, but by the same token, Jerome's frequent use of semper in H' for מַכָּה would indicate that his version too came from Jewish hands.

6. Some writers have gone so far as to maintain that there were Christian churches which carried on the practice of scriptural readings in Hebrew. The hypothesis was formulated by G. Zuntz, "On the opening sentence of Melito's Paschal Homily", Harvard Theological Review (1943) pp.299-315, who saw in the first sentence of this homily:

   "מִי מְנַרְנִי כִּי לְרַבַּנֵי הָעֵשֶׁבֶּסְכּוֹ שֵׂפֶרֶנוּ אֲנָגָמְכּוּ כְּלֵי הָעַמְנָהְוִּי מְשַׁאָרְלָו בֵּית יְשׁוּעַ אָבֵד..."

   a reference to an account of the Exodus

   in the Hebrew language followed by a "targum" in Greek, perhaps in the form of a reading from the Septuagint version. Though Zuntz has found considerable support, this view seems now to be falling increasingly into discredit. For a detailed discussion, see S.G.Hall, "Melito's Paschal Homilies 1 and 2", in Kyriakon. Festschrift Johannes Quasten (Münster 1970), vol. 1, pp.236-248.

7. Thus when MT applies מִפְּרָט to God, the expression is modified in the (Christian) Vulgate. Cf. R.Loeve, VT (1952), pp.261-272.
we cannot doubt that there was Jewish influence, it has not been proved that the Peshitta Psalter is of Jewish origin.

Let us now examine the other side of the case: Are there any characteristics which point to the Christian origin of the Peshitta Psalter? The task is a delicate one; for we cannot easily tell whether Christian elements go back to the translators themselves, or whether they were introduced in a putative adaptation of a Jewish version to Christian use. I have therefore concentrated on passages in which the Hebrew Vorlage itself seems to have been rendered in a specifically Christian sense.

(1) There are two passages in which the Peshitta takes the Hebrew to refer to a Son. Here P' differs from all other ancient versions, and his interpretation could hardly be associated with a Jewish translator:

(a) 2:12 MT

"Kiss the son, lest he be angry and ye perish from his way".

Not only does P' take as "son", unlike all other ancient versions (G' δριςατε παιδειας, Τ' καταφιλήσατε ἐκλεκτως, c' προσκυνήσατε καθαρός, Anther: ἐπιλάβεσθε ἐκεῖθεν, H' adstrate pure);

P' also has καθαρός, and thereby associates this son with the "way" (perhaps compare John 14:6 'Ἐγώ εἰμι ἡ οὐράνιοι οἶκος ..)
whereas the other versions have:  
G' εξ ἐκδόθη διακόνος,  
T' ἐξ ἐκδόθη διακόνος (ὅπως διήκονος)  
other Gk versions εξ ἐκδόθη, H' de via.

It is hard to imagine how it could have occurred to a Jew to construe the verse in this way, and yet the close correspondence between the Hebrew and the Syriac surely proves that we have here the work of the translator himself.

The earliest Fathers of the Church, relying on G', do not interpret the verse Christologically. However, we read in the Commentarioli in Psalmos attributed to Jerome:

"... in hebraeo legitur nescui bar, quod interpretari potest, Adorate filium. 
A pertissima itaque de Xpisto prophetia est, et ordo praecipi: Adorate Filium, non forte frascatur Dominus, hoc est Pater". 2

This same interpretation reappears in the Authorised Version: "Kiss the Son".

Thus P' renders נְבָרַדְיָא אֲשֶׁר מֹשַׁחְתָּר לְחָזָק הָיָיוֹת (P1) by נְבָרַדְיָא, unlike the other versions (G' omits, T' has, for the last three words, נְבָרַדְיָא אֲשֶׁר מֹשַׁחְתָּר לְחָזָק הָיָיוֹת; a'σ'τ' etc.δρός; H' ῥως). P' moreover adopts the vocalisation νικήθησα (so G'; but Tἀ'σ'θ'H' = MT), as in μ 2:7, with which this passage was apparently associated. The Christian colouring of the P' rendering needs no pointing out, see Dathe (p. xxv) and Oliver (p. vii). Once again, the resemblance between P' and MT means that we must see here the work of the translator, not a later reviser.

It is certainly noteworthy that this rendering occurs within a Psalm which Christians have interpreted from the earliest times to refer to Christ. The N.T. applies to Christ both the first3 and fourth4 verses, on several occasions, and the Psalm is constantly employed in the same way by early Christian writers5—though once again they were precluded by their dependence on G' from arriving at P's interpretation of V.3.

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2. This and other references to parallel interpretations in early Christian writers, of whom I claim no specialised knowledge, are based on whatever I have been able to assemble; no doubt others will in many cases be able to cite more appropriate examples.
3. e.g. Mt 22 44, Acts 2.34 f, Heb. 1:13
4. e.g. Heb 5:6, 7:17.
(2) The translator seems to have taken a number of opportunities to introduce the idea of election. This is not a particular characteristic of Targumim, which take the choice of Israel more or less for granted; but it is of course an important theme in the New Testament.

I noticed first that the Hebrew יְהַנֵּן was sometimes rendered in $P'$ by "elect". According to P-S coll. 636 f., the word can mean "pure" (occasionally to render יֵשָּׁב or יַעֲדֵל) or "outstanding"; but its usual meaning is "chosen, elect". The last seems to be the sense of $P'$ in two passages in particular, against all the other versions:

(a) $\psi 31:22$  

ךָּוִּי יְהַנֵּן וְיֵשָּׁבָה לָוָא לָהוֹ  

$P'$  

"Blessed by the Lord, who chose (יְהַנֵּן) the elect (יְהַנֵּן) unto himself"

(b) $\psi 32:6$  

כֶּלֶכֶל יְהַנֵּן כָּל-חָסֵיד אֲוִיר לָוָא מֵא  

"Therefore every one that is chosen by thee shall pray unto thee at an acceptable time".

We also find יְהַנֵּן "his elect" for יְהַנֵּן in $\psi 30:5$, and for יְהַנֵּן in $\psi 50:5$. In $\psi 4:4$ we find the singular יְהַנָּה.
MT

know ye that the Lord hath wondrously separated the Chosen unto him

wondrously separated the Chosen ne unto him"

in which we may suspect that the כִּי יָאִיר כָּלַהְתָּה חוֹלָ֑ת לִי is intended as none other than Christ himself. Compare the treatment of Augustine (Expositions on the Psalms ad. loc.): "Et scitote quoniam admirabilem fecit Dominus sanctum suum. Quem, nisi eum quem suscitavit ab inferis, et in caelo ad dexteram collocavit?"

It is noteworthy that the rendering כַּאֲשֶׁר for דִּבֵּר does not occur in any other Book of the O.T. The renderings which we find in P' for דִּבֵּר are:

<table>
<thead>
<tr>
<th>Passage</th>
<th>Numeral</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ps 11:1</td>
<td>15 times in (see Techen, p.151); 2 Chr 6:41</td>
</tr>
<tr>
<td>Ps 16:10</td>
<td>18:26; Dt 33:8, 1 Sam 2:9, Mic 7:12</td>
</tr>
<tr>
<td>Ps 12:2</td>
<td>66:2; 2 Sam 22:26, Jer 3:12</td>
</tr>
<tr>
<td>Ps 43:1</td>
<td>145:7</td>
</tr>
</tbody>
</table>

Hence it appears that even in דִּבֵּר, דִּבֵּר is not rendered by כַּאֲשֶׁר by any means exclusively.

Furthermore, in Ps 47:5, P' does not take MT כִּי יָאִיר כָּלַהְתָּה חוֹלָ֑ת לִי to mean "God hath chosen an inheritance for us" (so G' כִּי יָאִיר, T' כִּי יָאִיר, H' כִּי יָאִיר); instead he renders כִּי יָאִיר כָּלַהְתָּה חוֹלָ֑ת L' כִּי יָאִיר כָּלַהְתָּה חוֹלָ֑ת "he hath chosen us as his inheritance". In Ps 68:20, MT כִּי יָאִיר כָּלַהְתָּה חוֹלָ֑ת, with כִּי יָאִיר not found elsewhere in Psalms, is also rendered כִּי יָאִיר כָּלַהְתָּה חוֹלָ֑ת (but G' כִּי יָאִיר כָּלַהְתָּה חוֹלָ֑ת, T' כִּי יָאִיר כָּלַהְתָּה חוֹלָ֑ת, H' portabils nos). It is because this rendering is apparently a guess that I attach particular weight to it in showing that the translator was preoccupied with the idea of election; it is especially when a man has to guess that he reveals what is in his mind. There are, interestingly, two more passages from the same Ps 68 which we shall all have occasion to discuss below.

1. A.D. 354-430.
Yet when the choice of Israel in former times is brought out in MT, P' is rather less enthusiastic. In יִתְנֶאֶר צֶּרֶךְ בִּשְׂרֹת אֶדֶר-בְּכִירֵי

P' does not render the last word as 'chosen' (G' ἐλεκτοσία, T' בחרו, H' electors), but by פִּזְצַרמֶן

Again, in י 135:4:

P' makes relatively little of the word נִבְלָה (not found elsewhere in the Psalter), which is associated in particular (Ex. 19:5) with the election of Israel; he writes נִבְלָתֵל פֶּתֶר נִבְלָת לְגוֹיִם ("and Israel for his congregation"). With this we may contrast not only the other versions in this passage (G' θ' ελκ περιοντιαῖαν αὑτῶν, T' διὰ την προφητικήν, H' in peculum suum, "εἰς ἔξωτον") but also the Peshitta renderings of the word whenever it occurs in the Pentateuch:

Ex 19:5 דְֹאָס תָּלִים לְגוֹיִם נִבְלָת לְגוֹיִם דְֹאָס לְגוֹיִם בְּכִירֵי לְגוֹיִם

Deut 7:6, 14:2, 26:18

1. But in Mal 3:17, we find דְֹאָס.
(3) The word נָאִיר "annunciation", which occurs twice in the Leshitta Psalter, lends itself in both passages to Christian interpretation. There seems to be no parallel among Jewish writings to the meaning "tidings, annunciation" for any derivative of נָאִיר (e.g. Heb. נָאִיר, Aram. נָאִיר); see the Dictionaries of Ben-Yehuda and Levy. The places in י where נָאִיר occurs are:

(a) י' 19:5 ] MT 
P' יִנְבִּין עֲרָפָתָא נָאִיר
"in all the world hath their annunciation gone forth".

Perhaps P' read מְלָא הַעָרָפָת (so G' מְלָא הַעָרָפָת, כ' מְלָא הַעָרָפָת, H' מְלָא הַעָרָפָת) but was reluctant to repeat מְלָא (cf. V.4) and substituted this more specific term, perhaps P' took מְלָא as a noun from מְלָא הַעָרָפָת, and instead of מְלָא מְלָא "hope" (used for מְלָא הַעָרָפָת in י 9:19, 71:5) made the change to מְלָא מְלָא. Either way, it seems that the Hebrew itself was interpreted on Christian lines.

It is interesting that some early Christian writers who depended on G' interpreted the verse in a similar fashion. Paul cites it (Rom. 10:18) as proof that those who fail to accept the tidings of salvation through Christ, cannot plead that they never heard them, for "their sound went into all the earth". Later we find the verse frequently applied to the world-wide dissemination of the Christian faith. One example which I find of particular interest is due to Irenaeus (c.130-c.200), who combines it with Gen. 9:27 as a prediction of the conversion of the gentiles:

"But the blessing of Japheth was as follows: «May God enlarge Japheth, and let him dwell in the house of Sem, and Cham be his servant»; and this blossomed forth in the end of this age, in the manifestation of the Lord to the Gentiles of the calling, when God extended to them His call, and «their sound went forth into all the earth, and their words unto the ends of the world». So "enlarge" refers to the calling from the Gentiles, that is to say, the Church, and he "who dwells in the house of Sem", that is to say, in the heritage of the patriarchs, is Christ Jesus receiving the birthright".

Another example is the passage from Origen cited below.

(b) י' 68:12 ] MT יִנְבִּין עֲרָפָתָא נָאִיר
P' יִנְבִּין עֲרָפָתָא נָאִיר "The Lord will give the word of the annunciation (or Gospel) with great might".

2. Demonstratio §21. As the work survives in an Armenian version only, I quote the English translation of J.P. Smith ("Ancient Christian Writers 16"), London 1952, p.60. Another interesting treatment of this verse is found in Clement of Alexandria, Paed. II 8 init.
P' is alone in making an abstract noun of \( \text{προφηταις} \) (G' τοῖς εὐangelizomenois, T' ἀναλας ἀλλα ἀλλα, H' εὐangelizomenen) [στρατηγὸς κολλη], T' ἀδυνατιατρικοί). Once again it is difficult to understand what motive a Jew would have had for translating thus, but in a community which had but recently received its conversion to Christianity, such a rendering is quite natural.

For this verse too we find Christological interpretations of the G' version among the Fathers of the West. Augustine (op. cit.) comments ad loc.: " Καὶ ἐξ ἑκατέρου ἐκ τῶν ἐν τοῖς προφηταῖς, προγνωστικῶς ἀπογγέλλουσιν περὶ τῆς κηρύξεως τοῦ εὐangelευλογου, "Κύριος δότη "ρημα τοῖς εὐangelizomenois δυνάμει κολλη, δ βασιλείας τῶν δυνάμεων τοῦ ᾿Αγαπτοῦ", ἵνα καὶ ἢ λέγουσα προφητεία: "ὅπως τάχους δραμεῖται δ λόγος αὐτοῦ (§ 147:4) κληροδύναμιν, καὶ πλεκομένη γε διὶ "εἰς πάνω τὴν γῆν ἐξηρέθην δ" τῶν ἀποστόλων Ἰησοῦ "φθόγγος, καὶ εἰς τὰ πέρατα τῆς οἰκουμένης τὰ ῥήματα αὐτῶν".

(4) The word "saviour" (καταλύει) is found in P' in many passages where MT has an abstract noun from \( \text{Ἀσωτία} \) (Ἀσωτία, Ἀσωτία, Ἀσωτία) which should properly be rendered "salvation". This phenomenon occurs in fact in all the ancient versions, but far less so in those which are undoubtedly Jewish (G' T' \( \text{מ} \)) than in the Psalters of Jerome (V', H') and in the Monte Cassino Psalter (C')\(^4\), which come from the hands of Christians. Let us therefore first

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1. The root לָשׁוּ in this sense is always rendered by \( \text{ἐσωθινός} \) in the O.T., but the change from concrete to abstract is never found again: thus in Is 40:9, P' has \( \text{ἐσωθινός} \) for \( \text{לָשׁוּ} \)
2. c. 155 - c. 254.
4. ed. Dom Amelli, Rome 1912 [ = Collectanea Biblica Latina 1]
consider the other versions, and then try to see where P' fits in.

Out of eighty-odd passages where one of these abstract nouns occurs, T' uses ἡρῴδα virtually throughout; only in three passages is the nomen agentis (ἡρῴδα) found (י 55:3; 118:14, 21). G' uses σωτηρία and σωτηρίου with roughly equal frequency, but the nom. ag. σωτήρ occurs 9 times (י 24:5; 25:5; 27:1, 9; 62:3, 7; 65:6; 79:9; 95:1). 1

What remains of the minor Greek versions indicates that they too usually kept the abstract form. Aquila has nom. ag. (σωτήρ) only once (י 27:9, agreeing with G') out of 8 passages where his reading has survived; Symmachus too only once (י 25:5, again as G'), in 18 passages. Theodotion's translation I have seen only in י 18:36; 22:2; 42:12 and 106:4, in all of which he has σωτηρία. 2

How do the Christian versions compare? Mr. Loewe was the first to draw my attention to the renderings of Ḥ', which put a Christian interpretation on these nouns in several ways. In five passages, Jerome in Ḥ' uses the very name Jesus (י 51:14; 79:9; 85:5; 95:1 – all for ייוו and ייוו, for ייוו); in three he has salvator (י 25:5; 27:9 – in both as G' – and 65:6). Frequently he has salus, which is not of course tendentious; but in several other passages he prefers to use salutare, which cannot be distinguished in the gen. dat. and abl. sing. from the adjective salutaris.

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1. W. Flashar points out, in ZAW (1912) pp. 164 ff., a particular reason which will account for G's relatively high total. In each of these nine passages, G' has ὁ Θεὸς ὁ σωτήρ or the like. We know that the title σωτηρ was applied to the gods worshipped by the heathens amongst whom the Jewish translators dwelt; Flashar cites a number of inscriptions to substantiate this. He therefore suggests, to my mind convincingly, that G' used σωτήρ in these passages out of polemical motives, in order to contrast the so-called θεὸς σωτηριας with the true θεὸς σωτήρ. This is then a special factor, which would presumably not affect other versions not derived from G'. Hence a total of 9 occurrences of "saviour" for an abstract noun "salvation" within the Psalter, must be considered abnormally high for a Jewish version.

2. But in י 18:36, σωτήρ for ייוו occurs in the version which Field denotes S'.

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already in Jerome's time a title of Christ. In this way we find twelve places (ψ 9:15; 12:6; 13:6; 20:6; 21:2,6; 24:5; 27:1; 69:14; [78:22]; 106:4; 132:16) where Christian references have been insinuated in the text but we cannot accuse Jerome of having actually departed from the Hebrew Vorlage, e.g. ψ 9:15 exultabo in salutari tuo for MT בְּקַנְיָשִׁים הָאָדָם

In his Gallican Psalter (V'), Jerome does not introduce Iesus. He uses salvator only where G' has σωτής (ψ 24:5; 25:5; 27:9; 62:7) and, as an alternative, the masc. salutaris (ψ 62:3; 65:6; 79:9; 95:1). But here too he uses salutare in the gen. dat. or abl. sing. in 11 passages where G' has σωτής or σωτής (ψ 9:15; 12:6; 13:6; 20:6; 21:8; 35:9; 51:14; [78:22]; 106:4; 116:13; 132:16). Thus he ended up with 19 passages in V' - and 20 in H' - where MT had an abstract noun, and his translation an expression which was open to the interpretation "saviour".

The origins of G' are uncertain; it is stated in the Preface (pp. xiii f.) that we can hardly tell whether it was derived from a Greek or a Hebrew source. I have not examined

1. Cf. Lactantius iv 12: "... Jesus, qui latine dicitur salutaris, sive salvator; quia cunctis gentibus salutifer venit."

2. The only other place where G' has σωτής is ψ 27:1, and here V' has salus. Rahlfs notes that an abstract noun is found in a few other authorities; perhaps σωτής or the like stood in Jerome's Greek Vorlage.

3. Both in H' and in V' the mss vary here between salutari and salutare.
it in detail, but I note five passages where C' has the masc. salutaris in the acc. case, where MT and G' have abstract nouns (ψ 62:8; 67:3; 71:15; 91:16; 119:41). As an example, we take ψ 67(66):3 "ut sciant in terra vias tuas in omnibus gentibus salutarem tuum."

Among the versions just considered, the substitution of "saviour" for "salvation" is far less common among those that are undoubtedly Jewish than among the Christian ones. In addition to the difference in frequency, there is a difference in character. In none of the Jewish versions is there any suggestion that the saviour is not to be utterly identified with God himself, e.g. ψ 79:9 in G'; ἐπολειτύκον θημάς, δ' θεός δ' ἐστίν θημάς (הפה 'הנלא) ; the change falls within the limits of the freedom which a translator will usually allow himself. But in the Christian translations, such expressions as "exultabo in salutari tuo" ("in thy saviour") go much farther and suggest that the saviour is in a sense separate.

Now P' uses אֵל for an abstract noun in 20 passages:

<table>
<thead>
<tr>
<th>REF</th>
<th>MT</th>
<th>PESHITTA</th>
</tr>
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<tbody>
<tr>
<td>18:47</td>
<td>רֵעָו בַּאֲלִי בְּשָׁעִי</td>
<td>מְמַסְכָּב מַשָּׁאִי (ח. ב')</td>
</tr>
<tr>
<td>24:5</td>
<td>יְרֵצָה לִפְלֶיהֶנֶם בִּנְתִי</td>
<td>פֶּסְלִים אֵין נְאֵבָמ (ח. ב')</td>
</tr>
<tr>
<td>25:5</td>
<td>וְהָיָה כֹּל הָעֵץ הַקָּדָם כֹּל הַגָּזֶר</td>
<td>מַמְסֹכָה כֹּל הַגָּזֶר (ח. ב')</td>
</tr>
</tbody>
</table>

1. for so the expression would be most naturally understood, rather than 'in thy salvation'.
<table>
<thead>
<tr>
<th>REF</th>
<th>MT</th>
<th>PESHITTA</th>
</tr>
</thead>
<tbody>
<tr>
<td>27:9</td>
<td>ראתת-תִּנְבָּכַנְהָא -eker שֶׁלַי</td>
<td>ָהָא -eker מַלְאַנְסָא שֶׁלַי</td>
</tr>
<tr>
<td>35:3</td>
<td>ערַגֹּלְניָא יָשְׁרָתָה יָנָג</td>
<td>יָשָׁרְתָה בַּתְיָירָמְן קָרָתָה</td>
</tr>
<tr>
<td>37:39</td>
<td>ִּזְרַעֲתָה הַתִּירִיתָלָמְבָּי</td>
<td>ָהָיָא זָרָעַתָה הָעָלָמְבָּי</td>
</tr>
<tr>
<td>42:6</td>
<td>ַּזָּרָעַתָה שֶּׁזָּרָעַתָה בֵּּנְגַרְגָּר</td>
<td>'רַעֲתָה בַּבּּיָרְקָרְקָר</td>
</tr>
<tr>
<td>42:12, 43:5</td>
<td>ֵּתְּהָא -ker בֵּּיָרְקָרְקָר</td>
<td>ָוַיָּחָא -ker בֵּּיָרְקָרְקָר</td>
</tr>
<tr>
<td>62:3, 7</td>
<td>ַּרָאָה הָא -ker יִנְגָּרְקָרְקָר</td>
<td>ָוַיָּחָא הָא -ker יִנְגָּרְקָרְקָר</td>
</tr>
<tr>
<td>65:6</td>
<td>ַּתְּפָנָה אֲלָהָא -ker שֶּׁנְגָּרְקָר</td>
<td>ָוַיָּחָא תְפָנָה אֲלָהָא -ker שֶּׁנְגָּרְקָר</td>
</tr>
<tr>
<td>68:20</td>
<td>ַּתְּרָעַתָה שֶּׁרְקָרְקָר</td>
<td>ָוַיָּחָא לָרָעַתָה שֶּׁרְקָרְקָר</td>
</tr>
<tr>
<td>79:9</td>
<td>ַּתְּרָעַתָה שֶּׁרְקָרְקָר</td>
<td>ָוַיָּחָא לָרָעַתָה שֶּׁרְקָרְקָר</td>
</tr>
<tr>
<td>85:5</td>
<td>ַּרָעַתָה אֲלָהָא -ker שֶּׁנְגָּרְקָר</td>
<td>ָוַיָּחָא רָעַתָה אֲלָהָא -ker שֶּׁנְגָּרְקָר</td>
</tr>
<tr>
<td>89:27</td>
<td>ַּרָעַתָה אֲלָהָא -ker שֶּׁנְגָּרְקָר</td>
<td>ָוַיָּחָא רָעַתָה אֲלָהָא -ker שֶּׁנְגָּרְקָר</td>
</tr>
<tr>
<td>95:1</td>
<td>ַּרָעַתָה אֲלָהָא -ker שֶּׁנְגָּרְקָר</td>
<td>ָוַיָּחָא רָעַתָה אֲלָהָא -ker שֶּׁנְגָּרְקָר</td>
</tr>
<tr>
<td>118:14</td>
<td>ַּרָעַתָה אֲלָהָא -ker שֶּׁנְגָּרְקָר</td>
<td>ָוַיָּחָא רָעַתָה אֲלָהָא -ker שֶּׁנְגָּרְקָר</td>
</tr>
<tr>
<td>118:21</td>
<td>ַּרָעַתָה אֲלָהָא -ker שֶּׁנְגָּרְקָר</td>
<td>ָוַיָּחָא רָעַתָה אֲלָהָא -ker שֶּׁנְגָּרְקָר</td>
</tr>
<tr>
<td>140:8</td>
<td>ַּרָעַתָה אֲלָהָא -ker שֶּׁנְגָּרְקָר</td>
<td>ָוַיָּחָא רָעַתָה אֲלָהָא -ker שֶּׁנְגָּרְקָר</td>
</tr>
</tbody>
</table>
As far as the number of changes is concerned, P' is comparable with Jerome's versions. The changes themselves are less bold, but perhaps the phrase "my God and my Saviour" in 18:47, 25.5; 27.9, 62:3,7, 89.27, by including "and" (which is absent from the other versions), tends to distinguish God and Saviour and is thereby tendentious.

As it is by way of passing insinuation rather than formal exegesis that 'saviour' has been substituted for 'salvation', there would be little point in discussing whatever Christological interpretations may have been devised for each of the passages concerned. We cannot however pass over without remark the fact that one of these passages falls within 68, bringing the total number of arguably Christian references in that Psalm to three, namely Flection (V.20), Annunciation (V.12) and the Saviour (V.20 again). Now 68 is among the Psalms most frequently interpreted in a Christological fashion. Eph. 4:18 applies V.18ab to the Ascension; a more elaborate interpretation on similar lines, taking in V.17 as well, is offered by Irenaeus, Dem 883. Regarding the earlier part of the Psalm, Cyprian (Test. II 28) sees in VV. 2-8 a prediction of Christ's second coming as Judge. That these three features should appear in the P' version of this particular Psalm is hardly a coincidence.

We also find ḫcoln inserted without anything corresponding in MT or other versions in 4:2: The same word renders ḫcoln in 18:19 (but G' יֵעָשָׁתָא , T' יֵעָשָׁתָא , V' protector, H' firmamentum). Another rendering open to Christian interpretation is in 17.7.

<table>
<thead>
<tr>
<th>MT</th>
<th>F'</th>
</tr>
</thead>
<tbody>
<tr>
<td>יֵעָשָׁתָא</td>
<td>יֵעָשָׁתָא יֵעָשָׁתָא יֵעָשָׁתָא</td>
</tr>
</tbody>
</table>

Here the mss disagree regarding the presence of Waw. If ḫcoln is original, then it is hard to avoid seeing a Christian reference: "and make thy holy one (= photoshop) a wonder and a saviour of those that hope", this argument would lose much of its force, however, if we preferred to read อลี. On these two rival readings, see pp. 10:8 f.

Reviewing this material, we are justified in stating that this treatment of ḫcoln is significantly reminiscent of the Christian rather than Jewish versions, and constitutes additional evidence in favour of the hypothesis that the Peshitta Psalter is of Christian origin.

1. Daniełou, pp. 283 f.
(5) In one passage, P' seems hostile to Jerusalem:

ψ 48:14 \[ MT \] 


\[ \text{P'} \] 


J.F. Schleusner translates "periculum facite an evertere possitis palatia eius", and similarly Oliver. However, this can hardly have been the intention of the translator; we must understand P' to mean "uproot her citadels (sc. of Jerusalem)."

The Hebrew \( \sqrt{\text{עַלֶּכֶה}} \) is \( \text{אָרְכֶּה} \). BDB record that it is used in later Hebrew and Aramaic to mean "split, cut". The other versions have: G' \( \text{kataleleqese} \), \( \text{Jp09} \) suggests a destructive act.

It is of course possible that later interpolation was responsible, but as (a) all the mss agree and (b) we can

1. Lexicon in LXX, Glasgow 1822.
2. e.g. Jer. Targ. on Gen. 15:10 (Heb. \( \sqrt{\text{עַלָּכֶה}} \)).
3. G' is difficult. The simpler \( \text{diamphsa} \) can mean "destroy" (e.g. in Thuc. 4.48), but the lexica do not record such a meaning for the further compound \( \text{kataleleqoc} \), which means "divide, distinguish". As that meaning was followed by V' and Syrohex. (\( \sqrt{\text{עַלֶּכֶה}} \)), I imagine that it was intended by G' - even if it did not make good sense in the context - and I therefore doubt whether it can be said that P' was following G' in rendering thus.
understand the etymology which led to this rendering
\((\text{viz } \text{lOE} \rightarrow \text{mod})\), it seems likely that here
we have the original translation. On theological
grounds it could hardly have come from a Jew. Indeed, it
is hardly to be expected from a Christian, in view of the
references to the new Jerusalem in Revelation. However, at that time
Revelation was not included in the canon of the Syrians,
and there is little evidence that the name Jerusalem aroused
among them any feeling of reverence. They seem to have
thought rather of "Jerusalem, thou that slayest the prophets",
and with that the rendering of P here is consistent.¹

(6) As supplementary material I adduce three passages
wherein P varies grammatical morphemes in MT, so as to
introduce a reference to the Resurrection. In none of these
departures is P supported by any other ancient version.
This could of course be either Jewish or Christian; and
these places are cited, not on the grounds that they tend
to prove the hypothesis of Christian authorship, but because
of the interest attaching to them if that hypothesis be
accepted:

\[
\begin{array}{c}
\text{MT} \\
\text{P'} \\
\end{array}
\]

"Endeavour continually that thou mayest
live for evermore".

¹ Is this perhaps reminiscent of Christ's reply to the Jews who had asked
Him for a sign: Λῦσατε τὸν ἱματὶ τῶν τοιῶν (John 2:19)? Compare Mt 26:61, 27:40,
and Mk 14:58. Note moreover how the destruction of Jerusalem is
contemplated without any expression of regret, by Tertullian,
Adv. Judaeos, Ch. VIII.
Several N.T. passages may be compared herewith, e.g. Rom. 6:9, 1 Cor. 15:25-27.

If this evidence be accepted as proof that the Peshitta Psalter is the work of Christians, there are some interesting implications. First, it is likely that the Jewish community which was to embrace Christianity possessed a Syriac translation of the Pentateuch, but not of the Psalter. There is nothing inherently improbable in this. In the synagogue, the Pentateuch and the Psalter enjoyed different roles. The former was recited by a single reader to a congregation, which would be acutely conscious of any failure to comprehend the weekly lectionary. The Psalms, however - or rather as many of them as were used in the Service - were usually said.

1. According to Berg (p.26), the first part of יִתְנֶה כָּלָה has a Christian flavour; but I cannot see why.

2. I prefer this to the hypothesis that there was an older Jewish translation, which was replaced. The experience of Jerome shows how difficult it is to supplant an established Psalter; and were that the case, we would have expected to find in our P' mss or in patristic works, many traces of the older version. Yet we have stressed (pp. 7:1f), that such traces are altogether lacking.

3. Thus in many Jewish communities today, only about half the Psalms are ever used in public worship.
by each worshipper to himself. (They were also recited, no doubt, in private meditation.) Once the knowledge of Hebrew began to wane, it was a far more pressing need to translate the Pentateuch than the Psalter. Thus, in the cases of G' and T', a translation of the Pentateuch preceded, by all accounts, that of the Psalms. It would follow that at the time of their evangelisation, this Jewish community had translated the Pentateuch but had not felt the necessity for a Syriac Psalter.

Second, we must now assign the Peshitta Psalter to a later date than has been thought of in recent studies. It is in the second half of cent. ii A.D. that we meet the earliest Syriac-speaking Christians on whom our information is relatively reliable; Tatian, the author of the Diatessaron (cf Kahle, p. 284) and Bardesanes (b. 154). The establishment of a Christian community which spoke Syriac, and hence their translation of the Psalter, can hardly be earlier than cent ii. On the other hand, the translation can hardly be later than that century. Burkitt has shewn (pp 202 f.) that the text of the Old Syriac Gospels has been influenced by the Old Testament Peshitta, and he gives two examples wherein this influence was exerted by the Peshitta Psalter. One passage is particularly impressive: in Mt 13:35 (= 78:2), the Greek has ἀπὸ καταβολῆς (many authorities add χόρμου).

1. According to Vogel (p. 32n.), datings this century vary from cent. ii B.C. (Wutz) to cent. i a.D. (Baumstark and others).
But Sin. Cur. have כותב, like P in 17:2, where MT has רֵדֵע. This reading has surely come from the Peshitta Psalter. As Burkitt plausibly dates the Old Syriac Gospels about 200 A.D., it follows that the Peshitta Psalter must be a product of the second century A.D.

Let us now turn to the subsequent history of the text. We need not dwell on the point that the subject remains a very difficult one, as Barnes said, and that we can hope only to discover some guide-lines in the complex process of transmission. What follows does not set out to give a detailed account, such as can be found in the studies of Emerton on Wisdom and Dirksen on Judges, of the multifarious configurations of agreement and disagreement among the mss that survive today. The emphasis is rather on the general historical processes, from the earliest times onward, that have moulded the tradition. Naturally, no more can be offered here than the barest sketch, and that very tentatively. However, I have thought it wearisome continually to insert qualifying phrases, and so I apologise in advance for any appearance of dogmatism.

One of the most striking features of the tradition is that there are a number of inner-Syriac corruptions common to all the witnesses reported by Barnes. These corruptions have been pointed out by scholars over the years. The earliest

1. The agreement of both ancient mss suggests that it goes back to the time when the Evangelion da-Mepharreshe originated, rather than being a subsequent assimilation to the ψ text.

2. This was also the view of Nöldeke, but he applied it to the entire O.T. (p.264). I speak here only of ψ.
convincing emendation known to me is due to G. H. Bernstein, who proposed אֲשֵׁרָה נֲשָׁרָה in יִשְׁפַּע 20 for the obscure אֲשֵׁרָה נֲשָׁרָה of the mss (MT כִּמָּהּ פּוֹכֵי). Vogel has collected many others (pp. 200 ff), and added several of his own (pp. 202 ff). A few more are put forward in Thes., Ch. 10.

These errors - which are common, as far as we can judge, to the whole tradition - can be explained in terms of the work of pp. A:52 ff. There it was shewn that even in a most contaminated textual tradition, we may find that there are many errors common to all the mss; the reason being that all our mss are to some extent derived from one particular ms other than (and therefore a descendant of) the original, and that this common source contained several scribal errors. This ms on which all the mss are (perhaps only partially) dependent, is customarily styled the archetype; some may prefer Pasquali's term "capostipite". We recall that in order to believe in the existence of an archetype, we do not have to assume that certain unknown editors arbitrarily took one particular ms as the standard, to be copied scrupulously, with all its peculiarities and errors - however blatant these may be - to the exclusion of any other text. Nor need we suppose that the number of mss dropped to only one at some point after the translation was made. Our experiments shewed the emergence of an archetype to be an inherent feature of the birth-and-death process of ms propagation.

We further considered (pp. A:57 ff) the question of the dating of the archetype, and found that it was likely to have

been in existence at the time of the last crisis in the
history of the tradition, i.e. the latest period in which
the tradition was in danger of becoming extinct. In the
case of the Syriac Psalter, which must have become established
quickly, the only possible crisis point could surely be shortly
after the translation was made. Thus the archetype may be
assigned to a time shortly after that of the translation itself.
It is possible that this archetype, with its many errors, was
a direct copy of the original translation, transcribed under
what were, for some reason at which we can only guess, rather
unfavourable conditions.

Virtually any reading which is found in all our ms
authorities, then, goes back to the archetype, if not to the
Urtext. This brings us to a well-known controversy, on the
numerous agreements of P' with G'. Are these due to (a)
consultation of G' by the translators of P' themselves, or
(b) subsequent introduction of material from G'? The former
view was favoured by Baethgen, for two reasons (pp. 445 f.).
The text of Aphraates, who quotes the Psalter extensively, is
virtually identical with our own, so that a supposed revision
after G' must have taken place before 330 A.D.; and we may doubt
whether the Syrians of that early period had either time or
inclination for such text-critical activity. Secondly, P'
sometimes gives only the general sense of G', rather than an
exact translation, which a later interpolator would surely
have provided. Both arguments reappear in Barnes (JTS 1901),

1. "Ein Interpolator hätte sicher die Worte der G' einfach
übersetzt, nicht aber nur ihren allgemeinen Sinn wie er-
gegeben, wie es in den Peschittapsalmen tatsächlich
mehrfach der Fall ist."
who had of course made a special study of the mss. "The Syriac translators themselves seem to have been affected [by G'], for any text critically constructed from the earliest Eastern and Western mss must show some signs not to be mistaken of the influence of the Greek version" (p. 187). Moreover, "...the influence of the Septuagint frequently takes effect on the ideas or the manner of the Syriac translators rather than on their words" (p. 189). A particular feature of the Peshitta Psalter which Barnes attributes to this influence is the avoidance of anthropomorphisms. Vogel, however, whose investigation of this topic is by far the most extensive and thorough yet published, inclines to the opposite conclusion, without however being dogmatic\(^1\); for reasons which we shall consider presently, he prefers to believe that the influence of G' is later than the translation itself.

Two points which we have just proposed, viz (a) that the Peshitta Psalter is of Christian origin, and (b) that little time elapsed from the point at which the translation was first introduced until the archetype came into existence, affect the balance of probabilities in this delicate question. With regard to (a): If the translators were Christians, it is not unlikely that they came into contact with their Greek-speaking brethren, in Antioch and elsewhere. There were doubtless many Syriac-speaking Christians who, like Tatian, visited the West; and such men may well have brought back to the East mss

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1. "Ob die Frage der Verhältnisses der [P'] zu G' jemals endgültig gelöst werden kann, bleibt dahingestellt" (p. 502).
of the Septuagint. These could have been consulted by the translators where the Hebrew was obscure\(^1\), and sporadically elsewhere. "The Syriac translators must indeed have known that their own knowledge of Hebrew was far in advance of the knowledge possessed by the Seventy, and yet the stress of Greek fashion had its way now and again"\(^2\). On the other hand, (b) points in the same direction; for the shorter the time interval within which most of these changes must be supposed to have occurred, the less likely it is that that interval witnessed sufficient text-critical activity to explain all these assimilations to G'. Thus both (a) and (b) combine to favour the supposition that most of the agreements between G' and P' are due to the influence of G' on the translation.

As we have already noted, the opinion of Vogel, which cannot be lightly dismissed, is contrary, yet Vogel's arguments do not seem to me to prove his case conclusively. Let us examine the grounds on which he attributes the influence of G' on P' to revision rather than consultation of G' by the P' translator. He adduces in favour of his view:

1) the fact that the influence of G' is only sporadic; had G' been available to the translator, argues Vogel (p. 501), it would have been worked in more thoroughly. But this argument is double-edged: one could contend equally well that

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any reviser who had determined to assimilate the P' text to G', would have brought the text of P' far more closely into line with that of G' than it in fact is. In favour of our hypothesis that it was the translator who appealed to G' in such a sporadic fashion, we may cite two partial analogies. One is furnished by the Vulgate, which, like P', agrees sporadically with G' in most O.T. books. Jerome was translating from the Hebrew, but occasionally used - or was influenced by - the Greek, as he himself tells us in his Preface to Ecclesiastes¹: "...sed de Hebraeo transferens, magis me Septuaginta Interpretum consuetudine coaptavi: in his dumtaxat, quae non multum ab Hebraicis discrepabant".²

Perhaps a more apt illustration of the mentality of a translator who follows one source in the main but pecks at another in a fashion which to us appears quite unsystematic and capricious, is provided by the Peshitta version of Chronicles. There the translator's alternative material consists not of the Greek version but of parallel passages in other books of the O.T., which he chooses, often for no apparent reason, to substitute on occasion for portions of the Chronicles text; these portions may consist of anything from a short phrase to a dozen verses³. Thus the behaviour which our hypothesis leads us to attribute to the translator of the Peshitta Psalter is by no means unparallelled.

2. The analogy is not seriously vitiated by the fact that Jerome was faced with an already established Vetus Latina, which was based on G', and from which he was loth to depart.
3. See the articles by Fraenkel and Noble cited on pp. 7:75,77.
2) Greek loan-words which appear in P' in places where G' has the corresponding Greek word, namely אֱלָה, אֲבִּלֵי, אֲבִּוָא, אֲבִּיו, אֲבִּיוֹ, (אֶלֶהָכָךְ), אֱלָהִים.

If it could be shewn that these Greek loan-words had not yet become naturalised in Syriac at the time when the translation was completed, then the revision hypothesis would be greatly favoured. Of none of these loan-words, however, can that be stated with any confidence. Two of them, אֲבִּי and אֲבִּוָא, occur in Deuteronomy (27:15 and 4:37 respectively), which was surely translated into Syriac before \( \psi \); אֲבִּיו may well be not a Greek borrowing but a native Semitic word, cognate with Assyr. pila\( \dot{\text{k}}\)ku "hatchet"; it is therefore likely that these three words, at least, were in current use at the time of the \( \psi \) translator. Nor is it unreasonable to suppose the same of the other three; they are all common in extant Syriac literature, and many Greek words must have been absorbed into the language of Edessa, whose more cultured citizens knew Greek, during the first two centuries A.D. The P' translator may have used these loan-words independently of G', or he may have inclined towards them because he had consulted a G' text; either way, we are far from being convinced that they came in through revision after G'.

2. Friedrich Delitzsch, "Prole omena eines neuen hebräisch-aramäischen Wörterbuchs zum AT", Leipzig 1 86, p. 147, believes that the similarity between the Assyrian and Greek words is coincidental, and that אֲבִּיו might be connected with either.
3. See H.J. Drövers, "Old-Syriac (Edessean) Inscriptions", Leyden 1972. Examples are אֲבִּי in an inscription of 6 A.D. (p.2) and אֲבִּיו (אֶלֶהָכָךְ) in a cent. II inscription (p.6).
3) Cases where P' offers two translations, one of which agrees with G' and is supposed by Vogel\(^1\) to have been inserted by a reviser. An example is at 64:8, MT רָאֵב הַמֶּלֶךְ זָדַּה, G' καὶ ἐξωθησαν ( ζαδα ) ὀ θεός, P' γὰρ τὸν θεὸν ἀληθῶς\(^2\). It is however just as reasonable to suppose that the translator was fairly confident that the Hebrew had a particular meaning, but happened to turn to G' and to be struck by the divergence between its rendering and his own, and so decided to play safe by including both.

4) Phrases which read as if they had been translated from the Greek rather than any Hebrew text\(^2\), e.g. at 40:8, MT רַבּוּ הַנְּכָרָן, G' εν κοιμαλίν βεβαλη, P' ἠλλ' ἐτύλιξα. Here again one may still argue that it was the translator who followed the Greek rather than the Hebrew in rendering the phrase.

5) The abundance of inner-Syriac corruptions in \(\psi\), which have brought about the loss of the original P' reading, and open up the possibility\(^3\) of the original reading having been lost in still more passages, through revision after G'. This argument lacks force because it does nothing to exclude the rival hypothesis that the translator adopted material from G'. It is indeed possible that a few of the agreements between G' and P' came in after the translation, as Vogel suggests (e.g. that the copyist(s) in the chain from the original to the archetype came under the influence of G', or that some G' readings

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1. pp. 491 ff, 500.
2. pp. 494 ff, 500. Vogel sees, in each of our categories 2, 3 and 4, indications "dass dieser G-Einfluss erst nachträglich erfolgte" (p. 500).
achieved general acceptance after the time of the archetype); but the majority, I repeat, seem to be due to the influence of $G'$ on the translator himself.

We can now leave behind us the early stages of original and archetype, and proceed to the subsequent history of the text. For the period up to cent. v, we have little direct evidence, but some inferences are possible. The fact that the variation among our extant authorities (Aphraates, Ephraim, $P'$ mss of cent. vi and later) is in the main confined to very minor matters, indicates that great care must have been bestowed on the transmission of the text, ever since the time when the archetype was written. In particular, much effort was devoted to comparison of texts with one another. For the most part, this intense activity tended to preserve the text intact; the usual fate of an erroneous reading which for one reason or another arose, would be that it was soon detected and the true reading reinstated. On the other hand, there were a small number of cases in which there came into being an unauthentic reading which seemed about as plausible as the true reading; within any circle in which this impostor had the fortune to gain acceptance, the zeal of the scribes would perpetuate it; in this way a number of unauthentic readings must have become widespread (though only rarely was the original reading completely ousted) by cent. v. In many cases, the divergence of mss in this early period is reflected among the mss that survive today. To take just one example: In $\psi$ 21:4 our witnesses split more or less evenly between $\text{\textit{\textalpha\textbeta\gamma\alpha\nu\nu}}$ (A and others) against $\text{\textit{\textalpha\textbeta\gamma\alpha\nu\nu}}$ (C and others). Although our earliest evidence of the split goes back no
further than cent. vi, it is likely that both the alternative readings were attested in mss of earlier centuries.

When we come to the fifth century, we may draw on our knowledge of the controversies which divided the Syriac-speaking church of that time. These divisions created sects which each transmitted the text of $P'$ and at the same time retained its separate identity. The bearing of these facts on the history of the text is naturally a most important question, to which we now turn our attention.

Let us begin by recalling some well-known facts. After the Council of Ephesus (433), the Nestorians broke away from the rest of the Church, to become a separate body. They met fierce opposition, culminating in the destruction of their academy at Edessa (489); thereupon they left the territory of the Roman Empire for Persia, where they established themselves as an independent religious community. In the meantime, there had arisen the heresy of the Monophysites, who rejected the Council of Chalcedon (451), and chose to work as a party within what remained of the Syrian Church. Eventually their efforts, particularly those of Jacob Baradaeus (ca. 500-578) succeeded in winning a great proportion of the non-Nestorians for their own doctrine. Those who succumbed to neither heresy, i.e. the Malkites, became a minority among Syriac-speaking Christians; all but two of the Psalter mss collated by Barnes are either Jacobite (i.e. Monophysite) or Nestorian.

Although there must have been some interaction between the two groups, relations were for the most part unfriendly, as one can discern today from the numerous polemical tracts and the frequent anathemas inserted in the margins of manuscripts.
which have survived. There was moreover geographical separation; fig. B.9.1 shows that it is possible to draw a boundary between the principal centres of the two communities. To this day they have not been re-united.

**Fig. B.9.1.**

![Map showing geographical separation between Roman and Persian Empires and locations of Monophysites and Nestorians.]

**KEY**
- border between Roman and Persian Empires
- Metropolitan see of the Monophysites during period up to cent. x
- ditto of the Nestorians
- Other churches are indicated by dots.
- free-hand border which may be drawn between Monophysite and Nestorian sees.

**SCALE**

- 100 miles
- 300 miles
- 400 miles

This figure has been expanded from Map 36 of the "Atlas of the Early Christian World" by F. van der Meer and C. Mohrmann (Eng. tr. by M.F. Hedlund and H.H. Rowley), London 1958.
The importance of these facts to the student of the Peshitta text was pointed out by A. Rahlfs\(^1\). That these two real families, eastern and western, should have influenced one another, seemed to him "so gut wie ausgeschlossen" (p. 165); we thus have two independent textual families, each leading back to the text common to the Syrians both of the east and of the west in the fifth century (p. 198). On that basis the textual criticism of the Peshitta is greatly simplified. Any witness later than cent. v can be assigned to one of the two groups; and any reading which is attested within both groups is older than a rival which is confined to only one of the two. As J. Holtzmann\(^2\) formulates it: "Stimmen nestorianische und jakobitische Zeugen zusammen gegen nestorianische oder jakobitische Zeugen überein, so dürften erstere für gewöhnlich die Priorität vor letzteren beanspruchen, da die Übereinstimmung ein hohes Alter des Lesart vermuten lässt."

The evidence of the Peshitta Psalter seems at first to offer qualified confirmation of Rahlfs' views. The manuscript map allows us to divide all our witnesses between one close-knit cluster of Nestorian mss, and a looser cluster of Western authorities. Barnes too declared (p. xxxvi): "The boundary is indeed ill-defined, but the existence of the two groups cannot be denied". Admittedly, Rahlfs' statement that neither group appreciably influenced the other, seems to be belied by

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2. "Die Peshitta zum Buche der Weisheit", Freiburg im Breisgau 1903, pp. 30 f.
the fact that there are many occasions when a single ms (or a small group of mss) departs from all the other representatives of its sect to agree with mss of the rival sect. Barnes concluded (p. xli): "The history of the Peshitta is a history of never ceasing admixture of texts". Yet these findings do not invalidate Rahlfs' analysis entirely. It may be urged that his principles may still be applied, on the grounds that despite the incidence of contamination, the most likely inference when we find a reading which is solidly attested among both groups, is that it is at least as ancient as the schism. Roughly the same could be said of the books of Isaiah and Wisdom, in which it was again found possible to distinguish eastern and western texts.

But in at least two other books of the O.T., the situation is quite different. A thorough investigation of the P' mss of Lamentations convinced Albrektson that, within that book, "it is by no means possible to classify the variant readings as Nestorian or Jacobite and to speak accordingly of two distinct textual traditions" (p. 23). More recently, a study of the P' text of the far more extensive book of Judges led Dirksen to a similar conclusion: "A western and eastern textual tradition distinct from each other do not exist" (p. 88).

1. Later Western authorities, not so much singly as in small groups, offend particularly in this respect.

2. As we have pointed out above (p. 7:32), this is not far from Barnes' own policy.

3. For bibliographical details, see Thes., p. 7:71.
These findings cannot be explained away by the supposition that the tradition is so uniform that there is not sufficient divergence among the mss to serve as a basis for any meaningful classification; for both Albrektson and Dirksen found evidence enough to justify a quite different division into two classes, namely (a) the most ancient mss (four in Lamentations, six in Judges) plus certain later mss closely related to them, and (b) the bulk of the later mss. Thus Rahlfs' expectations have proved to be well wide of the mark in these two books, and this arouses our suspicion in Psalms as well.

These findings of Albrektson and Dirksen raise an important point of methodology: How does one decide whether or not it is appropriate to regard a given aggregation of mss as a particular class or family? The usual way of defining a class is to state at least one property which is possessed by all its members and by them alone. In our own field, writers are accustomed to justify the statement that a given collection of mss constitute a particular class, by presenting one reading - or, preferably, a list of many readings - found in every ms of that collection but in no other. For example, Emerton in his study of the P' mss of Wisdom was able to point (p. xlix) to no less than 17 places in which the mss which he identifies as Nestorian agree in a reading shared by no other ms.

Now the reason which Albrektson gives for denying the existence in Lamentations of an eastern and a western text, is that not one passage can be found wherein all the mss of eastern provenance agree in one reading, and those of western provenance in another. Those mss which are known to be of Nestorian origin "do not have one single peculiar reading in common. When these MSS together testify to a
variant reading, they are followed by [sundry] Jacobite authorities..." (pp. 23 f). Again, Dirksen (p. 85) observes that there are just two places in Judges where we find a reading "shared almost exclusively by most or all" mss of eastern provenance; but in one of these, the 'eastern' reading appears in two 'western' mss also, while in the other (1:11), the variation is merely between אדה ('western') and אדר ('eastern'). On such grounds, then, it is concluded both for Lamentations and for Judges that an eastern and a western text cannot be distinguished; and this involves a tacit assumption that the only legitimate way of defining a class is that mentioned above.

But it has been emphasised by Wittgenstein that we often define classes in a different way. He gives as an example the class of "games". There are some characteristics (e.g. amusement, competition) which are found in many of the activities which we call games; but one cannot name any property which is shared by all games and by no other activity. Our justification for using the general term "game" is that "games form a family the members of which exhibit family likenesses". The several individuals in a human family provide a fine analogy: "some of them have the same nose, others the same eyebrows and others again the same way of walking; and these likenesses overlap". Such concepts based on "family likeness" are exceedingly common in our everyday language; for example, most people who use the word "dog", and thereby recognise among living creatures a class of dogs, would not be able to state even one zoological characteristic which is shared by all dogs and by no other being. The specialist in biology has come to appreciate the value of these concepts in his own researches; M. Beckner proposed the term "monotypic"

for definitions of the sort which we first mentioned, and "polytypic" for those of the sort to which Wittgenstein drew attention¹. Within our own field, we may set up a formal definition of a polytypic class thus:

Suppose that we can assemble (a) a large number of readings, each from a different point in the text, and (b) a collection of mss; and that these readings and mss fulfill the following conditions:

1) each of the mss in the collection bears a large proportion of the readings, and no other ms does so;

2) each of the readings collected is borne by a large proportion of these mss, and by no more than a small proportion of the remainder;

then we may regard our collection of mss as a polytypic class.

It is remarkable that Barnes in 1904 did in fact distinguish the Nestorian family of mss on polytypic lines. He gives a 'tentative list of Nestorian readings', covering some eighty passages. In about nine-tenths of these, the 'Nestorian' reading is shared by one or two Western mss; but there is no ms which is not Nestorian and yet bears the majority of the readings listed.

Admittedly, a polytypic definition is far more cumbersome and difficult to check than a monotypic one; but it does constitute a valid method of defining a class. It could therefore be argued that, having found it impossible to distinguish an eastern and a western text on monotypic grounds, Albrektson and Dirksen were wrong to conclude forthwith that the two classes of text did not exist. True, to investigate the possible existence of polytypic classes is no easy task - though at the risk of being accused of peddling my own wares, I suspect that mapping analysis is likely to reveal which aggregations of mss are worth investigating. At all events, I would not go so

¹ "The Biological Way of Thought", New York 1959, pp.22 f. My formal definition below is based on Beckner's.
far as to suggest that the failure to consider alternative possibilities of class definition seriously vitiates the conclusions of Albrektson and Dirksen; but there is a methodological point here which ought not to be neglected.

Why then does Rahlfs' theory break down in relation to these two books? Albrektson believes that the text of the two sects became thoroughly intermingled: "Barnes was indeed right when he described the history of the Peshitta text as 'a history of never ceasing admixture of texts' " (p. 24). Certainly there were many opportunities for such admixture. Firstly, a ms might change hands from one sect to the other. Holtzmann suggests the possibility "dass die eine Sekte der andern Bibelhandschriften raubte und benutzte" (p. 31 n.), but there were other, less spectacular, means. Changes of ecclesiastical allegiance were not infrequent, both on an individual and on a group (e.g. village) level, the classic example being the community of Malabar; in this way, many mss and even whole libraries passed into the possession of a sect in which they did not originate. Again, a library might acquire mss from various sources, and some of these mss may have come ultimately from the rival sect. This point is illustrated by the library of the Syrian convent of S. Mary Deipara, from which come so many of the Syriac mss now lying in the British Museum; largely through the efforts of its abbot, Moses the Nisibene, in the first half of the ninth century, the convent acquired books "from every part of the vast region throughout which Syriac was spoken". Proof that mss did change

hands is provided by one of our Nestorian Psalters (K), which contains in a margin an anathema against the Nestorians.

Secondly, the two sects may have had dealings with each other which extended to comparison of their biblical texts. The doctrinal split did not sever relations between the two communities at all levels, and it is likely that the Bible text was considered to be, so to speak, neutral ground. Already during the century which followed the schism, there are indications that Monophysites and Nestorians read one another's works; and later on, the persecution which both sects endured under the Moslems, from the ninth century onwards, must surely have drawn them closer together. Furthermore, the geographical separation between the two sects was not complete, in that a considerable Jacobite population lived side by side with the Nestorians in Persia; such a circumstance would almost inevitably cause readings to be interchanged. An example of such interaction is the practice of Barhebraeus, who was the Jacobite Maphrian but nevertheless records a great number of Nestorian readings, of which he sometimes remarks (and this is right).

2. Dr. Brock tells me that the writings of the Monophysite Severus, Patriarch of Antioch (c. 465-538), leave no doubt that he had read works of Theodoret, Bishop of Cyrrhus (c. 393 - c. 458), who held Nestorian views for most of his life (though he was prevailed upon to renounce them formally at the Council of Chalcedon). It is significant, however, that Severus does not state the fact explicitly.
3. Atiya, p. 184 etc. From the seventh century onward, the Jacobite prelate whose ecclesiastical jurisdiction extended over this area bore the title "Maphrian of the East".
Yet for two reasons I find it hard to believe that textual admixture alone will suffice to explain the facts. First, the extent of this admixture must be presumed to have been vast, in that it cancelled out completely the usual tendency for a community centred in a particular locality to impart a distinctive character to the text it transmits. If we compare the history of the Vulgate, we find that various local text-types developed (e.g. Spanish, Transalpine, Insular) despite the considerable interaction which evidently took place between these different communities within the Catholic Church. To return now to the Peshitta, we have once again geographical separation, albeit on a somewhat smaller scale, and this is reinforced by the fact that the Jacobites were concentrated in the Roman Empire and the Nestorians in Persia, and by the prevailing antipathy between the two sects; it is hard to believe that such a situation permitted textual fusion so thorough as to suppress altogether the tendency for local texts to emerge. Second, it is odd that those forces which allegedly brought about this intermingling in some books were so much less effective in others, and that two of the most widely read O.T. books (Psalms, Isaiah) are joined here by the relatively little-known book of Wisdom, in contrast to Lamentations and Judges.

It is however possible to explain the situation observed in all these books in terms of another factor, distinct from textual admixture. We shall need to bear in mind two points. First, at the time of the schism, both the Nestorian and the Monophysite factions possessed a good number of adherents and hence a great many mss. Each group must have had a wide and
representative selection of the texts then available. There is no reason why the aggregation of mss in the possession of either sect should have formed a class in the textual sense; almost every Nestorian ms, for example, must have had more in common with some of the Western mss than with some of the other mss which happened to be in Nestorian hands. In other words, the ms collections originally held by the two sects interlaced. Second, we know that comparison of mss within a given community tends to standardise the text and thence to eliminate such interlacing. Thus B. H. Streeter's explanation for the emergence of "local" texts of the Gospels is that "as soon as there were numerous copies of a book in circulation in the same area, one copy would constantly be corrected by another, and thus within that area a general standard of text would be preserved"\(^1\); in our own case, of course, the split was doctrinal as well as geographical. Now I would suggest that in some biblical books the mss were compared more assiduously, and the text in consequence standardised more effectively, than in others. The more widely a book was read and studied, the greater the effects of ms comparison and standardisation. There can be little doubt that, of the five books we have mentioned, Psalms and Isaiah were read the most\(^2\); in these books, accordingly, the process which Streeter described was so

\[1. "The Four Gospels", London 1924, p. 35. It should be made clear that my concept of standardisation of the P text is very different from that of Goshen-Gottstein; see pp. 9:57 ff.
\[2. As a rough guide we may consider the number of times that Aphraates cites a verse from these books: Isaiah 208, Psalms 117, Lamentations 13, Judges 11, Wisdom 1.\]
effective that either sect standardised to a great extent—the text current within it. This standardisation, then, is to be regarded as a by-product of "informal" comparison of mss, rather than an end in itself which was pursued with the official backing of the appropriate Churches. It did not, of course, result in complete uniformity of text; it cannot, for example, be compared with the standardisation undergone by the Massoretic Text of the Hebrew Bible; but it did create a definite boundary between the texts of the two communities. In the lesser-known books of Judges and Lamentations, however, standardisation made relatively little progress; the result was that the interlacing which characterised the original stock of mss of the two groups, continued to be exhibited by the ms population derived from that stock, throughout the succeeding ages.

There is one feature of our map for the Peshitta Psalter which tends to confirm that the text of that book was well standardised in both the east and the west. In all the other traditions for which we have formulated a map, Ω is surrounded by extant mss; here it stands well away, in a corner of the map. We may surmise that, in cent. v, there existed mss lying in many different directions from Ω, but that standardisation drastically reduced the chance that any text which did not conform tolerably either to the Monophysite or to the Nestorian standard, should be perpetuated and leave a representative surviving today. Thus all the mss at other bearings from Ω were virtually doomed to extinction; or, to use a different metaphor, the Monophysite and Nestorian Churches each exerted a vast "gravitational pull" on the tradition, so that every ms became attached to one or other of these two clusters.
The fact that "it is not difficult to isolate a Nestorian family of mss" in Wisdom seems to militate against our arguments. Wisdom, however, can be thought of as a special case. We note that this family contains four mss only, which "agree together with remarkable unanimity", and we may suppose that these mss are all derived from a single ancestor of fairly late date (cent. xv ?). They therefore constitute a self-contained group, while other branches of the eastern tradition which might have interlaced with the western have simply died out, Wisdom having fared comparatively badly among the Nestorians. Certainly the western texts show little sign of having been successfully standardised (pp. c f.).

We now come to evaluate the relevance of the controversies of cent. v to the textual history of the Peshitta. Evidently Rahlfs' analysis cannot be upheld, firstly for the familiar reason that interaction between the two groups is far more likely than Rahlfs supposed, and secondly because the "altwestsyrische Text" and "altostsyrische Text", from which he hoped to build up the text as it was before the schism, never were well-defined entities in the first place. We can hardly hope to distinguish and to assess separately the effects of textual admixture between the two sects and of incomplete standardisation; what matters is that these two factors have combined to render Rahlfs' treatment invalid. In spite of this, the mss of the Peshitta Psalter do fall into

1. Emerton, p. xlv. The mss are EHQS [= 16e1, 18e1, 19e1].
two distinct classes, each of which may be studied in itself. This is an empirical fact, which will greatly facilitate our analysis, provided that we stay clear of Rahlfs' distributional rules, and forbear to generalise our results to other Books in which eastern and western texts cannot be distinguished.

Let us begin at the easier end, namely the Nestorian stream. It will be seen from the map that these mss form a far more tightly-knit group than those of the West. From Table B.7.5, in which we take similarity to MT as a rough index of fidelity to the original text of the Peshitta, it will be seen that the Nestorian mss rate as well as most of the Jacobite ones, although many of the latter are about three centuries earlier than the former. If the Nestorian witnesses differ relatively little both from one another and from the Urtext, then we may infer that the Nestorians were particularly scrupulous in transmitting the text.

We find corroboration in the history of the titles of the Psalms, which were generally copied together with the text itself. These titles have been the subject of several studies. Those of the East Syrian church differ completely from those of the West; but whereas the Jacobite titles diverge a great deal among themselves in different witnesses, the Nestorian titles show little variation. Thus J.M. Vostè:

2. In Biblica (1944), pp. 210-235: "Sur les titres des psaumes dans la Peshittâ surtout d'après la recension orientale"
after examining several Nestorian witnesses, observed: "La tradition orientale, au contraire (sc. as opposed to the Jacobite), est invariable aussi bien quant à la form que pour le fonds". After a collation of the titles in mss, printed editions and commentaries, W. Bloemendaal confirmed that "Vosté's remark...is correct; the Nestorian tradition remained almost unchanged." As these titles seem to have been composed in Edessa and adopted by the East Syrians in the second half of cent. $\nu^3$, and to have remained virtually the same from the time of $C^4$ (cent. vi), they must have been transmitted with particular fidelity, and the same must apply to the text of the Psalms themselves. Thus the text which resulted from the early standardisation which we suppose to have taken place among the Nestorians, has been more or less faithfully preserved among our extant Nestorian mss.

Why does the Peshitta Psalter appear to have been better preserved in the East? A clue may be found in a fact which was pointed out by Diettrich: The Syrians of the West came far more often into contact with Greek-speaking communities, and hence with the Septuagint. Thus $P^1$ was not their only source for the Bible. We know that Philoxenus and Barhebraeus

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1. 17 mss are mentioned explicitly, as examples, on p. 215.
2. op. cit., p. 20.
4. $C$ has Nestorian Psalm titles, but the text of $C$ is Western.
5. "Eine jakobitische Einleitung in den Psalter" [$Beih. zur ZAW - no. 5$], Giessen 1901, p. xlvi.
valued G' more highly than P'; and there must have been many who agreed with them, and many too who, while keeping to P', nevertheless did not regard it as the only acceptable form in which the Bible could be read. Effects of this undervaluing of P' were the emergence of translations into Syriac from the Septuagint, and attempts to revise P' after G'. I wonder whether we can trace here yet another effect, viz that the text of P' was treated with correspondingly less care than in the East, where it had, for practical purposes, no serious rival.

It is interesting to compare the situation in other books of the O.T. For his study of Chronicles, Barnes used just one Nestorian ms (17el, s). On the grounds that "Chronicles was not well known among the Nestorians, perhaps was not regarded as Canonical", Barnes declared¹ that even this Nestorian ms "is probably of Jacobite ancestry". One may therefore doubt whether it offers an altogether certain basis for comparison, though this doubt is somewhat quelled by Emerton's observation that "in Wisdom...it clearly belongs to the Nestorian family of mss" (p. xxi). At all events, it is noteworthy that the text of 17el in Chronicles "stands far above" that of its West Syrian contemporaries²; whether the ms is ultimately of Jacobite ancestry or not, we can at least state that the text did not degenerate seriously during its transmission through Nestorian hands.

In Isaiah, Diettrich felt that the reason for the marked textual superiority of the Nestorian text printed at Urmi (1852), in relation

2. Barnes, Chron., p. xxxi.
to that of Lee and of late Jacobite mss, was "dass jüngere Nestorianer den ursprünglichen Bibeltext im allgemeinen viel treuer konserviert haben als die Jakobiten" (p. xvi). Emerton does not record any impression that the Nestorian tradition is particularly reliable (pp. xlv-lv), but once again the question of canonicity as well as other circumstances (p. 9:44) put Wisdom on rather a different footing. The discussions of Albrektson and Dirksen, in which the very existence of a Nestorian text as such is denied, are of course not relevant here.

Despite the relative uniformity of the Nestorian tradition, one must not lose sight of the divergences which exist. There are 36 places where our Nestorian witnesses, K L m N O Ua Uc X, divide into two groups of which each contains more than one ms; the most striking of these variations are לַעַשׂ סָמַּל (51:21), the presence/absence of הָעַזְבָּא (79:5) and הָעַזְבָּא הָעַזְבָּא (80:5). Little order can be discerned in the combinations of the mss in these passages, except that LN agree against all the other Nestorian authorities on eight occasions.1

Far more numerous are those passages in which one of the Nestorian witnesses deserts the rest to present a reading which has Western support. Taking O as an example, we find:

1. namely 16:4 (hiat O); 34:14; 48:13; 60:12 (hiat m); 80:5 (L concurs); 105:8 (hiat K); 115:16; 118:25.
In all these cases the reading of 0 agrees with one or more Western mss. The same phenomenon is found in each of the Nestorian mss, thus: K, 9 times; L 15x; m 7x; N 26x; O 5x; Ua 5x; Uc 5x; Ua and Uc together, 12x; X 6x. In view of our arguments above, two possibilities must be considered in relation to passages mentioned here on in the last paragraph: either that the minority reading is due to Western influence or that both readings were handed down in Nestorian circles from the earliest times but that one attained far wider currency than the other.

Regrettably, there seems to be little Nestorian evidence older than that of the mss collated by Barnes. The most hopeful prospect is Sal, which has been found to have Nestorian affinities in Wisdom\(^1\), the Odes\(^2\), Ezra and Nehemiah, but not in Judges\(^3\). There are also patristic works which could be searched, from Narsai (399-502 !) onward\(^4\).

1. Emerton, pp. lii f.
3. Dirksen, p. 35, who owes his information on Ezra and Nehemiah to the Peshitta Institute.
4. Narsai's Homilies on the Creation; ed. P. Gignoux (Turnhout and Paris, 1968) in Patrologia Orientalis xxxiv 3,4, seem to contain no relevant quotation. However, some may turn up in A. Mingana's great edition of 1905.
We now come to those mss which represent the Western text. Here the witnesses range over a far wider area, which at first seems to be quite shapeless. Closer inspection, however, reveals that most of them fit into a wedge-shaped region, which radiates outwards from \( \Omega \). This is brought out in fig. B.9.2, which reproduces the portion of the map that contains the Western authorities. Each siglum is followed by a number giving the probable century.

There are three witnesses which do not fit into the wedge: F, Lee and Barhebraeus. In order to include them, we should need to increase the angle of the wedge by about half as much again in either direction. We shall return to them presently, but first let us consider those Western mss which the wedge does accommodate.

The significance of this wedge can be appreciated in terms of the result reached in Thes., pp. 5:5f, that if a ms \( M_2 \) is derived from a ms \( M_1 \), then a path running from \( \Omega \) to \( M_1 \) and thence to \( M_2 \) will not be very different from a straight line. The following hypothesis therefore suggests itself: The text of the Monophysites was considerably standardised soon after the schism, so that nearly all the mss in their possession came to be limited to a particular area of the map. This area is very roughly and tentatively indicated by the shaded oval which we have superimposed in fig. B.9.2; however, it may well have extended somewhat farther. From this original stock of mss are derived the texts of all the authorities which lie within the wedge. Those mss which are closest to \( \Omega \) represent an early state of the text current in the West, and the mss which we encounter as we proceed outwards from \( \Omega \) represent successively more degenerate forms thereof.
Note. The measurements of the angles subtended at $\Omega$ are only approximate.

Fig. B. 9. 2.
This hypothesis is supported by a tendency which does not seem marked at first sight but is nevertheless statistically significant, namely that, within the wedge, older mss are inclined to lie closer to $\Omega$ than are later mss\(^1\). In particular, the text of Philoxenus, one of the most prominent figures in the early Monophysite Church, is one of the closest to $\Omega$. Admittedly, there are many exceptions to this trend; we may single out for mention the cent. vii Codex A, which is represented as relatively degenerate. However, we recall what was said on pp. A\(\frac{3}{4}\)ff about age and generation; although later mss will tend in general to have been through more copies than earlier ones, the association is not close, and we ought not to be surprised to find late mss offering a text which has passed through less copies and has had less opportunity of becoming corrupt.

There would be little point in specifying minutely the place of each of these mss within the tradition, and still less in detailing all the multifarious combinations which appear in the passages where they disagree among themselves; we shall confine ourselves here to certain major points, proceeding as far as possible in chronological order.

The mss which best represent the text which resulted from the early standardisation effected within the Monophysite church are CGHQSZ. As we have already remarked, the absence of A is surprising, but it is a fact that this ms, despite its age,

\(^1\) The coefficient of correlation between the age and the distance from $\Omega$ of the 16 witnesses that lie within the wedge, is $+0.5306$. Using a two-tailed test, we find this to be significantly high at the 5% level (though it falls short of the 2% level, viz 0.574).
contains a number of undoubted errors which are not found in
CGHQSZ but reappear in sundry later western mss, e.g.

28:8] [אָבֶדֶא] JPSAT Le, bH < נָלַגְּנַבְּבָנהָנָבָהָנָבָהָנָבָהָנָב (MT הַנָּעַבְּבָהָנָב)
45:7] נָלַגְּנַבְּבָנהָנָב AT < נָלַגְּנַבְּבָנהָנָב (MT הַנָּעַבְּבָהָנָב)
68:23] נָלַגְּנַבְּבָנהָנָב ABDEHJT Le < נָלַגְּנַבְּבָנהָנָב (MT הַנָּעַבְּבָהָנָב)
102:24] נָלַגְּנַבְּבָנהָנָב AE < נָלַגְּנַבְּבָנהָנָב (MT הַנָּעַבְּבָהָנָב)
110:6] נָלַגְּנַבְּבָנהָנָב ARTZ Le < נָלַגְּנַבְּבָנהָנָב (MT הַנָּעַבְּבָהָנָב)
147:13] נָלַגְּנַבְּבָנהָנָב ADE < נָלַגְּנַבְּבָנהָנָב (MT הַנָּעַבְּבָהָנָב)

One can therefore justify the statement that A, albeit early
in time, presents what is generally a later form of text than
do CGHQSZ. In the Psalter at least, we should miss a number
of good readings if we followed the recommendation with which
Haefeli ended his book, that we should give up the heavy task
of collation and content ourselves with the text of A.

We note that in other books, the worth of A's text in comparison
to that of the other mss there extant has been found to be far higher.
In most of the studies listed on p. 7:71, A is ranked among the most
important and reliable witnesses. The reason for its relatively poor
performance in ψ is uncertain; the possibility should not be neglected
that a pandect like A may have been derived from different sources in
different books.

The variants just listed help to clarify the concept of
pure and degenerate Monophysite texts. Each of the readings
on the left-hand side is unauthentic but plausible and, by
virtue of the ceaseless comparison of texts which characterises
the transmission of P', achieved more or less wide currency.
The more readings of this sort are to be found in a given ms, the more 'degenerate' may its text be termed. Because of the differing judgments of different scribes, the mss in existence at any one time would exhibit greatly varying degrees of degeneracy.

We now come to cent. ix and to Cod. F. The place of F on the map shows that it is western but that it has been affected far less than most other western mss by Monophysite standardisation. We may surmise either that F was copied directly from a ms which was at least as ancient as cent. v and therefore pre-dated this standardisation or that F originated in a different milieu from the mss which lie within the wedge. It is not easy to choose between the two hypotheses, which indeed are not mutually exclusive. Counting somewhat in favour of the latter is H. Schneider's suggestion, based on F's text of the Odes placed after the Psalms, that the scribe was "wohl melchitisch"; on the other hand, our map for $\Psi$ does not indicate that F has any affinity with the two mss known to be of Malkite origin, P and T.

The fact that F is largely free of the effects of standardisation will account for those cases in which it alone preserves the original reading. In cent. v there were many passages in which a false but attractive reading had become quite widespread, though it was far from having ousted the true reading altogether. When the schism occurred, we may suppose that in all these passages the false reading was well represented

1. ZAW (1950) p. 198.
among the mss then in existence both of the Monophysites and of the Nestorians. We may imagine that among these passages there were a good many in which

(1) the weight of opinion within both sects, independently of each other, was in favour of the false reading, which accordingly came to be established throughout either sect, to the apparently total exclusion of the true reading, by virtue of the standardisation process;

while (2) F copied from his exemplar the true reading, which would have been fairly widespread were it not for the standardisation process; if he knew of the rival reading, he evidently declined to adopt it.

If it be doubted that these false readings should have commended themselves so well to both the Monophysites and the Nestorians, we may point out that Barnes too was content to accept them rather than the alternatives offered by F alone. The fact that F himself did not succumb to the temptation presented by the false reading, may be explained in more than one way. Presumably F found the true reading in his exemplar. Perhaps that exemplar was written at a time when the true reading was still widespread, and F, even though he may have known of a rival reading, was minded to adhere to his exemplar; perhaps F worked within a limited religious circle, into which the false reading had never penetrated. At all events, the phenomenon of unique preservation, at first sight so hard to accept, does admit explanation.

This argument explains why a Book like Judges affords so few instances of unique preservation. In Judges, standardisation had far
less effect, and there was a correspondingly smaller likelihood of a false reading being so thoroughly diffused through the ms tradition that the true reading should survive in one ms alone.

We note finally that F sometimes agrees with later western authorities which fall within the wedge; some instances are listed on p. 7:30. In view of the points made in our discussion of the relationship between eastern and western texts, this fact can, but need not necessarily, be taken to mean that F was influenced by the mainstream of Monophysite texts current in his day.

The ninth and tenth centuries provide us with the two "massoretic" mss which we have located on our map, namely X (9m1) and Z (10m1). These mss do not give the whole text, but exhibit certain phrases only, with vocalisation and other points. A most important paper on the nature and purpose of these mss was written by Abbé Martin. If I may abstract some of his conclusions: These mss were designed as aids in the correct pronunciation and punctuation of scripture, a subject which claimed the attention of anyone who applied himself to sacred studies in the Syriac speech-area. They all emanate from one circle or school, which was "en grande partie sinon exclusivement jacobite" (p. 263). One ms however stands out from the rest: our X. Although X is in other respects similar to all the other massoretic mss (p. 337), the handwriting and vocalisation point to Nestorian origin.

1. "Tradition karkaphienne, ou La massore chez les Syriens", Journal Asiatique (1869), pp. 245-379. Martin gives a valuable appendix of tables and facsimiles. A helpful introduction to this topic may be found within Wright's great article "Syriac Literature", in Encyclopaedia Britannica, 9th ed., vol. 22, p. 826.
Our map confirms that, in the Psalter at least, X offers a Nestorian text. Z on the other hand is western, and fits into the "wedge". It is noteworthy that out of all the mss lying in the wedge, Z is the closest to $\Omega$, despite being as late as cent. x. One may suppose that the punctilious scholars amongst whom these mss were produced transmitted the text with particular care. I would regard Z as a valuable representative of the 'early' or 'pure' Monophysite text, and would hope to find that other massoretic mss are of comparable worth.

These results concerning the western massoretic mss may be compared with findings in other books. Regarding Wisdom, Emerton states that "in certain respects the massoretic mss, although western, stand nearer to the ancient mss than to [the great majority of western mss]" (p. lxxxii). Albrektson finds that in Lamentations they "represent a stream of textual tradition which stands near the ancient MSS" (p. 31). The ten western massoretic mss extant in Judges form "a group of related mas, with the same type of text as found in the group of ancient mss" (Dirksen, p. 99).

It seems necessary here to examine the views of M.H. Goshen-Gottstein¹, who attaches particular importance to massoretic activity in the textual history of the Old Testament Peshitta. This activity, he believes, had far-reaching effects both in the East and in the West,

for thereby the text became "practically standardised" (p. 30). Thus he says of the text of ψ (p. 33): "When we examine the mss, the material leads us to distinguish between those written before the tenth century approximately and those written after it. That century saw the final fixation of the Syriac Biblical Massorah, the fairly rigid standardisation of the text by that time being characterised by the two authoritative Massorah mss, B.M. Add. 12178 (Jacobite) (Lour Z) and 12138 (Nestorian) (LX)."

A corollary which Goshen-Gottstein draws is that the "post-Massoretic" mss contain hardly any valuable material which is not to be found in the "pre-Massoretic" authorities that survive today. "If", he continues, "we compare the apparatus built on all the early mss with those mss later than the tenth century, we find that practically no additional variant of any 'value' can be elicited." In a footnote, he gives us to understand that the only variant of 'value' found in a post-Massoretic ms of the Psalter is 𐤄𐤃𐤄𐤃(N) against 𐤃𐤃𐤄𐤃(rel.) in ψ66:14. He makes an exception of one later ms, B (12al), which he terms "the only non-standard Peshitta ms written after the time of the Massoretic standardisation" (p. 35).

Goshen-Gottstein further deduces that, throughout the O.T., no borderline exists between Jacobite and Nestorian texts. "One of the main tasks of a future editor was, according to Haefeli, to differentiate between the two groups of text tradition, the Jacobite and the Nestorian. However, we need no longer wonder that neither Haefeli nor any other scholar could find any real textual differences, apart from orthographic and grammatical peculiarities. ...There exists no Nestorian manuscript from the time before the text was practically standardised" (p. 30).
These statements, despite the confident tone in which they are propounded, are not altogether consistent with the facts. The first question is the supposed standardisation effected by massoretic activity. It is quite likely that standardisation of the text was one of the aims of the massoretic scholars. Thus Bishop Joseph David of Mosul writes, in a letter to Martin quoted by the latter on p. 374: "Elle [the Syriac Massorah] a été faite... dans l'intention de fixer et d'établir, avec tous les signes possibles, la vraie leçon et la véritable orthographe". However, such standardisation cannot be said to have been the result; the evidence available does not by any means suggest that western mss later than cent. x all conform more or less to the same standard form. They vary among themselves, in fact, at least as much as those earlier than cent. x. Even if we follow Goshen-Gottstein in excluding B, we still find that P, S, T, Le, Barheb. and Anon. are hardly as close to each other, either on the map or textually, as his remarks would have led us to believe. Thus the mss of the West show little sign of having been successfully standardised in cent. x. In the East, admittedly, the mss are far closer to one another; but we have already explained this in terms of the fidelity of the Nestorians to the type of text which they evolved in the years following the schism.

Nor can we accept the corollary that no variants of "value" (except at 66:14) are to be found in mss later than cent. x. The following passages provide instances of distinctive variant readings which are certainly of interest yet do not appear in any ms earlier than the twelfth century:

7:15 \( \text{Le Ua Uc} = \text{KT, bH: } \text{most others (MT } \pi'\nu'y \text{)} \)

104:24 \( \text{Ua Uc} = \text{KLN, bH: } \text{rell. (MT } \pi'\nu'y \text{)} \)
These examples suffice to show that "post-Massoretic" mss do offer variants which one cannot afford to ignore.

The assertion that we have no Nestorian ms older than the ninth century seems to be true in much but not all of the O.T. The ms 8al, as we remarked above, has Nestorian affinities in some books but not in all (p. 9:49); two fragments bound together in B.M. Add. 14668, namely 7k10 and 8j1, are of Nestorian origin and cover some twenty chapters distributed among Isaiah, Jeremiah and the Minor Prophets; apart from these, the List mentions no Nestorian ms earlier than cent. ix. But the inference that eastern and western texts do not show "any real differences" grossly over-simplifies what we have seen to be an intricate question. In order to appreciate how little it can be justified, one need only glance back at the five passages just cited, where all the extant Nestorian witnesses agree against all the Jacobite manuscripts\(^1\), and these differences go much further than "orthographic and grammatical peculiarities".

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1. The argument is not affected by the fact that Barhebraeus and Lee, whose texts bear marks of Nestorian influence, casually join the Nestorian group.
With cent. xii we meet our first ms which may be confidently described on grounds of handwriting as Malkite, namely P(12t2). A later ms, also in a Malkite hand, is T (cent. xiii/xiv; no Leyden siglum). Textually they are quite close, and we may tentatively regard them as a Malkite group. Now, both lie within the "wedge" which contains most of the western mss, and both are quite far out. Thus they appear to be relatively poor representatives of the Monophysite type of text. This is disappointing; for if the Malkites represent those who did not embrace either the Nestorian or the Monophysite doctrine, we should expect Malkite mss to have been unaffected by the standardisation which occurred in those two sects, and to offer — as F does — a text of considerable independent worth. However, by locating themselves within the wedge, they belie these expectations. Occasionally, it is true, they desert the Jacobite witnesses to agree with the Nestorian, but no more often than do other late western authorities. Thus the Malkite tradition cannot be said to hold the balance of power between the two great families, Jacobite and Nestorian.

Two of our latest western authorities, namely Barhebraeus and Lee, fall outside the wedge. Their bearing from lies between that of the other western mss and that of the Nestorian cluster; according to the work of p. 5:6, this position

1. We may recall here the tentative suggestion that T is of Malkite origin (p. 9:54).
2. Barnes, p. xli, gives examples.
3. The reader can easily confirm this statement by referring to Barnes' "tentative list of Nestorian readings" (pp. xxxvii-xl).
suggests that their original stock is western but that they have acquired many Nestorian readings. We have already explained (p. 8:45n) how Nestorian influence came to be exerted on Barhebraeus, whose work in turn had a profound effect on the printed editions of F'. In that other late western texts (especially B) do not share this "eastward" tendency, we may doubt whether Barnes' deduction (p. xxvi) that "a considerable Nestorian element had intruded itself into Jacobite codices by the beginning of the XIIIth century" is valid as a general rule.

Now that we have completed our outline of the textual history, we may return to the "rules" evolved in Ch. 7 for discriminating between rival readings, and explain them in the language of traditional textual criticism as opposed to that of our own theory of map interpretation. We shall find it convenient to use the abbreviation RF to denote the reading of F (which need not, of course, be unique to F).

1) Suppose that F and the Nestorian mss agree against all the rest. Then the likeliest explanation is that the western mss other than F have a false reading which became established through standardisation among the Monophysites and was transmitted to all the western mss except F, and that RF is original. We must not be deterred by the fact that the reading to be rejected appears in our two oldest mss (CA); for they are both Monophysite.

2) If F stands alone, then RF is probably an error; but the possibility of unique preservation, as explained on p. 9:54, ought not to be overlooked.
3) If F finds no support among Nestorian, but some among Jacobite witnesses, then this additional evidence for RF increases its likelihood of being original above the level of case (2). In particular, if RF is solidly attested among our best representatives of the early Monophysite text (CGHQSZ), it must have been widespread among the early Monophysites; and as it also appears in the largely independent Codex F, the likelihood of its being original is considerable, albeit less than in case (1).

I would conclude this Chapter by stressing that generalisation of our results in \( \psi \) to other parts of the O.T. is precluded by the great differences which appear in the overall structure of the P' mss in different Books. These discrepancies present a most intriguing problem. As we have already noted, the eastern/western dichotomy, central to our analysis for \( \psi \), cannot be discerned in Lamentations and Judges. In turn, the P' mss of those books show a different twofold division, into ancient and later mss (the borderline being drawn around cent. ix), while the mss of \( \psi \) do not in any way lend themselves to such a classification. Wisdom is different again, for Emerton there adopted a division into three classes, ancient/eastern/western. As we turn from one Book to another, no common theme emerges, on which we might hope to base a systematic treatment of the history of the O.T. Peshitta text in general; it almost seems as if each Book were a law to itself. Perhaps we shall discover one day a unifying principle, a general theory into which the discordant situations in the various books can be fitted as particular cases. This cannot of course be achieved without ample knowledge of
the textual data, and proper methods for its analysis. The former need is being admirably fulfilled by the Leyden edition now in progress; how far the methods here proposed will contribute towards the latter, remains to be seen.
10. Annotations to the Text of the Peshitta Psalter: with a discussion on the use of computers etc. in the textual study of the Peshitta

The purpose of this chapter is threefold. First, it is shown how the map is applied in practice to the problems of discriminating between rival readings. Second, this is an opportunity of proposing some emendations, and of making other comments on matters of textual interest. Thirdly, it will be recalled that one of the reasons I gave in Chap. 7 for working with a unique Urtext rather than adopting a "Kahle view" was that in none of the variant passages recorded in Barnes do we find more than one reading which can be explained only in terms of a translation from a Hebrew source (rather than scribal error, assimilation to G' or to parallel passages, and so on). Those variants which involve the unique readings of F have already been dealt with. In most other cases it will be obvious to the reader how all variants (except for the one which we accept as original) can be explained without our postulating a second translation from the Hebrew; sometimes a note in Barnes' apparatus will make it clear. But where the cause may not be evident, I have added a note (see Γ 51; 106:13, 29; 118:17).

These annotations are not meant to be exhaustive. There is no need to spoon-feed the reader, who is usually left to

1. I have worked in this respect mainly on the first book of Γ.
apply the critical rules for himself in choosing between the variants at any point that interests him. My own selection of passages for special mention is necessarily subjective, but it will enable the reader to form an impression of the practical usefulness of the method. As for the emendations and notes on the relationship between $P'$ and $MT$, these are intended to supplement rather than to challenge the writings of J.A. Dathe\(^1\), F. Wutz, F. Baethgen\(^2\), B. Oppenheim, J. F. Berg, F. Zimmermann\(^3\), E.R. Rowlands\(^4\) and - most recently - A. Vogel. (I owe these references to the courtesy of Dr. W. Baars, of the Peshitta Institute).

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1. For details of Dathe's work, see p. 9:2; on Wutz, p. 8:7; on Oppenheim, p. 9:5; on Berg, p. 9:2; on Vogel, p. 7:1.

2. "Untersuchungen über die Psalmen nach der Peshitta" in Schriften der Universität zu Kiel (1878). See also his paper of 1882 (cf. p. 7:13).


THE ANNOTATIONS

ψ2:1] The mss vary between αἰ and ἀναι (MT שֶׁה, G' אֱפֶּהַ֣וֹנ). αἰ is apparently Aphel, meaning "they feel"; it is not suitable here, and our first reaction is to reject it. However, let us consider the distributional evidence:

αἰ Le Ua Uc = AB?DEHLR?TX bH

αἰ CFJKZ Dan

Now αἰ is supported by F, by two "μ-type" ms (CZ, but not H), and by the Nestorian ms K (but not LX). It thus is the reading favoured by our rules.

But it will be objected, "Why do the nations feel?" is inappropriate. So it is; and I believe that αἰ is not Aphel, but Peal with prosthetic Alaph (see pp. 8:33 ff.), which is relatively common before Resh. Thus αἰ represents the older spelling, which may well have survived because copyists mistook the word for Aphel.

ψ 4:3] MT וַאֲקָם֭הָּ לְכָלְּמָּה

P' וַאֲקָם֭הָּ לְכָלְּמָּה דְּרָכְּנָּהָּ

"how long will ye hide my glory?"

1. We use μ as shorthand for "Monophysite" and ν for "Nestorian".
Lagarde suggested that the translators rendered as if it were. Perhaps however another explanation may be offered. We know that does not occur elsewhere in ψ, and that the verb 'is rendered in P' by in ψ 35:4; 40:15; 69:7; 70:3. An Aphel of this verb is attested, and so we may read: "how long will ye shame my glory?".

ψ 7:15 For (F + CHZ and others), which seems original, most Nest. witnesses have ψ. N however omits ψ and reads "and cunning". This seems to be an inner-Syriac corruption of the Nest. reading:

ψ 10:15 This half-verse has three variations which are graphically slight but involve very different meanings. We have:

MT

G' ... καὶ κοινωνοῦ, ζητήσατε ὑμαῖς ἀμαρτία αὐτῶν, καὶ ὃς μὴ εἰρηκόν δι' ἁμαρτήματα

Barnes has in his P' text:

1. I have this at second hand from Techen, pp. 158 f.
and records the variants:

\[
\begin{align*}
\text{ACHZ} & \quad \text{Le Ua Uc} = \text{DEFJKPTX} \text{ m } \text{bH} (=\text{MT}, \text{G}') \\
\text{non liquet L} & \\
\text{ACDEFHJKPZ} & \quad (+T^2; \text{ non liquet } T^1)(=G') \\
\text{Le Ua Uc} & = \text{Lx}m, \text{ bH} (=\text{MT?}) \\
\text{ACDEFHJZ} & \\
\text{Le Ua Uc} & = \text{K[L]PX[m]}, \text{ bH} (=\text{G}') \\
\text{T} &
\end{align*}
\]

(The readings of BGNOQRS are not available throughout.)

Thus P' means:

"As for the \underline{wicked} man, let his sin \underline{overtake him} and let \underline{him} not be found".

Let us see how our rules help us to choose. Rule 1 gives \underline{\text{Lk}}. It is likely on intrinsic grounds too that \underline{v} was rendered \underline{\text{m}}, not \underline{\text{m}}. For the other variations, intrinsic criteria are inconclusive. Most of the divergences involve grammatical morphemes, which were treated so freely by the translator that he was theoretically

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1. \underline{\text{Lk}} is of course from \underline{\text{v}}, and \underline{\text{m}} from \underline{\text{v}}.
capable of writing any one of the alternative readings. The only lexical divergence is \( \sqrt{\text{כָּדָה}} \) against \( \sqrt{\text{כָּדָה}} \) for \( \sqrt{\text{כָּדָה}} \); in fact both Syriac roots are found elsewhere to render \( \sqrt{\text{כָּדָה}} \) (e.g. \( \psi \) 9:11; \( \psi \) 9:13). So we must turn to the distributional factor, and Rule 2 suggests \( \sqrt{\text{כָּדָה}} \) and \( \sqrt{\text{כָּדָה}} \). This yields: "As for the wicked man, let his sin be sought ( \( \sqrt{\text{כָּדָה}} \) ? ) and let it not be found". If we accept this, then it seems that the translators did their best with a difficult Vorlage and resigned themselves to the discord between the severity of the first line of the verse: "Break the arm of the sinner" and the compassion of the second.

Once we postulate this, we can explain the origin of the variants. Some Western mss tried to remove that discord by substituting \( \sqrt{\text{כָּדָה}} \) ("...but as for the poor man,...") so ACHZ. Others modified the verb \( \sqrt{\text{כָּדָה}} \) (whereby the sin is effaced) to \( \sqrt{\text{כָּדָה}} \) (of the sinner); they may have been influenced by G'. Most of those who had \( \sqrt{\text{כָּדָה}} \) (but not KP) made the further change from \( \sqrt{\text{כָּדָה}} \) to \( \sqrt{\text{כָּדָה}} \), which better suits this new sense.

\[ \text{\( \psi \) 13:16} \] MT \( \text{כָּדָה} \), \( \text{P'} \) \( \text{כָּדָה} \)

We do not find \( \sqrt{\text{כָּדָה}} \) rendered elsewhere in \( \psi \) by \( \sqrt{\text{כָּדָה}} \); the usual equivalent is \( \sqrt{\text{כָּדָה}} \) (about 10 times, see Techen, p. 306), which may take an accusative of the person rewarded.

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1. I am intrigued by the resemblance of \( \text{P'} \) in Jer 50:20 but I do not know whether it is anything more than a coincidence.

2. T's \( \text{כָּדָה} \) is a further corruption.
Two explanations suggest themselves: (a) that the translator read הָלַע for לָע; (b) that the original reading is "who hath rewarded me". Again, in י 57:3, יִּשֵׂה יָדַע becomes in P' יִּשֵּׂה יַדֵּע. I prefer to suppose inner-Syriac corruption in both passages; but if the readings are in fact original, then we again see in י 57:3 that whoever wrote יִּשֵּׂה for יֵדַע was preoccupied with thought of the Saviour (see pp. 9:13 ff.).

This י is of course parallel to י 53. There is however some variation in P' between the two Psalms, not corresponding to any difference in MT or G'. I do not know how this is to be interpreted:

14:3, 53:4 (but BR as in י 14).

14:14, יִּשֵּׂה (AF יִּשֵּׂה לֹא יִּשֵּׂה). 53:5

14:4, יִּשֵּׂה לֹא יִּשֵּׂה (AF יִּשֵּׂה לֹא יִּשֵּׂה). 53:5


and thus to read in י 57:3 יִּשֵּׂה. The nom. ag. יִּשֵּׂה is not found elsewhere in י , but it is attested in Heb. 11:6 (for Gk μισθωτος διωγμος, also applied to God) and is fairly common in Syriac literature.
Out of the 20 other occurrences of הָנֹתִין in ψ, P' renders 19 times וַיְאוּן 1 (Techen, p. 158). Perhaps וַיְאוּן originally stood here too, but the phrase in V. 5 כַּפַּרְתָּן אָלְפָּן caught the eye of the copyist, who wrote מַאֲכֶל for the not dissimilar וַיְאוּן.

ψ 17:7] MT מַאֲכֶל וַיִּתְנַשֵּׁף מְעַרְעַב נַעֲצֵה

For P', Barnes prints in the text:

"and make thy holy one (= בָּשָׂם) a wonder, and a saviour of those that hope."

If this is the original text, it has a very Christian turn. However, Barnes records the variant 2:

Le Ua Uc = ADEGKLNOTm

BC (over an erasure) FHPQ

Now the Waw is not in F or the "early μ" mss available (HQ-C is doubtful). Thus Rule 2 suggests that we prefer the reading לָמה . This would probably be taken as vocative ("make thy holy one a wonder, O saviour..."), which is not far from the usual interpretation; even so a

1. In ψ 71:13 it is omitted.
2. There are two other variations: לָמה for לָמה and the addition of תָּחֹן after לָמה . Rule 2 favours Barnes' text at both points.
Christian view could be admitted if וַיַּרְאֵּ רַמְגָּסְא were taken as accusative ("...a wonder, even a saviour").

This confirms that Rule 2 has given the better reading; for a meaning which seems almost unequivocally Christian (וַיֵּרֶאֲרֲגָסְא) is likely to have been substituted for one which is not (וַיֵּרֶאֲרֲגָסְא), rather than vice versa.

ψ 18:13 For MT מְגָסַר כְּלִילָה, P' has מְגָסַר מְגָסַר ("from the splendour of his pavilion")1. This is strange, for מְגָסַר is rendered elsewhere in ψ by מְגָסַר or מְגָסַר2. The clue is surely that מְגָסַר (which occurs only 5 times in ψ ) is found in the preceding verse: MT מְגָסַר מְגָסַר. P' מְגָסַר מְגָסַר. It seems that the translator's eye may have jumped to מְגָסַר instead of מְגָסַר , so that we have two renderings of the former and none of the latter.

Now, we find in Hebrew mss a traditional method of spacing out the Song of Moses (Exod. xv), and this layout has often been imitated in the ode of 2 Sam 22 = ψ 18. It is prescribed in the Talmudic minor tractate Soferim 12:10 that the Songs of Moses and Deborah be laid out in a manner resembling the laying of bricks (אַרְוָא דְּעֵבִי עֵבִי נֶעְרָה : see extract below). No special format was set down for the Song of David (2 Sam 22 = ψ 18), and it was in fact recommended that it be spaced.

1. This is also the reading of the parallel in 2 Sam 22 (so Le, A). G' has here וַיֵּרֶאֲרֲגָסְא וַיֵּרֶאֲרֲגָסְא , and in 2 Sam וַיֵּרֶאֲרֲגָסְא וַיֵּרֶאֲרֲגָסְא , but this does not seem relevant to P'.
2. In ψ 54:5 = 86:14 it is paraphrased.
out in the form of stichoi, like the rest of the Psalter (Sof. 13:1). Nevertheless, the "brick" spacing is found in many mss and editions, and it particularly favours eye-skip between גגדר and לorious א HDF שטיע מצעב, מצעב מצעב למעט מצעב.

If this had been the arrangement in the Vorlage of $P'$, then it would have been easy for the translator's eye, after reading מצעב, to skip to instead of גגדר and the rendering would be explained. It is hardly likely, of course, that this occurred independently in 2 Sam and $\psi$; we may accept the opinion of D. M. Englert, that the $\psi$ text was taken over into 2 Sam.

1. "The Peshitto of Second Samuel", Philadelphia 1949 [= Journal of Biblical Literature Monograph Series, vol. 3], p. 95. Over the 59 variations between 2 Sam 22 and $\psi$ 18 according to MT, $P'$ in 2 Sam agrees with $P'$ in $\psi$ in 55 places, in virtually all of which he deviates from the MT of 2 Sam to follow that of $\psi$. (The variations in the 4 other places are not substantial.) Thus Englert concludes that the $\psi$ text was adopted in $P'$ to 2 Sam. We cannot tell whether 2 Sam was translated before $\psi$ (in which case the $P$ text of 2 Sam has undergone revision) or after (in which case the $\psi$ version may have been taken over by the translator of 2 Sam himself).
How likely is it that the Vorlage was so arranged? The reference in Soferim indicates that the "brick" spacing, at least for the Song of Moses, is ancient. It is surely not unreasonable to suppose that this well-known arrangement was imitated for the Song of David, as in many Hebrew mss, despite the disapproval of Soferim against this extension of the use of "brick" structure.

ψ 21:4 We have the two alternative readings

MT רָבָּמֶהמ֚וּ "for thou greetest him"
Le Ua Uc = B\text{vid} CDEKLNOM, bH

AF\text{vid} GHJPQZ "for thou coverest him"

MT רָבָּמֶהמ֚וּ, G', προφήθασας αὐτόν. Because, רָבָּמֶהמ֚וּ agrees with both MT and G', we are faced with the familiar dilemma: are we to accept it because it agrees with MT, or reject it because it may have come in from G'?

Rule 2 would favour רָבָּמֶהמ֚וּ, in F and GHQZ (but not C). However, Techen's glossary shows that רָבָּמֶהמ֚וּ occurs only twice elsewhere (ψ 68:14, 31); in both passages it is in the passive ptp. (רָבָּמֶהמ) and does not render רָבָּמֶהמ. On the other hand, רָבָּמֶהמ is found 12 times (Techen, p. 311) for רָבָּמֶהמ, which is to be preferred on those intrinsic grounds. Here then I would depart from Rule 2.
Even by Syriac standards, "wailing bones" are somewhat bizarre. Our suspicion increases when we find that the verb  does not occur elsewhere in  .

The passage is discussed by Vogel (p. 353), who does not question the text. He notes that P', like G', has a verb in 3pl. rather than 1 sing., and he suggests that P' read  , "they mourn". Against this is the fact that P' never renders  elsewhere by  . The usual equivalent is  (usually Aphel), not only in (30:12  ) but also in the other books.  

If we look at the many other occurrences of  in  , we find that the commonest rendering by far is  (20 times, see Techen). Might this have originally stood here too? If the Urtext had  , our present text would have resulted from a corruption:

\[ \text{כְּהִנָּה} \rightarrow \text{כְּהִנָּה} \].

1. although the noun  appears in  144:14.
2. Vogel cites the expansive rendering in Jer 25:33  but this is hardly parallel.
3. The corruption is earlier than Aphraates, who also has  (p. 342). Another corruption whereby Shin was mistaken for a single stroke is postulated by R.P. Gordon (1971 p. 502) who in Hab. 3:4 ingeniously proposes  for  (MT ).
In that case, $P'$ and $G'$ would testify independently to a consonantal text.

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ψ 22:30] MT
G'
P'
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In his article, "Textual and linguistic problems in the book of Psalms", G. R. Driver offered (pp. 176 f.) the following explanation of $P'$s rendering, and of MT. The phrase $\text{טן תבמ}$ does not mean "the hungry of the earth" but "those wrapped up in the earth"; to support this alleged meaning of $\sqrt{\text{נָה}}$, which is not given in any dictionary of classical Syriac, he adduces such words in cognate languages as the Modern Syr. $\text{כֹּפֶן} $"arch over cradle" and Ar. $\text{כֹּפֶן} $"wrapped in a shroud". Taking $\sqrt{\text{נָה}}$ in this sense, he claimed that $P'$ supported his view that $\text{טָנָה} $ in MT does not mean "the fat ones of the earth" but "those hidden in the earth". We thus have a parallel to $\text{טָנָה} $ in the same verse. In order to arrive at the latter translation, we need only postulate a second Heb. $\sqrt{\text{נָה}} $"was hidden", comparing Ar. $\sqrt{\text{דֵּר}} $, which in certain themes (II, V) has the idea of "wrapping up".

2. It deserves to be pointed out that the justification for identifying a new root in Syriac is far less than in biblical Hebrew. What remains of the latter is relatively meagre, and it is likely that many roots occur so rarely - perhaps only once - that their meaning was forgotten but can be recovered with the help of modern developments in comparative philology. Of Syriac, however, we possess a voluminous literature, and it is correspondingly less likely that a root which has not yet been traced throughout that literature ever existed in classical Syriac.
Against the above argument it may be said that the rendering 𐤊𐤆𐤄𐤃 (for "יוון") makes good sense in itself, and can be explained without our resorting to the hypothesis that 𐤊𐤆𐤄𐤃 bears a new meaning. P' may have rendered thus because he too felt that "יוון" (in its usual sense) was incongruous, and that the hungry made a better subject than the fat.¹ Vogel quotes several more examples in which we may explain a discrepancy between MT and P' by supposing that the translator replaced an expression in the Hebrew Vorlage by one which he felt to accord better with the context. Thus whatever we may think of the identification of a second Heb. "יוון", I doubt whether P' will bear the meaning assigned to it by Driver.

ψ 23:2  

MT  𝕴 inflamm

P'  𐤊𐤆𐤄𐤃 𐤆𐤃 𐤊ServletResponse "in pastures of strength"

Does this mean "rich pastures"? The phrase in P' is suspicious.

The only other occurrence of "יוון" in ψ is in ψ 37:2, where it is rendered 𐤊𐤆𐤄𐤃 ². I therefore suggest that this was the original rendering here too, and that corruption has occurred: 𐤊𐤆𐤄𐤃 → 𐤆𐤃 𐤊ServletResponse.

¹ So Vogel (p. 48, following Baethgen): "...weil für hungrigen Leute das Essen notwendiger zu sein schien als für fette."
² However, outside ψ the usual rendering is 𐤊𐤆𐤄𐤃.
If Semkath were badly written, the change could have arisen quite easily.

\[\psi 23:5/6\] Here \(P'\) has a striking rendering:

MT

\[\begin{align*}
\text{and my cup is intoxicating, like life (??). Thy grace and thy mercy have followed me...}.
\end{align*}\]

Why the reference to \(\text{..}\) ? Dathe commented (p. 52): "hoc est additamentum nostri interpretis, idque, uti videtur, satis ineptum. Nam non video quinam sensus subsit huic comparationi vitae cum calice inebriante...

Fortasse legendum est: \(\text{..}\) " But the two-fold hypothesis that the translator added \(\text{..}\), and that \(\text{..}\) was corrupted to \(\text{..}\), is far-fetched.

Vogel (pp. 491 f.) suggests two possibilities. \(P'\)

"gibt das \(\text{..}\) doppelt wieder: 1. als Adjektiv (\(\text{..}\) vivus vel merus, immixtus), 2. als Substantiv (benignitas tua). Ob nicht das \(\text{..}\) eine nachträglich in \(P'\) eingefügte Übersetzung des \(\text{..}\) ist?"

Neither explanation is easy. The development of \(\text{..}\) to a sense not elsewhere attested (alive→pure→wine) is hardly possible\(^1\). As for the supposition that \(\text{..}\) came

\[1.\] Dathe himself found this in Sionita's edition of 1625, and would not accept it.
in from G', it is no doubt a fact that G' and P' show striking agreement in taking תק as if it were νῦν; but מ is no way to translate ᾿Αρτυς (=strongest? best?).

How then do we explain P'? The agreement of G' (אס) with P' suggests that G' is relevant; but I wonder whether this is a red herring. P' was capable of putting νῦν for תק independently of G', as in 39:7, 12; 62:10; 73:18 (in all of which G' has πλήν).

We note that the word מ appears in the next verse. This prompts us to suggest eye-skip once more as the explanation. If the Hebrew Vorlage had been laid out thus:

1. כרסי רוחה ז"ל...
2. שיבח חסד ירפורני כל כמי
... מ

the translator's eye, having come to the end of the first line, may have jumped to מ instead of בהז. With the resulting phrase מnal he did what he could, writing מnal. He then perceived his mistake and went back to בהז, where he resumed with מnal.

If this hypothesis is accepted, then the Vorlage had here a line of 19 letters and 4 spaces. Yet another case of eye-skip, giving a putative line of 18 letters and 4 spaces, is proposed in 41:10; and P' in 74:12 can be explained in terms of damage to a line of 17 letters and 3 spaces. These three examples suggest that the Vorlage
was written in lines much shorter than those found in most modern editions.\(^1\)

It may be objected that in \(\Psi 18\) we postulated a different layout, involving lines of about 40 letters. Perhaps, however, the scribe of the Vorlage departed there from his usual shorter lines to preserve the traditional arrangement. In the same way, the Pentateuch mss read in the synagogue today contain the Song of Exod. xv according to the "brick" structure (this requires a line of about 40-45 letters), but not infrequently have shorter lines (of 30-35 letters, for example) in the other columns of the scroll.

Another possible objection is that if the translator was aware of his mistake, he would surely have deleted the words \(\text{...} \). To this I would reply that he may have wished to avoid making a mess in actually obliterating the words, and merely marked them for deletion. Similarly in ms Z a line is sometimes attached to a letter which is to be deleted (Barnes, p. xxiv). Whoever copied the translation, however, misunderstood the deletion mark and transcribed those words.

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\(^1\) Although this seems at first to be too short, there are several examples of lines of comparable length among the facsimiles published by Kahle in his "Die hebräischen Bibelhandschriften aus Babylonien", Giessen 1928; these date mainly from cents. vii-ix. Even shorter lines are found in the Syriac Cod. Sinaiticus of the Gospels (cent. iv ?). Thus there is no inherent objection to the hypothesis.
The mss diverge:

\[ \text{Ua Uc} = \text{CFGHKNOQSZm} \]

\[ \text{Le} = \text{ABDEJFT, bH} \]

Rule 1 gives \( \text{לָמוֹדָה} \) as original; it is also closer to \( \text{MT} \). The reading \( \text{לָמוֹדָה} \) is another example of the substitution of "saviour" for "salvation"¹. We have seen this done by the translator; here the copyists are responsible².

\[ \psi 32:9 \]

"which one subdues with a bridle from their youth".

Perhaps the Vorlage was misread, the \( \text{יְהוָה} \) of \( \text{יְהוָה} \) being taken together with \( \text{יְהוָה} \) to give a false reading \( \text{יְהוָה-יְהוָה} \). The translator could do nothing with the remaining:

1. This could be taken to mean "the helper and saviour, even his anointed", (genitive of identity; cf. NID 205).

2. However, I think it unlikely that occurrences of \( \text{לָמוֹדָה} \) for abstract nouns, when attested in all the mss, are due to copyists rather than the translator.
Other explanations have been offered, none of which explains the omission of ידוע: (a) P' understood ידוע in the sense "youth", which has been seen in פ 103:5 (where ידוע is parallel to מירינא). There, however, P' has א"ש, a guess which would not be expected from one who was familiar with this sense. (b) Vogel (p. 208) holds that P' read ידוע, comparing Gen 48:15 (ידעוּ מ'ְּלָל), Num 22:30 (similarly). But the Mem is essential to this idiom, and we have no evidence that it was in the Vorlage. (c) Wutz (p. 73) regards כְּמוּ אֲליֵה as an inner-Syriac corruption from כְּמוּ מֵאֲליֵה ( = G MACHINEpelשׁ), representing a Hebrew ידוע, which he would read here in preference to MT ידוע. The emendations are not easy.

This verse is omitted in all the P' mss. Now, the translators are known to have allowed themselves considerable freedom, and in particular to have abbreviated the text on occasion. Often the omission seems to have been deliberate, in that expressions repeated in MT are rendered only once. However, no such reason would apply here, and it is strange that this verse should have been omitted.

Let us suppose that the verse had been in the Urtext. As it ends in MT with יִקְּרִי, the P' version would have ended כַּלּוּ בְּרָעָן. But verse 9 ends כַּלּוּ, and the resemblance is significant. In the following reconstruction of a hypothetical Urtext containing verse (10), my rendering is mere conjecture; what matters is that it would have ended כַּלּוּ
It would have been easy for a scribe who completed verse 9 to try to find his place again in the Vorlage and jump by mistake to \textit{םדבכ} by homoioteleuton\textsuperscript{2}. I therefore suggest that V.10 be restored, having fallen out in the archetype.

\[\psi 1:10\] The text of P' shows a curious repetition:

\textit{MT}: \textit{םדבכ הָנָּה שֵׁנֵי רַגְלֵי נְפָרְבִּים, בְּגֹבַּל לֹחַיִי}

\textit{P'}: \textit{םדבכ לֹא שֵׁנֵי רַגְלֵי נְפָרְבִּים}

"even the man who seeks my welfare, on whom I rely, eats my bread on which I rely."

I find it difficult to choose between two explanations of this repetition of \textit{םדבכ לֹא שֵׁנֵי רַגְלֵי נְפָרְבִּים}, which can hardly

2. It is not necessary to suppose that the lines were set out as above; but if \textit{םדבכ} and \textit{םדבכ} each stood at the end of a line, this would have been particularly conducive to homoioteleuton.
have been done deliberately by the translator.

(a) The translator may have added it inadvertently. Suppose that he had a Vorlage of lines of about 19 letters, as suggested in the note on ὦ 23:5-6.

Having read (and translated) Ἰνήμ, he looked back to find the place again, and his eye went mistakenly to Ἰνήμ (note the similar ending). He thus began the line again, but perceived the mistake after Ῥ, and continued with Ἀρεβίλι.

(b) The repetition first occurred in a copy, through a similar process; a copyist went back by mistake from Ἀρεβίλι to Ἰνήμ.

This Psalm yields an unusually high number of lexical variants, in which certain late authorities differ from what must be held to be the Urtext:

V. 3,11] יִלָּה > לַות (F Le; so Syrohex.)
V. 6] יִלָּה > לִיָּה (R Le)
V. 16] יִלָּה > לִיָּה (B Le; sim. Syrohex.)
V. 17] לָת > לָת (EJ Le)
V. 20] יִלָּה > לִיָּה (B ? J)
V. 21] לִיָּה > לִיָּה (m Ua Uc)

1. It is not essential to suppose this arrangement, but conditions favourable to such an error would be created thereby.
The tone of this Psalm makes it likely that its use in Divine Service was particularly widespread, and this would have led to variations.

\[60:10\] For \( \text{~} \), P' mss AEHLN have \( \text{~} \), whereas the others have \( \text{~} \). This illustrates a phenomenon pointed out by Diettrich\(^1\), which we now see to apply to our \( \psi \) mss in general, viz "die durch private Benutzung des Psalters hervorgerufene Ersetzung des Suffixes des 1 pers. Flur. durch das der 1 pers. Sing."

However, this must not be elevated into a principle that where the mss diverge between 1 sing. and 1 pl. we should prefer the plural; for public use of the Psalter can produce the opposite effect.\(^2\)

\[63:9\] MT \( \text{~} \), P' (most mss) \( \text{~} \) (ENT) \( \text{~} \). Intrinsically, \( \text{~} \) is surely the correct rendering of \( \text{~} \) (as in \( \psi \) 101:3 etc.); but distributional criteria tell against the hypothesis that the Urtext survives only in ENT. What seems to have happened is that the translation had \( \text{~} \), but that a copyist committed an error in the direction of greater familiarity (p. 2:12) to give \( \text{~} \), which stood in the archetype. In these three mss, the text has been corrected after G', and the reading thus brought in coincides with that of the Urtext.\(^3\)

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1. BZAW 5, p. xxxviii.
2. Thus in the Jewish liturgy for the Day of Atonement, \( \psi \) 86:17 is quoted with 1 sing. changed to 1 pl. (Routledge edition, vol 1, p. 45).
3. In the terms of Kantorowicz (p. 5), \( \text{~} \) in ENT is "richtig" but not "echt".
For $\psi 68:10$, the P' mss offer:

$\text{ABCDEGQRS}$

$\text{Le Ua Uc = GKNOM, BH}$

As $\text{ABCDEGQRS}$ is in P and four of the five available "purer-$\mu$" mss (viz CHQS > G), it is favoured by Rule 2. However, there is a good reason for preferring $\text{ABCDEF}$: we have already noted (p. 7:57) that $\sqrt{\psi}$ is the usual rendering for $\sqrt{\psi}$, that $\sqrt{\psi}$ is attested in $\psi$ only twice (viz here and in the next verse), and that in both passages there is a rival reading from $\sqrt{\psi}$. I therefore depart here from Rule 2, to accept $\text{ABCDEGQRS}$.

$\psi 68:19$] This is perhaps the best-known passage in the Peshitta Psalter. MT has: $\text{יִתְנָלַ֛ה דּוֹמָא} \text{ לִבּוּ}$. The P' mss diverge:

\[
\begin{align*}
\text{יְפִּ֖תָה} & \quad \text{יְפִּ֖תָה} \\
\text{Le=AB?CDEGJ} & \quad \text{sup ras} \\
\text{Lsup ras QRS, BH} & \\
\text{Ua Uc=FKNOM} &
\end{align*}
\]

The passage is discussed by Barnes (pp. xlii f.). Although $\text{יְפִּ֖תָה}$ agrees with MT, he pointed out that it may have been introduced from G' (or from Syrohex.). On the other hand, some doubt is thrown on the Western $\text{יְפִּ֖תָה}$ by the fact that $\psi 68:19$ is quoted or adapted in Ephes. 4:8 in the form $\text{δώκιν δοματα των ανθρώπων}$. He was inclined to choose $\text{יְפִּ֖תָה}$ as the original reading, because:
(a) it is found in A and C, which are far older than any ms attesting its rival;

(b) it is more likely that the mss of P' were influenced by the Syrohex. than by the New Testament. "The influence of the Yaunāyā in other passages of the Psalter is an established fact, the influence of the New Testament is only a probable inference."

Thus he regarded the presence of λπα in F as due to revision after MT, and its Nestorian attestation as the result of assimilation to G'.

He notes also that κπαν is the reading of T', and that λπα would have been the more acceptable reading on theological grounds: "God does not 'receive'; He gives!"

If we no longer believe that F (or a source of F) underwent revision after MT, we shall not find it so easy to explain why F has λπα. Indeed, Rule 1 will contradict Barnes' choice, and recommend that we regard λπα as the original reading.

Against λπα it may be said that it is objectionable and therefore unlikely, and that its presence in F and the Nestorian witnesses could be explained in terms of G' influence or scribal error independently affecting F and the υ-text. But is it really so objectionable? The opposition is not simply between √λπα and √λπα; for all the mss agree in having after the verb the preposition Lamadh. Now ... \(\text{λπα}\) is theologically quite acceptable; it can mean
"bring unto", which is close to "give", e.g.

Gen 27:9

Ex 27:19

Thus I see no good reason to depart here from Rule 1, and I take בוֹל as original. The translators succeeded in producing an acceptable rendering, not by changing the content words, but by varying the grammatical morpheme Beth. This is consistent with their behaviour elsewhere (see e.g. pp. 9:20 f.); they are usually careful in their treatment of "lexical" morphemes, but accord less respect to "grammatical" ones. The reading בוֹל then may have arisen either by scribal corruption or (more probably) by assimilation to Ephesians.

\[\text{MT: } \text{This may be over an erasure in A} \]

\[\text{P' mss: } \text{renders } \text{, but } \text{renders } \text{......} \]

1. I suggest that the influence of the NT on the mss is not merely a "probable inference", as Barnes stated; see e.g. our treatment of in \(\psi\ 110:4\) (p. 7:58 f).
shows great similarity to \( \psi 44:5 \):

\[ \text{MT: } \text{בַּעַלְתָּא} \]

One might have thought that AT preserved the Urtext, and that the reading in the other mss had been conformed to \( \psi 44:5 \). However, Rule 1 tells us that it is unlikely that AT alone should have the original. This is confirmed by a linguistic fact: the usual rendering of \( \text{ברל} \) is \( \text{ךץ} \), and the noun \( \text{ךץָּא} \) is found nowhere else in \( \psi \).

So the majority reading is the older, and we may take it to have stood in the archetype. Is it possible that the original was close to MT, and that the present text is due to assimilation to \( \psi 44:5 \)? It is difficult to see any motive for which this might have been done deliberately; on the other hand, if we suppose that it was inadvertent, and that a scribe wrote the words from \( \psi 44:5 \) in a careless moment, then it is strange that the phrase \( \text{ךץָּא} \) – for \( \text{ךץָּא} \) (\( \psi 77:12; 143:5 \)) – should have become embedded in this fragment of \( \psi 44:5 \) which had come into his mind. I wonder, therefore, whether the present reading goes back to the translator himself, and suggest the following hypothesis.

The Hebrew Vorlage was damaged over the words \( \text{ךץָּא} \). Perhaps they formed the last line of a page which had been somewhat mutilated; as they contain 17 letters, they could well have made up a line. From what remained of \( \text{ךץָּא} \), the translator thought he could
make out "אשרועה יִעְקָר", and decided that he could supply the rest of the line from יִקְמַי: 5.

MT מקום菲尔 יִשְׁרָעְתָּה בַּכֹּרֵב הָאָרֶץ
damaged text ??? יִשְׁרָעְתָּה בַּכֹּר? ??? | McKim

translator's reconstruction ??? יִשְׁרָעְתָּה יִעְקָר? ??? | McKim
text as filled out by him צורה יִשְׁרָעְתָּה יִעְקָר

translation נַעֲשָׂה נְעוּשָׂה נַעֲשָׂה נַעֲשָׂה

The text of AT represents a later attempt to conform P' to G'.

76:5] For MT יִרְאָה (G' θαυμαστὴς ), the P' mas offer:

Le = BCFGHRZ, bH

Ua Uc = D [E] JKLNOQSTM

(sic) A

Let us examine the intrinsic evidence. A's reading is nonsense; and neither שְׁמַע nor שְׁמַע is found elsewhere for יִרְאָה, which is rendered elsewhere in י by (7:2, 10; 16:3; 93:4).

1. In 136:18, it becomes Ⱁ.
The following hypothesis, I submit, will account for these facts. P' rendered אֶפְּנָא in the Urtext, but a scribe added involuntarily an extra stroke in copying it, and produced the nonsense-word אֵפְנָא, which was the reading of the archetype. It invited emendation; some omitted the Mem (א'), others the Beth (א' - naturally a popular reading among Christians). Only in Cod. A was the reading of the archetype preserved.

ψ 80:17] MT

Here the Psalmist is entreating God on behalf of Israel, represented in the figure of a vine. The majority reading seems hostile: "Burn her shoots with fire!"², whereas גֹּמָא is a complaint: "They have burnt her shoots with fire" (3 pl. equivalent to passive). Rule 2 gives an advantage to גָּמָא (in F and QS, but not in C¹H), and indeed it is more likely that the reading sympathetic towards Israel (גָּמָא) came before the other, which marks a hardening of attitudes.

1. Stoll (p. 102) calls this "graphische Perseveration". If I may try to explain the phenomenon in layman's terms: It sometimes happens, when a man who has been carrying out a certain action continually (such as the formation of letters) decides to stop, that the body does not obey the instruction immediately but repeats the action involuntarily once or twice before ceasing. Stoll attributes fifteen errors observed in his experiment to this cause, e.g. the writing of m for n.

2. גֹּמָא can hardly mean "he hath burnt", for it follows the series of imperatives גָּמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָא...וְגוֹמָา
"and thou didst cause to cease those who had made him victorious". Evidently P' vocalised רְויָּה, but although √ לַיָּה suits the context (defeat in war), it is hardly a translation of √ לַיָּה. I would prefer √ לַיָּה, which frequently renders √ לַיָּה both in ш and elsewhere, and suggest מְלוּשָּׁה, from the well-attested לְשָׁה (Payne-Smith 896: lustratorius, purificans²). The change from Dalath to Zain could well be an aural error, which also gave a reading suited to the context.

Most P' mss have מַהְלָךְ מִכָּה, but in B the last word is מְלָךְ מִכָּה, which seems to have come in from מַהְלָךְ מִכָּה. Two expressions in MT, מַעֲרָה בְּלֶא הַנִּה שָׁרָה have been interchanged in P': מַעֲרָה בְּלֶא הַנִּה שָׁרָה. Such transpositions are not uncommon in the Peshitta Psalter (see Vogel, p. 48, under "Umstellung von Wortpaaren oder -Gruppen"), and so we cannot follow BH in adducing P' in favour of the emendation שְׁלֵשׁ מִכָּה.

Most P' mss have מַעֲרָה מִכָּה. However, instead

1. √ לַיָּה can also mean "to be unimpeachable, innocent", but does not have the essentially sacerdotal character of √ לַיָּה.

2. One phrase quoted there is מַעֲרָה מִכָּה.
of the latter word we find \( \text{כֹּסָדָּלָּא} \) (from \( \text{כָּדָל} \), which can mean "altar" or "burnt-offering") in GT; Barheb. tells us that it is also a Nestorian reading. It does not represent an alternative rendering (based on \( \text{נִלּוּל} \)?) but an assimilation to \( \psi 78:58a \) (\( \text{כֹּסָדָּלָּא} \) \( \text{כֹּסָדָּלָּא} \)) where MT has \( \text{נִלּוּל} \).

\( \psi 108 \) Most of the verses in this Psalm find parallels elsewhere (VV. 2-6 = \( \psi 37:8-12 \), and VV. 7-14 = \( \psi 60:7-14 \)). There are certain differences in the P' renderings of parallel passages:

(a) \( \text{יִהְיֶה} \) \( \psi 108:2,2 \). \( \text{בָּלִים} \) \( \psi 57:8,8 \). (MT \( \text{יִכְבּ} \))

(b) \( \text{חָמָם} \) \( \text{נָשַׁב} \) \( \text{נִשְׁבַּב} \) \( \psi 108:5 \)
(\( \text{חָמָם} \) \( \text{נָשַׁב} \) \( \text{נִשְׁבַּב} \) \( \psi 57:11 \)
(MT \( \text{גָּרַע} \) \( \psi 108 \) \( \text{רַע} \) \( \psi 57 \))

(but in both places P' read \( \text{רַע} \))

The use of a verb in \( \psi 57 \) and an adjective in \( \psi 108 \) seems to follow G' (\( \text{שְׁתִי} \) \( \text{מֵעָגָלָנָה} \) ... \( \psi 57 \), \( \text{שְׁתִי} \) \( \text{מֵגָה} \) ... \( \psi 108 \)).

1. This difference in the passages in G' is found in most of the G' mss. We note that P' in \( \psi 108 \) has an adjective, like G', but disagrees on the subsequent preposition (\( \text{ἐπίκαι} \) \( \text{τῶν} \) \( \text{οἴνοι} \), \( \text{ὥσπερ} \) \( \text{ὡς} \)).
(c) מְנַבֵּרָה יָסָרֵי 108:7, דִּבּוֹרָה יָסָרֵי 60:7 \(^1\) (MT יָּשָׁרֵי)

(d) מָלֵא יָסָרֵי 108:8, דִּבּוֹרָה יָסָרֵי 60:8 (MT יָּשָׁרֵי)

(e) מְנַבֵּרָה רְאוֹפֶה 108:10, דִּבּוֹרָה רְאוֹפֶה 60:10 \(^1\) (MT יָּשָׁרֵי)

(f) In יָסָרֵי 60:11, the second half of the verse appears in P before the first.

(g) מְנַבֵּרָה יָסָרֵי 108:13, דִּבּוֹרָה יָסָרֵי 60:13 (MT יָּשָׁרֵי)

(h) מְנַבֵּרָה יָסָרֵי 108:13, דִּבּוֹרָה יָסָרֵי 60:13 (MT יָּשָׁרֵי)

Thus יָסָרֵי 108 was translated independently of יָסָרֵי 57,60. I cannot say, however, whether these variations could be due to one translator at different times, or whether they indicate different translators.

112:10] For MT יָּשָׁרֵי, most P\(^{\prime}\) mss have מְנַבֵּרָה. Certain late Western authorities (viz B, BH, the Polyglots and Le, Erpenius) have דִּבּוֹרָה. This is an interesting phonetic error (Dalath being devoiced, and fricative Heth being replaced by plosive Qof).

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1. The other reading has been introduced into one or two later Western witnesses (B, E or Le).
ψ 118:17] For דֹּאֶד, דָּוָּד ( = MT, G'), B has דֹּאֶד, דָּוָּד. This may have come in from a number of passages where דֹּאֶד, דָּוָּד appears, e.g. ψ 16:8 (דֹּאֶד, דָּוָּד וְהָאָסָּא).

ψ 119] The P' version of this ψ differs from MT in verse arrangement:

(a) V.91 has been omitted;

(b) instead of V.119, we find a second rendering of ψ.117, with somewhat different wording;

(c) between ψV. 151 and 152, V.148 is repeated (with unchanged wording);

(d) ψV.171 and 172 have exchanged places.

I wonder whether these dislocations may go back to the translator. He was labouring under conditions particularly conducive to errors. First, we note that all these changes occur in the second half of this very long ψ; if he was translating the whole ψ in a single session, he must have been fatigued at this stage. Second, the acrostic system of the Hebrew, yielding series of eight consecutive verses each beginning with the same letter, made it easy for him to lose his place. Now each of the four

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dislocations concerns verses within a single eight-verse block, no "boundaries" being crossed, viz (a) § , (b) ¶ , (c) ¶ , (d) ¶ . I would therefore attribute all these changes to the fatigue of the translator, who was affected by homoiarcteon, i.e. when he referred back to his Vorlage after writing down his translation, he sometimes picked up again at the wrong point.

ψ 119:158] MT יָשָׁשׁ אֲלֵה הַמַּחְמָרֵי יָשָׁשׁ פ' This rendering (אָפָה for יָשָׁשׁ) is mentioned by Prof. D.W. Thomas in his "Additional notes on the root יָשׁ in Hebrew" , pp. 56 f. Acknowledging his indebtedness to Prof. Sir Godfrey H. Driver for the references, he pointed out two passages in P' where the Syriac יָשׁ was claimed to have a meaning ascribed by Thomas to the Hebrew יָשׁ ("be submissive").

The P' rendering here "can hardly mean 'and I knew', but rather perhaps 'and I was still, quiet, submissive' (through vexation, grief)" (p.57). But we may counter Thomas' objection by stating that 'and I knew' makes good sense in P': "I saw the wicked, and I knew that they did

1. JTS (1964) pp. 54-7.
not keep thy word." The reason for which it is claimed that P' can hardly mean 'and I knew' is that this is not the meaning of MT. Thus Thomas assumed that P' understood MT well, so that P's rendering must bear a similar sense to MT. But perhaps Thomas, in making this assumption, overestimated the knowledge of the P' translators. Baethgen listed (p. 429) several examples of renderings which seem to be based on guess-work, and there are many other instances in the Peshitta Psalter. On one occasion מַקְלַן itself is introduced as a guess, viz in פ' 139:3.

Now מַקְלַן occurs only twice in פ', and the rendering in the other passage ( פ' 139:21) is not accurate enough for us to be able to discount the possibility that the translator was unsure what מַקְלַן meant. As long as that possibility remains, I submit that מַקְלַן is likely to be a guess, albeit off the mark.

Let us take the opportunity of considering the other passage, Ezra 4:13. Here we have:

MT: מֶנָּה-גֶּלֶב לֹא וַתְּנַבְּרָה לְךָ אַחֲרֵיכָם מִן-הָעִם. פ': (ed. Lee) מֶנָּה-גֶּלֶב לֹא וַתְּנַבְּרָה לְךָ אַחֲרֵיכָם מִן-הָעִם

1. Prof. J. A. Emerton, "A consideration of some alleged meanings of מַקְלַן in Hebrew", Journal of Semitic Studies (1970) pp. 145-180, mentions both passages (p. 156). On פ' 119:158 he comments: "...While it may be argued against Thomas that the meaning 'and I knew' does make sense in the context, the fact remains that it is a surprising rendering of the Hebrew and that the difficulty is solved if his suggestion is accepted". But the discrepancy here between MT and P' would constitute a difficulty only if it exceeded the level of discrepancy to be observed elsewhere.
Thomas doubted whether "know" was appropriate here; he suggested that we point לַעֲדֹת not as Peal (לַעֲדֹת - so Walton) but as Apel (לַעֲדֹת), and that we translate: "moreover this will not quieten, subdue, kings".

Now if לַעֲדֹת - however vocalised - is correct, then פ' is difficult; but we can make some sense of it without resorting to a new Syriac root לַעֲדֹת. Walton translates: "Quin etiam reges non agnoscerat", and similarly C.A.Havley writes (p.38) that פ' "paraphrases MT = 'and the royal taxation will suffer damage' by 'neither will she (i.e. the city) recognise kings, i.e. המלך for מלך... פ' is then very freely translated" (my italics).

But the ms evidence leads us to doubt whether לַעֲדֹת is sound in any case. There exists an apparatus criticus to the book of Ezra, in an article by C.Moss. Of the five mss which Moss cites in this passage, none agrees with Lee in reading לַעֲדֹת; instead we find

לַעֲדֹת in A (7a1), 0(8h5), s(17e1) \{ both without \}

לַעֲדֹת in F2 (16/9a1), e(17a1)\4

1. This would be quite anomalous, the regular form being לַעֲדֹת.


4. Further evidence of this reading appears in Thorndyke's collations (see vol. 6 of the London Polyglot), which give, against the lemma לַעֲדֹת: Poc. omittit לַעֲדֹת, item Uss.
Thus two of the three most ancient mss of P' have שָׁנָה, which gives a straightforward if vague sense: "moreover this will disturb kings", such as one might expect from a translator confronted by the difficult Aramaic original. It is likely that שָׁנָה is original, that it became corrupted in some mss to שָׁנָה, and that, by virtue of the obscurity of the latter reading, שָׁנָה was introduced in certain texts (e.g. London Polyglot, Lee) to create tolerable sense. Thus the value of this passage as evidence of a new Syriac root נָבָה, as posited by Thomas, is slight.

ψ 119:176 | MT רָבָּה יָעַמ | G' נָבָה בְּאֶרֶץ פִּיו | Syrohex. נָבָה כְּנֶפֶשׁ | . The P' mss have:

Ua Uc = KLOXm

Lo = ABEFHJQRTZ, bH

N has נָבָה כְּנֶפֶשׁ; C נָבָה כְּנֶפֶשׁ, but with נָבָה מָיִם over an erasure; the first hand may well have written נָבָה.

Rule 2 suggests that נָבָה כְּנֶפֶשׁ (in F + HQZ but perhaps not in C) be preferred. However, that reading agrees exactly with Syrohex, which of course counts against it; and I would choose here נָבָה כְּנֶפֶשׁ (Nest. and perhaps C).

1. The third (8a1) is not cited in this apparatus.
Here ACF alone have ... with 1 pl. endings as in MT ... whereas the other mss have 3 pl. m. (... ) reflecting G' ( ... ol ... at Γνωστὴς αὐτῶν... ) We may presume that ACF have the true reading; this possibility is admitted by Rule 2. It seems that the 3 pl. had become widespread by cent. v, and that virtually all the ν-mss and many of the μ-mss carried it. Thus 3 pl. is in all the Nestorian witnesses (Ua Uc = KLNmx) and most Western ones (Le = BDEHJQTZ, bH). However, 1 pl. survived in Ur-F and in a few μ-mss; hence it is represented today in ACF.

To sum up: The intrinsic evidence often supports, and only seldom contradicts, the editorial policy laid down by our map. Thus the method here proposed can claim a degree of success.
THE USE OF COMPUTERS ETC. IN

THE TEXTUAL STUDY OF THE PESHITTA

The two outstanding desiderata in Peshitta studies today are (i) a critical edition, (ii) a concordance. The Peshitta Institute in Leyden has undertaken the former task\(^1\), and moreover become a source of information on studies in P\(^\text{'}\) (and indeed in the other versions) for scholars all over the world\(^2\). As for the task of preparing a concordance - which I believe to be of comparable importance to the critical edition itself - concordantial glossaries do exist for a few books\(^3\) and Prof. De Boer tells me that the Peshitta Institute is interested in compiling a concordance to the whole O.T. Both these tasks involve considerable effort of a mechanical nature, and we may ask how much of this can be delegated to electronic machines, including computers.

1. At the time of writing (July 1973), the final edition has appeared of the Canticles or Odes, Prayer of Manasseh, Apocryphal Psalms, Psalms of Solomon, Tobit and 1(3) Esdras. Song of Songs and IV Ezra were published in a sample edition (1966).

2. I would mention in particular Dr. W. Baars, who keeps a full and up-to-date bibliography.

3. For the O.T. proper, see: A. Lazarus, "Zur syrischen Uebersetzung des Buches der Richter", Kirchhain N-L 1901 (pp. 32-71). E. Rosenwasser, "Der lexikalische Stoff der Königsbücher der Peschitta", Berlin 1905. L. Techen: op. cit. (see p. 7:1). Some time ago, Heer T. Sprey told me that he was compiling such a glossary for Daniel; other co-workers in the Leyden project may well be doing the same for other books.
The essential point about the use of computers is that they have to be programmed; that is, we must have a precise idea of the method to be used in solving the problem, so that we can give the computer adequate "instructions". Thus, when a mathematician solves an equation by computer he knows all the steps involved; the reason that he uses a computer instead of performing these steps himself is that the computer can store far more information and can work at enormous speed and with less risk of making slips. Computers in themselves do not solve problems; they execute procedures worked out by the programmer.¹

Let us take first the preparation of a concordance. We shall presumably require the concordance to list the Syriac forms not in simple alphabetical order but in order of the roots from which they are derived, i.e. more or less as in Brockelmann's Lexicon. A student who is investigating the Syriac יִּלּוֹנֶשֶׁת will not want to look up in several different places the different forms יִּלּוֹנֶשֶׁת, יִּלּוֹנֶשֶׁת, יִּלּוֹנֶשֶׁת, יִּלּוֹנֶשֶׁת, יִּלּוֹנֶשֶׁת etc.; he will expect to find them all under יִּלּוֹנֶשֶׁת. Again, he will expect יִּלּוֹנֶשֶׁת "sword" and יִּלּוֹנֶשֶׁת: "desert" to be listed separately; similarly יִּלּוֹנֶשֶׁת "he will open" and יִּלּוֹנֶשֶׁת "Jephthah". Thus at some point in the process, every word in the text must be parsed so that it can be brought under the appropriate heading².

² On analogous problems in the Greek text of the NT., see Fischer, p. 299.
I believe that parsing can be done far more efficiently by a human than by a machine. It would be a formidable task to write a series of mechanical instructions for parsing any Syriac word; and the computer program would be so complex that the human brain would probably take less time to identify the form. Thus I doubt whether much would be gained by transcribing the P' text by itself on to punched cards. What might be worth while is to parse the words of

1. I have discussed the analogous problem of "computerising" Hebrew texts with Edward James, not only a lecturer in Computing at Imperial College London but also a competent Hebraist. He estimated that a program to parse a Hebrew word would take several minutes (or even hours) of computer time to be executed; and the same must be true for Syriac. Furthermore, there would sometimes be more than one possible solution; for example, is Ethpeel from blame or "restrain"? Common sense would usually guide a human investigator to the right alternative; but it is a quality of which the computer has no part.

2. G. E. Weil and F. Chénique, "Prolégomènes à l'utilisation des méthodes de statistique linguistique pour l'étude historique et philologique de la Bible hébraïque et de ses paraphrases", Vet. Test. (1964), pp. 344-366, proposed to put the Hebrew O.T. on punched cards, giving the consonants, vowels and accents (pp. 351 ff.) but apparently without parsing. They further envisaged encoding the versions in the same way (pp. 365 f.). C. Hardmeier, "Die Verwendung von elektronischen Datenverarbeitungsanlagen in der alttestamentlichen Wissenschaft", ZAW (1970), pp. 175-185, seems to accept the proposals of Weil and Chénique (pp. 183 f.), and to believe that it is a practical possibility that the Hebrew text thus encoded be "morphologisch analysiert, d.h. vom Computer verarbeitet". However, I fail to see how the computer could identify the Hebrew words according to their roots, grammatical affixes, etc., and hence yield the sort of information which most investigators require. Indeed the problems which Hardmeier himself hoped to solve on this basis (pp. 178 ff.) could not be dealt with by a computer supplied with such data alone.
the text one by one, giving as many details as are likely to be required. This would involve our devising a code to represent a Syriac word thus analysed; the task is likely to prove more difficult than one might have at first envisaged, but not insuperable. This information could then be punched on cards. A suitable sorting program could then be written to print out each form under its root and in the appropriate place, and to list every passage where that form occurs.

The concordance thus obtained would be inferior to, say, the concordance of Hatch and Redpath to the Septuagint, in two ways. First, we would have only Syriac forms; but we must also know the Hebrew forms to which they correspond. Second, we would have only the reference; but a satisfactory concordance should also give us some idea of the context. Both problems can best be solved, I believe, if we include this additional information in our encoding. The Hebrew word would not have to be described in as much detail; no doubt scholars could agree on a policy. As for the context, we need only add a pair of numbers, e.g. (2,1) to mean that the machine should print out a phrase beginning two words before the word under analysis and ending one word after. If the cards, augmented with this additional information, were then submitted to the computer, a very useful concordance would be produced.

The encoding would be arduous, and if we were interested only in a concordance, we might well decide not to get involved with the computer but to work by hand. Nevertheless

1. Several scholars could co-operate, and their work could be co-ordinated by a general editor.
to have the Peshitta thus encoded would open up many possibilities, because we could then write a program to search for any phenomenon which interested us. Thus we could instruct the computer to list every instance in P' of a construct followed by a preposition, or of a Hebrew noun being rendered in P' by a verb, and so on. The degree of detail employed in the original encoding will be dictated by the sort of searches which we envisage performing by computer. Whether this considerable investment of effort— which could be shared out among many scholars if a common policy were evolved—would be justified, is a matter which will have to be discussed. I cannot, however, see any real short-cut if the facilities to be obtained are to meet the requirements of the modern researcher.

We now ask whether electronic machines could be used in the collation of P' mss. There are two phases to consider: (a) Can an "electric eye" scan the mss (or facsimiles thereof), so that we are spared the labour of reading each ms? (b) Can the computer help in the task of preparing a reliable apparatus, and of storing in easily accessible form the ms groupings throughout the text?

Let us deal first with (a). This requires that the machine be programmed to read a ms. The difficulty is that the process of reading handwriting cannot be easily reduced to mechanical terms. The reader of a ms will draw continually on his experience. He will "get used to the writing", that is, he will learn how the scribe makes distinctions between different symbols, and what variation
to expect in different realisations of the same symbol. Sometimes an individual letter would be uncertain if one were confronted with it in isolation, but it can be confidently identified through the word in which it occurs. All this depends, as I have said, on the experience of human beings; and the recognition processes are far too subtle to be translated into purely mechanical instructions.

Very different is the collation of printed books, in which every symbol assumes exactly the same form each time it occurs. V. A. Dearing records that "Charleton Hinman adapted an optical device used for comparing aerial and sidereal photographs so as to make possible for the first time speedy and accurate comparisons of books that differ only in minute re-settings of their type" (p. 1). But handwriting in Syriac mss - if I may judge from those I have seen in the British Museum - is not regular enough for a machine to be able to identify which symbol a given sequence of handwritten strokes represents.

1. The recognition of symbols in a ms is in these respects analogous to the process whereby the hearer recognises the phonemes of spoken language as pronounced by different speakers.

2. Familiar examples are the letters Lamadh (✧), Ayin (✧), Nun (✧); or Heth Yodh (✧) and Yodh Heth (✧). A sequence of strokes which represents one of these alternatives in one situation could represent another elsewhere; and although a human collator would readily take in the situation, the machine would not be reliable.

The position regarding (b) is much more hopeful, to judge from Fischer (pp. 307 ff.). The comparison of each ms with a pre-determined "basic text" is best done in the traditional way; each of these collations is then coded and punched on a tape. This means, inter alia, that we can readily find all the places where the mss group themselves in any given manner; that the ms reports are transformed quickly and without error into a full apparatus criticus; and that any statistics we may require (e.g. distances between ms pairs) can be easily obtained.

Thus two principal tasks in the preparation of a critical edition, viz reading each ms and comparing it with the basic text, will still have to be done by hand. When there are a great number of mss to collate, as in the case of the Peshitta¹, this will still be formidable. Yet I believe that the methods proposed here can go some way towards lightening the critic's task by providing him with an objective basis on which he can select for collation a relatively small sample of mss and be more or less confident that all variants of value will be represented in that sample.

Let us review what has been attempted in this thesis. The ground had been prepared by Barnes' collation of less than a tenth of all the mss available. Those portions of Barnes' apparatus which were deemed suitable were fed into the computer; this yielded a map which allowed us to classify

¹ Fifty is a typical figure for the number of mss carrying the text of an O.T. book. Virtually all the mss for each book (except ￥, attested in about 300 extant mss) are to be collated, on the grounds that only in a few exceptional cases do we have as yet any objective reason for failing to collate any ms.
the mss and to estimate the potential usefulness of a ms from its position on the map. We should not now find it difficult to form an opinion of a new ms if we were to examine it in an adequate number of test passages; given our knowledge so far, we could readily determine whether the new ms represented a type which would add little to our knowledge (e.g. in $\psi$, a "poor relation of the Buchanan bible", in Barnes's term)², or whether it had much independent value. Thus we would be spared in many cases the trouble of collation. A similar policy could be tried in other books: (a) a preliminary collation of about 20 mss, (b) map analysis yielding a textual history and classification of the 20-odd mss, (c) examination of the rest of the mss in the light of what had been achieved so far, to see in each case whether a detailed collation would be justified.

It is well-known that a student who has devoted his energies to a particular problem will tend to see everything in terms of his own field; the reader will no doubt make allowances for this. However, I do feel that the approach developed here may prove its worth in harnessing modern techniques for the problems facing students of the Peshitta today.

1. But "adequate" is the operative word. Cf Pasquali, p. 36: "Ma il peggior metodo di tutti (sc. for deciding on the value of a ms) è quello di contentarsi, anche solo per i codici più tardi, di un saggio unico o di saggi troppo scarsi."

This last Chapter of Section B is devoted to an examination of other text-critical studies of a numerical nature. My purpose has been, in the main, to bring these alternative methods together, to explain them in terms which will be intelligible (or so I fondly imagine) to the literary and the mathematical specialist alike, and to offer my own appraisal of each method.

An additional purpose which this Chapter is to serve is rather more formal. A candidate who presents a Thesis is required to indicate "in what respects his investigations appear to him to advance the study of his subject". The fact that I feel that original matter is distributed more or less evenly throughout the Thesis, leads me to fear that a great deal of inconvenient repetition - and not much else - would be the result were I to seek to fulfill this requirement by means of an explicit statement. Instead, I hope that this review will enable the reader to survey the ground which other investigators have gained so far, and hence to assess for himself whatever contribution this Thesis may represent.

The term "numerical" has been taken in a very wide sense, to include any method which involves more than a trivial element of mathematics. In most instances, but not all, we find that statistics of some kind are compiled and processed. I have included every such study that I have encountered. Whether the list is complete, however, may be doubted, for the material is scattered in books and articles which find themselves classified under exceedingly diverse headings; and even if my list were complete at the time of writing (March 1973), it would soon become out-of-date. Nevertheless I have no doubt that the compilation of the material which follows has been well worth the effort.

It proved convenient to divide the chapter into three parts. Part A describes methods whose aim is to exhibit the relationships between the mss in the form of a stemma. Other approaches, which attempt to reduce these relationships to order without appealing to a stemma, are considered in Part B. Within both these Parts, I have presented the various methods in chronological order of publication. In the final Part, I consider two methods of analysis which have proved valuable in relation to analogous problems in other fields but have not been applied, in any published work that I have seen, to the study of manuscripts.

The reader is recommended to refer to certain other "review" articles on related themes. R. Marichal, "La critique des textes", offers an informative survey of studies in textual criticism generally; unlike us, he is not primarily concerned with numerical strategies, and most of the methods here examined were too specialised (and in many cases appeared too recently) to come to his attention. Equally valuable is the sixth chapter, entitled "Modern Methods of Textual Criticism", of B.M. Metzger's "The Text of the New Testament". The archaeologist too has much

1. For example, I have discussed the contribution of V.A. Dearing, in which the mathematical element is a matter of graph theory rather than statistics. In this respect, the word "numerical" in our title is perhaps misleading, but it seemed the most convenient term on most other considerations.

2. in the volume "L'histoire et ses méthodes", Bruges 1961 (being the eleventh volume of Encyclopédie de la Pléiade), pp. 1247-1366.

to teach the textual critic who would utilise numerical approaches: G.L. Cowgill \(^1\) offers a description and appraisal of three powerful techniques, and some interesting results and conclusions are presented by F.R. Hodson et al.\(^2\). One must bear in mind nevertheless that the value of a method within one discipline is not an infallible guide to its value in another.

For ease of reference, I have listed the different treatments discussed in this Chapter, together with the number of the page on which our discussion begins.

Part A

| Method of Dom H. Quentin | 3 |
| Method of V.A. Dearing | 34 |
| Method of P. Canivet and P. Malvaux | 49 |
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Part B

| Methods of the "mapping" type | 109 |
| The theory of "disconnexions", due to M. Bevenot | 113 |
| Seriation and the work of J.G. Griffith | 123 |

Part C

| Hierarchical clustering | 142 |
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References

"Essais de critique textuelle", Paris 1926.

Both books stimulated a great deal of controversy. A bibliography of eighteen studies on Quentin's methods was compiled by P. Collomp, "La critique des textes", Strasbourg 1929, pp. 72ff.

Three articles expressing criticism of Quentin's procedures seem to me to be of particular importance:


Despite these and other criticisms, Dom Quentin's method has by no means ceased to enjoy approval in many quarters. J. Burke Sievers, "Quentin's theory of textual criticism", in English Institute Annual (1941), pp. 65-98, believes that its fundamental principles are sound, but that Quentin's own development of them was insufficient and in some respects misguided; if the system were properly revised and improved, he concludes, it would become "an indispensable aid to future editors". Marichal too, writing in 1961 (op. cit., pp. 1285-1291), gives a sympathetic account of Quentin's method, which he believes to be in many ways superior to that of common errors. Another favourable critic is G.P. Zarri, "Il metodo per la recensione di Dom H. Quentin esaminato criticamente mediante la sua traduzione in un algoritmo per elaboratore elettronico", in Lingua e Stile (1969), pp. 161-182. Zarri prefers Quentin's approach to that of Maas for many reasons, in particular because the former deals - at least initially - with the "objective" data alone; he believes moreover that the labour which Quentin's approach would involve could be greatly facilitated with the help of the computer. Like Sievers, he points out certain difficulties ("inconvenienti"), but feels that the method is basically valid. A short account (in English) of attempts to apply it to a textual tradition, by Zarri in collaboration with E. Maretti, appeared in La Ricerca Scientifica (1968), pp. 1333 ff.
The methods which we are about to describe were devised by Dom Henri Quentin for use in the task entrusted to him in 1907, namely the preparation of a critical edition of the Vulgate. So complex is the textual history of that work¹, the relations between the mss having been continually entangled through contamination, that Quentin found that the time-honoured methods would not suffice and that it was necessary to invent a novel approach. His aim was to express the relations between the mss in the form of a family-tree, and thence to derive what he called a "règle de fer" whereby one might choose between rival readings. In his efforts to accomplish this task, he enunciated a good number of original ideas on textual criticism, which have come to enjoy a great deal of respect today².

His first objective was to form a general impression of the groups into which the mss fell. Much could be learned (Essais, pp. 72 ff) from those places where a small group of mss shared a reading different from that of the majority ("variantes à témoins rares"); if a particular small group of mss was found to agree against the rest


². On the other hand, I find myself not altogether out of sympathy with the complaint of Chapman (p. 8n.): "...in his effort to systematize, [Dom Quentin] has explicitly or implicitly denied almost every canon of textual criticism which I have been accustomed to revere".
repeatedly, then they could be classified together. ¹ To supplement these considerations—which were of course already well-known—Quentin recommended that a table be drawn up showing the number of agreements between each possible pair of mss, over the collection of variants admitted to consideration.² Inspection of this table, he maintained, would help us form some idea of the affinities of the mss (Essais, pp. 70 ff). This is the earliest instance known to me of an appeal to such a table of statistics within the field of textual criticism. The table of agreements— together with the closely allied table of disagreements or distances³—has since been employed by a great number of investigators;⁴ within this thesis it has of course proved indispensable.

1. From the example which Quentin offers (p. 73), it emerges that, in order to be counted as a member of the group, a ms did not have to join the group on every occasion when the majority of its members had a common reading different from that of the other mss, provided that it did so on the majority of those occasions.

2. His policy was to limit himself to those variants which lay in substantial matters and would not have depended primarily on the habits of the individual scribe (hence he excluded variations of spelling, dogmatic corrections, etc.).

3. The two measures are connected by the simple relation:

$$\text{No. of agreements (between a given ms pair)} + \text{no. of disagreements} = \text{total no. of variant passages.}$$

Thus each is a straightforward function of the other.

4. For a list, see Thes., p. 3:4.
These preliminaries completed, we proceed to the stage which lies at the heart of Quentin's system, namely the comparison of the mss in groups of three. The concept fundamental to this operation is that of an intermediary, which term we now define.

Consider three mss ABC, and suppose that their relationships to one another can be expressed in the form of a family-tree¹. Let us trace, along the lines of that tree, a path from A to C. If it is found that B lies along that path - if, in other words, it is not possible to get from A to C without passing through B - then B is said to be intermediary between A and C. In that there are several possibilities of orientation, the historical relationship between ABC may be any one of the following:

![Diagram of family tree with possibilities](image)

(i) (ii) (iii) (iv) (v) (vi)²

Fig. B.11.1

1. This involves the first two assumptions of Thes., Ch. 1, viz "unique original" and "unique exemplar", which are not unrealistic (pp. 1:9 f); as we have said "family-tree" and not "stemma", there is no need yet for III ("no subsidiary sources").

2. This tree, which appears to violate assumption II, will be discussed below.
but all these figures could have been formed by twisting in different ways a wire marked $A----B----C$.

The importance of the intermediary lies in the following proposition ($\alpha$):

If $B$ is intermediary between $A$ and $C$, then we shall find, over a sufficiently long text, a number of places where $AB$ agree against $C$, and a number of places where $BC$ agree against $A$, but none at all where $AC$ agree against $B$.

and in its converse ($\beta$):

If, on comparing three mss $ABC$ over a certain stretch of text, we find that $AB$ agree several times against $C$, and $BC$ agree several times against $A$, but $AC$ never agree against $B$, then $B$ is intermediary between $A$ and $C$.

Thus if $AB>C$ denotes the number of times that $AB$ agree against $C$, then the result:

$$AC>B=0,$$

while $BC>A$ and $AB>C$ are reasonably large¹ - is both necessary and sufficient, according to $\alpha$ and $\beta$, for $B$ to be intermediary between $A$ and $C$.

Hence we must compare our mss by threes, and whenever we obtain such a result - which Quentin terms a "zero" - we shall have identified one of our mss as an intermediary between two of the others. Every such discovery will bring us nearer to the reconstruction of the whole family-tree.

¹ It is important that both $BC>A$ and $AB>C$ be rather greater than zero, because to observe two zeros among the three totals $AB>C$, $AC>B$, $BC>A$, indicates not an intermediary relation but the virtual identity of two of the mss compared (Mémoire, p. 222). Thus if we found both $BC>A$ and $AC>B$ to be zero, this would mean that $A$ and $B$ did not differ at all over the collection of variants employed for that comparison.
We observe that case (vi) in fig. B.11.1 is special, in that it explicitly refers to a ms which derives from two exemplars. This holds out the prospect that, if we use Quentin's method, the presence of contamination will be properly traced and will not impede our attempt to recover the history of the text. This prospect is far more inviting than Mass' confession of utter helplessness in the face of contamination ("Gegen die Kontamination ist noch kein Kraut gewachsen" - p. 31), and doubtless accounts for the persistence of interest in Quentin's work. This then is an area wherein we shall have to consider with particular care the effectiveness of the method.

Before examining the logical validity of $\alpha$ and $\beta$, let us pause to appreciate the original contribution which Quentin made by his proposal to search for intermediaries. First, there is the proposal itself, the usefulness of which we shall examine shortly. Second,

1. See Zarrà, p. 176 "...quello di Don Quentin è l'unico metodo meccanico che preveda di tener conto in modo rigoroso, durante la costruzione stessa dello stemma, anche di situazioni contaminate".

2. It has been suggested that the principle of the zero, although it had never before been employed in textual criticism, was not altogether novel. Marichal states (p. 1287) "il y a déjà plus d'un demi-siècle que les historiens l'appliquaient à la recherche des sources et à la critique des témoignages", he does not however give references. Again, Proser (p. 472) regards the principle as a development of certain guidelines set down by E. Bernheim, "Lehrbuch des historischen Methode", Leipzig 1908 (5th ed., the first edition, which I have not seen, appeared in 1889). But I doubt whether Quentin's priority can properly be denied. To be sure, Bernheim does have a section (pp. 429-437) on the possible relationships between three sources, where he shows how one may recognize relations of each of the following types:

\[
\begin{array}{c}
A \\
B \\
C
\end{array}
\quad
\begin{array}{c}
A \\
B \\
C
\end{array}
\quad
\begin{array}{c}
A \\
B \\
C
\end{array}
\]

(p. 430)  (p. 430)  (p. 434)

These diagrams bear an obvious resemblance to fig. B.11.1, but they are far from proving that Quentin was either influenced or anticipated by this work of Bernheim. There are a number of essential differences, which deserve to be noted.

For Quentin, the concept of intermediacy was of fundamental importance. This relationship was topological, not historical, indeed it covered a range of situations that were historically quite different. His first task was to identify whatever intermediaries he could, questions of orientation were left to a later stage. In Bernheim's thought, however, intermediacy enjoyed no great prominence (although he would have said, for example, that in the second of the diagrams above, B was intermediate between A and C), and was never conceived as a unifying principle underlying several different patterns of descent. We may go so far as to say that he speaks throughout in terms of historical relationships alone. Thus, his first aim is not a search for zeros and intermediaries, instead, he envisages a number of different possibilities of historical relationship between his three sources and tries to decide which one is appropriate, in a single operation. Finally, Bernheim does not discuss the relationships shown in fig. B.11.1 (iv)(v). Bernheim himself drew, for his procedures, on the work of earlier scholars, in particular Julius Ficker (1826-1902), whom Marichal presumably had in mind, but though I cannot claim to have studied the matter in detail, I have yet to be convinced that Quentin's originality can be reasonably challenged.
we see here the introduction of a topological relationship (namely intermediacy), which requires us to view the family-tree as a geometrical figure (or, to be more accurate, a graph), whereas earlier writers had regarded the tree merely as a handy device for representing the descent of the mss and had spoken only of historical relationships (such as ancestry, derivation, and so on). Third, there is the realisation that in order to discover topological relationships, one does not necessarily have to make up one's mind on the rightness or wrongness of readings; the data can be used in its "objective" form. It is noteworthy that although the first contribution was greeted with mixed feelings, the second and third have exerted great influence on today's investigators as can be seen from the work of Froger.

The logical basis of \( \alpha \) and \( \beta \) has never, to my knowledge, been treated rigorously. Let us now do so, in the same way as we treated the stemmatic method in Ch. 1. Thus we shall try to identify, for either proposition, the presuppositions which would have to be made in order for the truth of that proposition to be a logical necessity. We shall find it convenient to distinguish our treatment of case (vi) from that of the other five, and to retain the notation of Ch. 1 for the six assumptions there identified.

It is easy to deduce \( \alpha \) for cases (i) to (v) - let us write \( \dot{\alpha} \) to remind ourselves of this restriction - by considering in turn each of those five possibilities from fig. B.11.1. We shall have to appeal to assumptions IV
("no coincidence in error") and V ("no successful correction")\(^1\), because otherwise AC might come to agree against B\(^2\); VIab ("all copyists err" and "some errors peculiar to each copyist") are also necessary, because otherwise AC>B might not give the only zero.

Proposition \(\alpha\) in case (vi) - which we may denote \(\bar{\alpha}\) - will have to rest on a rather different basis. It will be true provided that B always follows either A or C. This will probably not happen if there is any arc ms lying along the arc AB or CB; for let X be such a ms (fig. B.11.2b). Then there will probably be many places

\[
\begin{array}{c}
\begin{array}{c}
A \\
\Downarrow
\end{array} & \\
\begin{array}{c}
C \\
\Downarrow
\end{array} & \\
\begin{array}{c}
X \\
\Downarrow
\end{array} & \\
\begin{array}{c}
B \\
(\alpha)
\end{array}
\end{array}
\quad
\begin{array}{c}
\begin{array}{c}
A \\
\Downarrow
\end{array} & \\
\begin{array}{c}
C \\
\Downarrow
\end{array} & \\
\begin{array}{c}
X \\
\Downarrow
\end{array} & \\
\begin{array}{c}
B \\
(\beta)
\end{array}
\end{array}
\quad
\begin{array}{c}
\begin{array}{c}
A \\
\Downarrow
\end{array} & \\
\begin{array}{c}
C \\
\Downarrow
\end{array} & \\
\begin{array}{c}
X \\
\Downarrow
\end{array} & \\
\begin{array}{c}
B \\
(\gamma)
\end{array}
\end{array}
\quad
\begin{array}{c}
\begin{array}{c}
A \\
\Downarrow
\end{array} & \\
\begin{array}{c}
C \\
\Downarrow
\end{array} & \\
\begin{array}{c}
X \\
\Downarrow
\end{array} & \\
\begin{array}{c}
B \\
(\delta)
\end{array}
\end{array}
\end{array}
\]

where AC agree in having the correct reading and X has committed an error in copying A; in some of these places

1. These two together effectively include III.

2. Quentin was aware that contamination was capable of preventing the appearance of the zero (Mémoire, pp. 220 ff), but Sievers (pp. 83 ff) rightly takes him to task over the general rules he lays down on the effects of contamination. Sievers substitutes a single master rule: "In any three related manuscripts, whenever the reading in one extreme is through contamination altered to the reading in the other extreme, the intermediary will not yield zero." This appears sound, except that we should rather say: "through contamination, conjecture, or coincidence in error".
X will perhaps be followed by the conflate ms B, and thus AC>B will not yield zero. Again, if B is not the conflate ms itself but a descendant thereof (fig. B.11.2c), then there will be places where ACX agree in a correct reading which B has copied wrongly, so that there too AC>B will exceed zero. Thus Δ can be proved only when B is derived directly from A and C. Moreover, we must assume that no third source was utilised (fig. B.11.2d), for in that case B might sometimes follow that third source against the agreement of A and C. Finally, the zero will fail to appear if B deviates for any reason from both his sources (e.g. in committing an unintentional error in a place where A and C are sound). We conclude that we cannot expect to find a zero in case (v1) unless the conflate ms itself, and its two sole immediate sources, have survived and are the three mss under comparison. In such a case, a zero might indeed be observed; but as it is almost universally true that far more mss have perished than have survived, such a possibility is remote. We cannot then hope to unravel more than a tiny proportion of the instances of contamination on this basis.

We now come to β. One way of proving β (the dot has the same function as before) is to treat it as a particular application of the method devised by Froger (Thes., p. 1:36) for deriving a network from the two-way

1. despite one of Zarrl's diagrams (p. 179, labelled 1/4 + 2/4).
splits evinced by the mss. Here we have, by hypothesis, several instances of AB:C and BC:A, but none at all of AC:B. Choosing A as our "base", we apply Froger's method in this elementary instance:

```
  A  B  C
  |   |   |
  B  C
  |   |
  O
```

the ms sets the network

Fig. B.11.3

As there are five possible ways of orientating this network, $\beta$ is proved. However it will partake of the dependence of Froger's method in general on assumptions I-VIab.

But what presuppositions would we have to make in order to deduce $\beta'$, as opposed to $\beta$? We note that $\beta'$ actually is stricter than and includes $\beta$, because the former states that the zero implies any one of five possible situations, whereas the latter admits all those five possibilities and adds a sixth. Therefore, if we accept assumptions I-VIab and thereby prove $\beta$, we shall in a sense have proved $\beta$ ipso facto - but in such a way as to make the mention of case (vi) utterly superfluous. We must therefore ask: Can we set up a less stringent set of assumptions from which it would follow that a zero implies that each of the six situations is a real possibility
and that there are no other possibilities? I do not think it possible to devise such a set of assumptions, from which \( \beta \) could be formally deduced. On the other hand, we shall see below that it is hard (though not impossible) to conceive of any situation, other than the six shown in fig. B.11.1, giving rise to a zero. Thus although a formal proof of \( \beta \) cannot be offered, we can nevertheless state, when we have studied a reasonably extensive stretch of text and find ourselves in the presence of a zero, that

(a) this zero could have been brought about by any one of the first five situations;

(b) the sixth offers an additional possibility consistent with the evidence;

(c) still further possibilities could theoretically be envisaged, but they are very unlikely (see pp. 11:14f below). In sum, it is straightforward to prove \( \alpha \) and \( \beta \), by appealing to assumptions I-VIab; but the logic ceases to be clear-cut when case (vi) is brought into the argument.

Thus we have shewn to what extent \( \alpha \) and \( \beta \) can be justified logically; but our analysis so far has remained, for the most part, within the framework of assumptions I-VIab, which postulate the same conditions of textual transmission which we had to postulate in Ch. 1 in order to justify rigorously the procedures of Froger. Now as we have pointed out repeatedly, this set of assumptions is not always justified in practice, and we must therefore ask how \( \alpha \) and \( \beta \) will be affected if we relax those assumptions, whether in order to come nearer to real life or for economy of effort. The dangers whereby the application of Quentin's method...
The assumptions which clamour to be relaxed in the interests of realism are III ("no subsidiary sources"), IV ("no coincidence in error") and V ("no successful correction"). The invalidity of any of these could easily bring about danger (a). Let the reader turn again to fig. B.11.1. In case (i), AC > B will no longer give zero if C succeeded in correcting away some of the errors which arose in the writing of B, and thus in recovering the sound reading of A. Case (ii) is similar. As for cases (iii)–(v), a coincidence in error between A and C would cause them to agree against B, and then no zero would be observed. Thus danger (a) is very real. What, however, of (b)? Consider, for example, three mss thus related:

```
    ω
   /|
  /  |
A   B
  \/
   C
```

1. It might be urged that as coincidence in error etc. occur in only a minority of passages, we shall still find that AC > B is a far smaller number than BC > A and AB > C, albeit not actually zero. Quentin himself proposed that we treat a low figure of this sort (which he termed a quasi-zero) like a true zero; he is supported here by Marichal (p. 1289). We shall discuss the quasi-zero below (pp. 11:25ff). Note that quasi-zeros are of no interest to Zarri (pp. 174 ff), who distinguishes only two cases: either we have a zero ("non si è mai verificato l'accordo contro [i.e. the agreement of two of the mss compared against the third]" or we do not ("si è verificato almeno una volta l'accordo contro").
We would normally expect B to have inherited a number of errors originated by \( \beta \), and hence AC ought to agree against B in several places. Now it is theoretically conceivable that, in all those places which lie within the sample of text on which all our results are based, B has succeeded, by conjecture or by the use of subsidiary sources, in recovering the reading of \( \omega \); in that case, AC>B will yield zero.\(^1\) But the likelihood of such thoroughgoing and successful correction – provided that our text sample is adequately long – can be safely neglected.

We conclude that, in a tradition for which III-V are not valid, we cannot be sure of observing a zero even when an intermediacy relationship exists. If, however, we are fortunate enough to observe one, then we may be virtually certain (provided that assumption VI holds good\(^2\)) that it does indicate intermediacy.

Another assumption which we may wish to relax is VIb, namely that "our construction domain is large enough for us to be confident that it contains, for each copyist, at least one passage where he committed an error..." Obviously the shorter our sample, the lighter our task;

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1. It might be argued that in such a case B would have become identical with \( \omega \), so that there would be no harm in treating B as if it actually were \( \omega \). But B might not have maintained his success outside the sample chosen for analysis.

2. On the consequences of relaxing VI, see below.
we should be relieved if it could be shewn that it was 
not essential for our sample to be so long that few 
scribes could have copied it without committing an error. 
Suppose then that we select a sample too short to justify 
VI. We shall not suffer on grounds of (a); but (b) will 
constitute a serious danger. In order to understand this, 
let the reader imagine a great score-board on which all the 
totals needed for all the comparisons by threes are built 
up. This score-board will have a large number of rows, 
one for each possible ms triad that can be formed from our 
collection of mss; in each row there will be shown three 
numbers, viz the three totals which have to be calculated 
in the comparison of each triad (after the types AB>C, 
AC>B, BC>A). Initially, all the entries on the score-
board are zero. We now run through the text. At each 
passage where the mss diverge, we shall ascertain, for each 
triad\(^1\), whether two of its members agree against the third; 
if so, we shall add 1 in the appropriate place. At first, 
we repeat, all the entries on the board will show zero; 
but as we proceed, one space after another will cease to do 
so. Thus every entry begins by being zero, and so no zero 
can be taken seriously until it has proved itself, so to 
speak, by remaining intact over a reasonably extensive 
stretch of text. Suppose for example three mss ABC which

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1. All this would of course be exceedingly tedious for a 
   human operator, but a computer could readily tackle 
   the job in this way.
are copied independently from the same exemplar. If we confine our examination to a very short stretch of text, it may happen that whereas A and B have committed errors within that domain, C has not - even though C may have made a great many mistakes over the rest of the text. Then we would obtain a zero for AB>C, but we would be wrong to conclude that C was intermediary between A and B. This, then, is the danger of working from a text sample which falls short of the requirements of VI. An investigator who accepted as significant a zero that was based on an inadequate sample, would be like a newly arrived schoolteacher who was convinced that those children who gave him no trouble during his first lesson would remain well-behaved for the rest of the year.

This completes our examination of the logical basis of the comparison by threes. We conclude that a zero which is observed over an adequate sample is meaningful, and is all but certain to have resulted from an intermediacy relation. But two new questions immediately arise. Is it practical to search all the possible triads in order to identify all the zeros there may be? And if we succeed, what guarantee is there that enough information will have been amassed in order for the entire stemma (or rather the network) to be recovered?

With regard to the former question, the labour involved in conducting all the possible comparisons by threes is enormous. The number of triads that can be formed from m mss is \( m(m-1)(m-2)/6 \) - which, for \( m=100 \),
comes to more than 160000 - although of course the preliminary table of agreements will point to certain triads as being especially worth studying\(^1\). And to derive the figures for even one triad takes a long time, proportionate to the number of variant passages considered. I would go so far as to say that it was the sheer magnitude of labour that really dissuaded most textual critics from adopting Quentin's system. The case is altogether changed, however, with the advent of the computer, to which all the calculations could be delegated; today one cannot reasonably object that Quentin's procedures are not practical.

But the second point presents a far more serious objection. We have already seen that if assumptions III-V are unjustified, then we cannot be certain of detecting, by the observation of a zero, all the intermediacy relations that may exist among our extant mss. But more serious is the fact that even if we \textit{could} identify all the intermediacy relations, we would still know nothing like enough\(^2\) to be able to reconstruct the whole network. Consider once more the different possibilities shown in fig. B.11.1. It will be seen that all of them have one thing in common: at least

\footnotesize

1. If we succeed in dividing the mss into families, then the relationships within each family can be determined if we consider all the possible triads that can be formed from its own members; the relationships between the families can be studied if we select one representative from each family and compare by threes those representatives.

2. except in certain trivial cases.
one of the three mss is a direct descendant of one of the others. Thus there can be no question of an intermediacy relationship unless one member of the triad is a descendant of another extant ms. It follows that if our set of mss constitutes a terminal group\(^1\), i.e. a group of mss none of which derives from another extant ms, we shall find no zeros at all. This is particularly important in view of the generally accepted doctrine of eliminatio codicum descriptorum\(^2\), i.e. that an essential preliminary to recension is the elimination of any ms that can be shewn to derive from another extant ms (Maas, p.1, 84). If we eliminate such mss, as most would agree that we ought, then we shall have reduced our collection of mss to a terminal group – in which no zeros can be observed. And even if we do not eliminate them, there will still be regions of the stemma which Quentin's system will leave uncharted. Consider for example five mss ABCDE whose inter-relations are unknown to the investigator but are in fact as shown in fig. B.11.4a. Then we may well find zeros for AE>D, BE>D, CE>D; but that

1. after the terminology of Greg (p. 5).
2. See Thes., p. A:34.
is not enough to determine whether the true network is (a) or some other possibility such as (b). In other words, Quentin's system has shed no light on the terminal group ABC. Thus our a priori expectation is that whatever zeros we observe will leave us far short of what we require.

Yet one would have had a very different impression to judge from the work of Quentin himself; for he provided a classification for the mss of the Vulgate Octateuch and of several other texts. How can we explain this discrepancy between our expectations and the achievements Quentin claimed?

It seems that his success was due to three devices, all of doubtful legitimacy:

(1) The domain on which the figures were based was kept small; it never contained as many as 100 "variantes caractéristiques" and sometimes less than 30.

(ii) All instances of a reading peculiar to a single ms were excluded from consideration.

(iii) When three mss ABC were compared, and the three totals AB>C, AC>B, BC>A were obtained: if none of those

1. Remarkably enough, Zarr1 complains of precisely the opposite difficulty, namely that he found so many zeros that he was faced with a veritable "alluvione" of alternative stemmata (pp. 177 ff). The explanation is that Zarr1 had worked with very small domains, containing only thirty variant passages or even less (cf (i) below).

2. See p. 11: 5 , n. 2.

3. Cf Essais, p. 65: "S uivant la longueur des textes, 20, 25, 50, 80 variantes caractéristiques doivent largement suffire pour faire un classement".
three figures was zero, but one of them was far lower than the other two, Quentin termed this lowest figure a "quasi-zero", and drew virtually the same deductions as if a true zero had been found.

We must now consider whether each of these three procedures can be justified.

We have already noted the danger of working from a text sample which is not lengthy enough for VIb to be fulfilled. Now the domains on which Quentin based his results do not in fact appear to be so long that we can be reasonably sure that virtually any copyist would have committed an error in the course of his transcription. Thus Quentin worked on a passage of under 250 words in order to classify the mss of the Passio SS. Mariani et Jacobi¹, and on a sample of only 60 lines from the Lai de l'Ombre². Nor is it by any means inconceivable, when we think of the care bestowed on the sacred text of the Bible³, that a scribe could have succeeded in copying the whole of Quentin's eight-chapter sample from the Octateuch⁴ without

2. Essais, pp. 147-164.
3. As Quentin himself states: (Mémoire, p. 210): "Tout différent est le cas des manuscrits bibliques. Le respect de la parole divine s'y montre assurément au soin avec on y transcrit le moindre iota ...".
making a single mistake. And indeed there is evidence that if Quentin had worked with domains large enough to make assumption VIb at least highly probable, he would not have obtained any zeros (and perhaps even quasi-zeros) at all. For example, he took up the challenge implicit in Bédier's preface to his edition (1913) of the Lai de l'Ombre, and applied his own method in order to discover the relations between Bédier's seven mss ABCDEFG. From a total of 962 lines, he selected a domain of only 60, which yielded 30 variants. There he observed a number of zeros, and in particular he found F to be an intermediary between each of the pairs CD, CE, GD, and GE. However, Bédier points out (p. 328) that elsewhere in the poem we find 9 occurrences of the grouping ABF:CDEG, none of which happens to fall within Quentin's sample. Had Quentin based his figures on the whole poem (or on a different sample in which the split ABF:CDEG was represented), he would not have obtained any of those four zeros, and would have arrived at correspondingly different conclusions. Again, his sample of eight chapters (one from each book) of the Vulgate Octateuch yielded 91 variants, and his analysis picked out a fair number of zeros.

1. This text-tradition will already be familiar to the reader from our earlier discussion (pp. 2:26 ff).

2. When the nature of the readings themselves was considered and the orientation determined, Quentin's conclusion was that F is derived not only from D (as Bédier and Gaston Paris had agreed) but also from a member of the family represented by the two closely related mss CG (as had not been envisaged previously).
Dom Chapman, however, who preferred to base himself on a far wider field of variants, had to state (p. 5n.): "On my larger statistics, his [Quentin's] method will not work, for his comparison by threes will no longer produce zeros". Thus many of the zeros which Quentin reported seem to be no more than side-effects of inadequate sampling, and disappear when the whole of the text is taken into account.

We now come to the question of isolated readings. Although Quentin excluded them from comparisons by threes, he proposed to take account of them in the following way (Mémoire, p. 230). Suppose that, having compared the three mss ABC over a domain which includes instances of unique readings, we find that none of the three totals BC>A etc. yields a zero, but that one of them (let us say AC>B) can be made to do so if we disregard unique readings. That is, all that prevents one of our three mss (in this case B) from being the intermediary is the existence of its unique readings. In such a situation, Quentin would deduce that it was not B itself that stood as intermediary between A and C, but a lost exclusive ancestor of B (which we may denote B°). An example of Quentin's application of this is his treatment of the Theodulfian group of Vulgate mss (Mémoire, p. 257). Having decided — after the removal of unique readings — that Bern is intermediary between Hub and Anic, and Anic between Bern and Theo, he draws the following

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1. about 1150 select readings in Genesis and 876 in Exodus.
stemma:

The rationale behind this is, apparently, that the unique readings of B (to revert to our earlier notation) are errors which have come in during the latest stages of its descent, and that we need only remove them in order to obtain the text of an ancestor of B. But the doctrine that all unique readings are by definition errors is dangerous (see p. 3:13), and the whole procedure is therefore suspect, as the following counter-example will shew.

Consider five mss ABCDE, related as in fig. B.11.6(1), and suppose that a fair number of scribal errors have been committed by the various copyists and that no coincidence in error or successful conjecture has occurred. What shall we find if we perform a comparison by threes on the triad ABD? Since γ has committed many errors that have come down to
BC - and cannot therefore be neglected as readings peculiar to a single ms - we shall observe several instances of AD agreeing against B. Similarly the sins of $\xi$ will ensure that $ABD$ is much greater than zero. But BD will never agree against A, except in places where A's reading is isolated$^1$. If such places are to be ignored, then A (or rather $A^0$) will appear to be intermediary between the two mss BD, and - by parity of reasoning - between each of the pairs BE, CD, CE. This will yield the network of fig. B.11.6(11), which, no matter how we orientate it$^2$, will not show the true relationships.

Finally let us consider Quentin's appeal to the quasi-zero. Can we set up two propositions $\alpha'$ and $\beta'$ relating

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1. either because it has a peculiar error, or because it preserves the true reading in a place where $\beta$ - followed by its descendants BCDE - has lost it.

2. The right place to "pick up" the network is, of course, along the dotted line $A^0A$; but as $A^0$ is by definition an ancestor of A, that would be impossible.
to the quasi-zero and corresponding to the propositions \( \alpha \) and \( \beta \) on which our treatment of the zero was based? Certainly \( \alpha' \) is justifiable. If we adopt a relaxed (and not unrealistic) version of III-V, and regard contamination, coincidence in error and successful conjecture as somewhat untypical — rather than being utterly ruled out, as before — we may expect an intermediary to yield a quasi-zero on a comparison by threes. (The reader can convince himself on this point by referring to fig. B.11.1.) But if we admit \( \alpha' \), it does not necessarily follow that we can simply convert it without further ado, to obtain \( \beta' \), which would imply that intermediacy can be deduced from a quasi-zero. The fact is that although intermediacy is likely to bring about a quasi-zero, other quite different situations can have the same effect. This may be illustrated from Dom Quentin's own model tradition (Thes., pp. 5:2 ff). Let us perform a comparison by threes on the triad EKL. From the apparatus criticus (p. 5:4), we obtain:

\[
\begin{align*}
\text{EK} & > \text{L} = 17 \quad \text{(see lines 2,3,5,5,6,7,7,9,10,10,11,11,12,13,13,14)} \\
\text{EL} & > \text{K} = 13 \quad \text{(" " 2,2,3,3,3,5,6,9,13,14,15,15,15)} \\
\text{LK} & > \text{E} = 2 \quad \text{(" " 3,6)}
\end{align*}
\]

with a quasi-zero against E. But a glance at the true textual history (p. 5:3) will suffice to show that E is not intermediary between K and L in any sense whatsoever.

Hence the quasi-zero does not necessarily indicate intermediacy. To be sure, a quasi-zero can be well accounted for if we postulate an intermediacy relation; but that relation is no more than a plausible hypothesis,
and certainly not an inescapable logical deduction. To build up a whole stemma from such hypothetical intermediacies, as Quentin did, is to heap one supposition upon another, and to set up a structure that is utterly precarious. In the mordant words of E.K. Rand (p. 246): "From quasi-zeroes, I fear, only quasi-stemmata can be derived."

By means of these three devices, then, Quentin succeeded in fitting his mss into a network. This network then required orientation, which Quentin accomplished by considering the claims to originality of the different readings, in essentially the same way as Froger proposed (see pp. 1:36 f). But as certain aspects of the derivation of the networks themselves had been questionable, the final stemmata came in for a great deal of criticism.

First of all, it was inevitable, in view of what we have said about terminal groups (p. 11:19), that, in the stemmata 1 which Quentin obtained, most of the mss appear to be descended from extant ancestors. For example, he draws (Mémoire, p. 310) a genealogy of nine mss of the

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1. Quentin himself used the term "schéma" rather than "stemma" for his historical trees, but that does not mean that he did not regard them as valid stemmata. (The reason he gives is that "stemma" is a Latin word which "n'a pas passé dans la langue française": -- Essais, p. 80.)
Spanish group, in the form of a tree containing only two lost point mss. Such results are, of course, most unusual; it is to the point that our stochastic model also suggests (pp. A:34f) that they are highly unlikely. On this ground alone, they were treated with suspicion. As Dom Chapman put it (p. 389n.): "It is as if I explained my relationship to all my cousins by putting their names in a genealogy as my grandfathers and great-grandfathers".

Even more alarming is the fact that in many of Quentin's stemmata, we find instances of a ms being apparently descended from a later ms. Thus on p. 394 of his Mémoire he draws a tree showing Lugd (cent. viii) as a descendant of Leg (dated 960). In order to defend this, Quentin was forced to declare (Essais, p. 103): "La vérité, c'est que mes généalogies s'appliquent non aux manuscrits eux-mêmes, mais aux types de transmission du texte qu'ils représentent." Thus Dom Quentin claims that the stemmata refer only to text-types. This raises a host of new problems, as Chapman (p. 390n.) points out. We are asked to believe that every extant ms is our sole representative of a whole family of mss all attesting the same type of text. This is, of course, by no means satisfactory; in particular, Dom Quentin offers no definition of the term "type", which seems a dangerously nebulous concept. But if we grant for argument's sake its validity, we must now ask how we can deduce the readings of the text-type from those of the corresponding extant ms. Apparently, the text-type is supposed to be
identical with the extant ms except that in places where the latter has a unique reading, the text-type follows the majority of the mss¹. This procedure fits in well with Quentin's contention that instances of unique readings be left out, but it is open to a serious objection, viz. that it dismisses unique readings as aberrations of the individual ms, whereas one might have thought that they would often have presented valuable evidence of the characteristic readings of its particular "type". Thus I feel that it must be admitted that we cannot attain anything like certainty in deducing the text of the "types" from that of the mss. In that case, the stemmata drawn by Quentin - which now claim to deal not with known mss but with uncertain "types" - can hardly provide an "iron rule" for choosing between rival readings.

¹. I have not found this stated explicitly, but it can be deduced from certain statements in the Essais, e.g. "Ce que je compare, en effet, ce sont uniquement des formes diverses du texte, indépendamment de toutes les particularités propres aux copies de ce texte" (p. 103); see also his remarks on the unique readings of P, on p. 138.

It is interesting to note how Quentin shifted his position between the publication of the Mémoire in 1922, and that of this defence (which first appeared in Rev. Bén. for May 1924). In the former, he had justified the removal of unique readings on completely different grounds, and sought to take account of them by postulating a lost ms which was the real intermediary (see p. 11:23); there his arguments had been formulated unequivocally in terms of manuscripts, and indeed would be rendered incoherent if one were to understand them to refer instead to types.
In addition to the foregoing criticism of the stemmata themselves, there has been considerable dissatisfaction with the readings which they would lead us to accept. Thus Chapman (op. cit.) considered the canon set up by Quentin for the mss of the Vulgate Octateuch, viz that the agreement of at least two of the three mss Am, Tur and Ott gave the reading of the archetype, and found a great many passages in Genesis, Exodus and Leviticus where that canon would lead to the adoption of a reading which is in itself quite unacceptable and

1. Two important appraisals of Quentin's Mémoire, which criticise a number of his results, deserve to be noted here, viz:


Neither had access, however, to as much data as Chapman, whose arguments we proceed to discuss.
far inferior to one attested by other mss. Chapman puts his case in detail and, to my mind, most convincingly. Again, in his treatment of the Lai de l'Ombre, Quentin set up a rule that when ABCG agree against DEF, we must adopt the reading of the former; but Bédier discusses in detail (pp. 342-350) five passages in which DEF offer what he considers to be by far the better reading. It seemed to Bédier hardly conceivable that those readings of DEF, which by their

1. In many of these cases, Quentin too felt that he could not print in the text of the edition itself an unsatisfactory reading which his rule directed him to accept. Occasionally, that reading is simply rejected; thus in Ex. 28:37 he relegates, without comment, "ea" (Tur, Ott) to the apparatus, and adopts "eam" (most other mss). Far more often, however, he insists that the rule has given the reading of the archetype, which is however corrupt. He then adopts for his text the more satisfactory reading - which in most of these cases has strong ms. support even though it is not prescribed by the rule - but places an obelus in the text to mark a passage corrupt in the archetype, and notes, immediately below, the reading to which his rule first led him, with the formula "errante archetype". (For instances of this procedure, see Gen. 8:19, 9:3, 40:19, 44:23, 34; Ex. 7:3; Lev. 19:26; Num 17:8. It is noteworthy that in Deuteronomy, which was completed after Quentin's death, the formula is softened to "errante ut vid. archetypo" or "cvm archetypo".) In such cases we are asked to believe that those mss which attest the reading which he accepts as original, owe it not to fidelity of transmission alone - for they are all derived from an archetype in which it had become corrupted - but to a happy conjecture or correction. In the terms of Kantorowicz, Quentin insists that that reading, albeit "richtig", cannot be "echt". I am by no means alone in finding that hypothesis, in a great number of passages, hardly plausible.
intrinsic superiority clamour to be acknowledged as original, be relegated to the "oubliettes" of the apparatus in favour of the poorer readings of ABCG. Here of course I can only note the conviction of Bédier himself, and could not venture an opinion of my own.

Our verdict on Quentin's method as a practical tool in textual research cannot then be favourable. The investigator is likely to find the search for zeros well-nigh fruitless, and if he then comes to pin his hopes on what he regards as quasi-zeros, he may well be deluding himself. This agrees with the view of other writers who, in all good faith, sought to apply these methods to their own problems and were disappointed. Thus E. C. Colwell, who applied it to the mss of Fam. 13 in Mark¹, declared: "The amount of labor involved is enormous; the results are meaningless". What is particularly significant is that the Benedictine project itself, for which Quentin first devised his method,

discontinued the application thereof after his death. Nevertheless, the failure of the method of comparison by threes should not blind us to the fact that many of Quentin's results which did not depend primarily on those comparisons have stood the test of time and are highly valued by the experts of today. In the words of Dom Bonifatius Fischer: "Quentin war als Textkritiker und Handschriftenkenner weit besser als seine Methode.

1. Genesis-Numbers were edited by Quentin in accordance with the principles set down in the Mémoire; the edition of Deuteronomy, which he had supervised for some time until his death in 1935, adheres in the main to the same system. The remainder of the Octateuch was published in 1939; although the editors state (p. ix): "...textum ad normas illas recognoscere studuimus quas pro toto Octateucho stabiliverat b. mem. D. Henricus Quentin", the fact remains that the "iron rule" could not be applied to those books because they were not extant either in Tur or in Ott, and the editors did not devise another "iron rule" to take its place. The break with Quentin's procedures is acknowledged in the next volume to be published (Samuel, 1944, p. viii): "Iamvero istorum voluminum praeparatio, ob haud facilem codicum adhibendorum selectionem et eorum in classes distributionem, nos in novas induxit inquisitiones, cum quae a D. Henrico Quentin peractae sint Octateuchum non excedant".

2. The most important of these results are listed by Dom B. Fischer, "Bibelausgaben des frühen Mittelalters" [= Settimane di studio del Centro italiano di studi sull' alto medioevo, 10], Spoleto 1963, p. 520n.

3. loc. cit. (see previous note).
METHOD OF V.A. DEARING

References


This handbook was examined by B.M. Metzger, "The Text of the New Testament" (Oxford 1964), pp. 167-9. Prof. Metzger's view of Dearing's procedures and conclusions could not be described as one of whole-hearted approval. Dearing replied to some of Prof. Metzger's criticisms, and took the opportunity of modifying his own views in some respects, in "Some notes on genealogical methods in textual criticism", Novum Testamentum (1967), pp. 276-297.

See also the description of a project entitled: "A Study of the Text-History of Cassiodorus' "Psalm Commentary": Analysis of Variants for a Critical Recension of the Text", the principal investigator being Prof. J.W. Halporn, in Computers and the Humanities i (1966-7), p. 208. The project is to include "development of programs to indicate MS. relationships based on methods outlined by Vinton Dearing." At the time when the description was written, the study was in an experimental stage. Articles were envisaged for the journal Wiener (surely Wiener?) Studien, but I have seen nothing more as yet (Nov. 1972).

This book of little more than a hundred pages covers a remarkably wide range of topics. The first chapter introduces the main concepts of Dearing's approach, which, he acknowledges, owes a great deal to the labours of Greg and Dom Quentin. He employs the term "state" (sc. of text) to denote the form(s) in which the text under analysis appears in a given document. Another concept to which he attaches particular importance has already been mentioned (Thes., p. 2:34), viz the distinction between bibliographical and textual analysis. The former has to do with the genetic relationships between manuscripts as such, while the latter concerns the relationships between the states of text to be found
therein; this book, as its title implies, confines itself by and large to the latter. Chapter Two sets out detailed methods of obtaining what he terms a textual diagram, from collations of mss etc.; these methods will be discussed below. The chapter also contains a convenient and amply illustrated list of the different varieties of scribal error (pp. 10-17), and a discussion on the means whereby one dates manuscripts (pp. 44 ff.) and printed books (pp. 47 f.).

The third chapter is entitled: "Mechanics of Calculation", and deals with a host of practical problems, including the possibility of facilitating the analysis by means of computers. Finally there is a chapter of examples; three of these concern works of English literature, and they are followed by an analysis of the Greek text of Philemon and - perhaps a trifle unexpectedly - by a proposed solution to the Synoptic problem.

The first step in carrying out this method is to list the different splits evinced by the mss in each variant passage throughout the text under analysis. When the splits are all mutually consistent, Dearing's procedures will usually lead, as far as I can judge, to a network which is essentially the same as that to be obtained by applying

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1. On the state of this question today, see Thes., pp. 10:38-45 and references quoted therein, especially Dom Fischer's article in J.T.S. (1970) pp. 297-308. An eminent authority in this field is Dr. W. Ott of the Zentrum für Datenverarbeitung in the University of Tübingen.
the methods of Greg or Froger. Dearing is well aware that before that network can be correctly orientated, questions of chronology and of the rightness and wrongness of readings must be considered. It is, however, when we meet an abundance of inconsistent splits which rule out the application of those other methods, that Dearing's analysis comes into its own; for it is claimed to be able to take such complications in its stride in the following way.

Suppose we have, for example\(^1\), five mss ABCDE, which exhibit the three groupings ABC:DE, ACD:BE, ADE:BC as well as a full set of "unique" groupings (i.e. \(\Sigma:A\), \(\Sigma:B\), \(\Sigma:C\), etc.). The second split conflicts with both the first and the third; as they stand, this collection of variants will not yield a network. However, suppose that we temporarily disregard B. Then, as the reader may verify, our collection of groupings will reduce to:

\[
\begin{align*}
AC:DE & \quad A:CDE & \quad C:ADE & \quad D:ACE & \quad E:ACD
\end{align*}
\]

which are all mutually consistent and yield the network of fig. B.11.7(a). That network is termed a partial diagram. Similarly, if we reinstate B but exclude E, we obtain the collection of groupings:

\[
\begin{align*}
AD:BC & \quad A:BCD & \quad B:ACD & \quad C:ABD & \quad D:ABC
\end{align*}
\]

---

1. In fact in his 1967 article, Dearing is anxious to make it clear that his own theories are based on those of Greg - so anxious indeed that he attributes to Greg statements which are not to be found in the latter's work and are apparently Dearing's own. Thus on p. 283: "Greg's third rule is that the common intermediary in a group of manuscripts will be hypothetical only if all the manuscripts in the group are terminal".

2. This example was made up by Dearing (p. 35).
which yields another partial diagram, as in fig. B.11.7(b).

We now have a set of partial diagrams which, between them, cover all the mss. Dearing then proceeds to combine (or to use his term, overlay) them, and arrives at fig. B.11.7(c).

![Diagram](image)

Fig. B.11.7

Note: The orientation of these networks is arbitrary. As Dearing does not intend them to be interpreted as bibliographical diagrams, he has deliberately chosen orientations whereby each diagram will not look like a stemma (in that there is more than one ms at the top).

This combined figure he regards as the appropriate textual diagram. We are not explicitly told the exact purpose of this diagram, but it is evidently intended to represent, in some way, the relationships between the five mss. Other instances of inconsistent groupings may be treated similarly. We subtract some of the mss until consistency is attained, and thus derive a partial diagram. By making a series of different subtractions, we obtain a set of partial diagrams which together include all the mss. Each of these subtractions should be devised judiciously so as to involve as few mss as possible. The complete textual diagram can then be made up.
As we have already suggested, the question which needs to be answered at this point is: On what basis can we utilise a textual diagram in order to choose between rival readings? Or, to put it crudely, what are these diagrams for? We can form some idea of how Dearing intended them to be employed by studying one of the examples at the end of his book. Bearing in mind what sort of person is likely to read this thesis, I have selected his treatment of the text of Philemon.

Dearing confines his study to the ten uncial (ACDEFGKLP) cited by Tischendorf, on whose collations he relies. As four of these mss (ACDE) embody corrections made later than the first hand, he deals with altogether fourteen "states" of text.\(^1\) When we examine the different groupings occurring in the course of the epistle, we find them inconsistent. However, if we limit ourselves to the six states C\(^*\)C\(^2\)D\(^o\)E\(^**\)FG, we can form a partial diagram, which may be augmented by four other partial diagrams each involving four mss, to yield fig. B.11.8 in which all fourteen states are included.

\(^1\) A single asterisk after a siglum indicates the reading of the first hand. The four correctors considered in Dearing's study are denoted: K\(^c\), C\(^2\), D\(^o\), E\(^**\) (after Tischendorf's notation).
This tree must now be orientated, and Dearing reasons as follows. In several passages, he is confident that a particular variant reading is not original, being due to homoteleuton, ditography, or the like. The only states in which he cannot at this stage identify a single error are $\lambda^c$ and $P$. As the deductions which Dearing now makes are, to my mind, thoroughly questionable, I shall quote him in his own words, so as to avoid the charge of having distorted his ideas. I have added comments, most of which are explanatory, in square brackets. We take up Dearing's argument on p. 92.

"These readings [which we have branded as not original] show that $\lambda^*, A, C^*, C^2, D^*, D^c, E^*, E^{**}, F, G, K,$ and $L$ are descendants. The dates of the mss show that $\lambda^c$ [cent. vii] and $P$ [cent. ix] cannot be the ancestors of the others. [For example $\lambda^*$ is assigned to cent. iv.] Since $\lambda^c$ and $P$ always have the better readings, and $D^*$ and $E^*$ consistently agree against them in directional
readings [i.e. variant passages wherein we may be confident as to which is the older reading. The reason that Dearing singles out D* and E* is presumably that they are adjacent to \( \xi^c \) and P in fig. B.11.8], the final diagram will conform to the pattern:

\[
\begin{array}{c}
\xi^c \\
/ \\
P \\
\end{array}
\]

[the rest]

The relative dates of the manuscripts will then require that \( C^*, C^2, E^*, \) and \( E^{**} \) be removed from being intermediary between the ancestor [i.e. the archetype, or rather its analogue in what is a textual rather than a bibliographical diagram] and \( \xi^* \). [The "archetypal" state must be at least as old as \( \xi^* \) (cent. iv), but \( C^* \) etc. belong to cent. v or later]" Dearing then prints the tree reproduced in fig. B.11.9.

---

1. I do not claim to understand fully this Scheinlogik.
"This," says Dearing, "is a textual, not a bibliographical, scheme.

He then proceeds to reconstruct the text of the ancestor, and here he applies the mechanical rules of the stemma - despite the fact that a stemma is a bibliographical scheme, which this figure, as he expressly states, is not. In particular he holds that when ₠ and P agree, their common reading is that of the archetypal state, which presumably gives the

---

1. It is, perhaps, because he sensed this illogicality that he instead wrote, when he reached this stage (viz the construction of a textual diagram) in his study of the Synoptic problem (p. 102): "This is a textual diagram, but as there is no bibliographical evidence to the contrary it may serve also as a bibliographical diagram." He makes a similar remark on p. 78, in a study of certain Chaucer mss. But after all his insistence on the fundamental distinction between textual and bibliographical diagrams, the facile suggestion that the former can serve for the latter if there is no evidence to the contrary is surely something of a volte-face.
oldest text that can be attained from the fourteen states employed.

Thus he corrects Tischendorf in five passages:

V. 6) εἰς Χριστόν  

V. 10) ἤν ἤγέννησα ἐν τοῖς δεσμοῖς  

V. 11) νῦν δὲ καὶ σοὶ καὶ ἐμοὶ εὐχρηστὸν  

om. καὶ (10) D  

V. 12) ἤν ἀνέπεμψα σοι, ἀντίν, τούτ' ἔστιν τὰ ἐμὰ σπλάγχνα: ἤν ...  

D  

V. 25) D adds ἀμὴν at the end.

Whether Tischendorf or Dearing is right in these different passages, is a question which I leave to those who are better qualified to answer it.

What we must attempt to do here, however, is to evaluate Dearing's whole procedure.

---

1. This is a basic tenet of stemmatics. Thus Dom Quentin (Essais de Critique Textuelle, 1926, p. 37) speaks of "l'archétype qui, en somme, est la forme du texte la plus voisine de l'original à laquelle on puisse arriver par la voie des manuscrits conservés."

2. At least in V. 12, the text favoured by Dearing has few supporters among modern scholars of the N.T. text. Thus A. Lukyn Williams, in the Cambridge Greek Testament (volume on Colossians and Philemon, 1907) accepts Tischendorf's choice of readings. αὐτὸν, he believes, is inserted for the sake of emphasis. "Its object is to bring Onesimus vividly before the reader, and thus prepare the way for the strong contrast τούτ' ἔστιν τὰ ἐμὰ σπλάγχνα" (p. 183). On similar lines, the New English Bible translates the verse: "I am sending him back to you, and in doing so I am sending a part of myself". According to Williams, οὐ δὲ and προσλαβοῦ, adopted by Dearing, are not original; their insertion is evidently due to the difficulty of αὐτὸν after the relative (op. cit. p. 172), and, I presume, to some extent inspired by V. 17 (ἐί ὦν με ἔχεις κοινωνίαν, προσλαβοῦ αὐτὸν ὡς ἐμέ).
It would not be difficult to attack the theoretical basis of his method. However, there may be many who feel convinced that Dearing's methods are found empirically to give acceptable results, and who would remain unmoved by any rebuttal, however compelling, of the underlying theory. I have therefore preferred to put the method to the test by means of a simple model tradition.

---

1. Basically I feel that Dearing is trying to have the best of two worlds. When, in one of his textual schemes such as Fig. B.11.9, he draws a line going down from state A to state B, then either this signifies that B is actually derived from A or it does not. If it does, then Dearing owes us a rigorously argued rationale to explain why his procedures — in particular, the overlaying of the several partial diagrams resulting from a series of subtractions — should be expected to lead to an accurate account of the historical relationships between the states. On the other hand, if the lines of these textual diagrams do not necessarily indicate direct descent, then he should have stated by what authority he bases on such a diagram any decision whatsoever between rival readings, and in particular applies to it the mechanical rules which are appropriate to a stemma and which were never intended to be used except on the understanding that that stemma does represent correctly the inter-relations of the mss. Compare the remarks above (pp. 2:35 f.) on the theories of A.A.Hill.
Let A be the original ms of a certain hypothetical tradition, and let copies then be made according to fig. B.11.10. A dotted line denotes the use of a subsidiary source.

Most of A's text is transmitted intact to all five descendants; but there are eight readings which undergo changes in one or more mss, and we shall denote them abcdefgh. B copies A correctly, except that he unconsciously replaces b by the error m. C, whom we shall imagine to be somewhat officious, writes down a different text in no less than six places; for acdefg he puts nopqrs respectively; of these, opqrs are fairly plausible readings, but n is an inadvertent error which gives no tolerable sense. D is an almost faithful copy of A, except that t is unwittingly substituted for h. E is copied from C, but occasionally uses B; hence E corrects away C's bad reading n, and also chooses f (from B) rather than r (in C). Similarly, F used C and D; he kept C's text except for bringing in from D the readings ac for n and o respectively. The resulting texts are shown in the following table:
TABLE B.11.1

Readings

\[
\begin{align*}
A &: a\ bc\ d\ e\ f\ g\ h \\
B &: a\ m\ c\ d\ e\ f\ g\ h \\
C &: n\ b\ o\ p\ q\ r\ s\ h \\
D &: a\ b\ c\ d\ e\ f\ g\ t \\
E &: a\ b\ o\ p\ q\ f\ s\ h \\
F &: a\ b\ c\ p\ q\ r\ s\ h
\end{align*}
\]

Suppose now that A is lost and that we seek to reconstruct its text. We shall suppose that we still have the five remaining mss, and that we know EF to be a number of years later than BCD. The following groupings will then be observed in the eight passages:

- BDEF:C
- CDEF:B
- BDF:CE
- BD:CEF (three times)
- BDE:CF
- BCEF:D

There is a conflict between the third and the fifth. However, if we subtract C, the groupings reduce to:

---

1. The first grouping will of course disappear, in that all four mss that remain agree.
DEF: B BDF: E BD: EF (3x) BDE: F BEF: D,
wherein no conflict occurs, and we obtain the partial diagram:\(^1\):

```
  B
 /\ 
D   E
 / \ 
F   C
```

Another way of removing the conflict is to subtract E. The groupings will become:

BDF: C (twice) CDF: B BD: CF (four times) BCF: D

For this collection, Dearing prescribes\(^2\) the textual diagram

```
  B
 /\ 
D   F
```

Combining the two partial diagrams, we obtain fig. B.11.11a.\(^3\)

---

1. That this is the textual diagram appropriate to this particular collection of groupings, is explicitly stated (p. 28, lines 3-4).
2. See p. 28, lines 2-3.
3. A third subtraction which would have given consistent groupings would be that of F. Combining the resulting partial diagram with the first, we obtain the overlay

```
  B——- E
    D   F
```

Dearing admits (p. 40) the possibility that two or more overlays may be equally satisfactory, and suggests criteria for choosing between them; in this case, however, those criteria do not favour either diagram against the other. Our conclusions belong good whichever alternative is chosen.
This brings us to the orientation. We have eight passages, in each of which there are two alternative readings. Let us suppose, realistically enough, that we can identify with some confidence which is the older reading, in a few but not in all of the variant passages. To be specific, we shall suppose that we can identify as errors the readings m n t, but that we as yet hesitate—in the hope that the stemma will help us choose—between c d e f g (the true readings) and o p q r s (plausible emendations introduced by the hypercritical scribe C).

Then we may follow precisely Dearing's treatment of the Philemon mss. Each of the mss BCD has one of the errors so far detected (mnt), so all three are descendants. As for EF, we have not identified an error in either. As we know them to be later than BCD, neither can be the ancestor of all the rest. The orientation must therefore be as in Fig. B.11.11(b), which indicates textual relationships bearing no resemblance at all to the true descent of

![Diagram](attachment:image.png)

Fig. B.11.11.

the states, shewn in fig. B.11.10.

1. The reader will probably be mystified by this argument unless he refers back to the Philemon study (p. 11:34).
The method has therefore failed to provide us with an accurate history of the text. Will it put up a better performance in helping us to choose between rival readings? This is easily ascertained. Following the precedent of Dearing's Philemon study, we shall have to accept any reading supported by both E and F. Thus, over the five passages in which we look to the stemma for guidance in our choice, we shall be directed three times to the wrong reading (viz when these mss agree in p, q and s). We could hardly have done worse if we had instead decided between rival readings by tossing a coin. Anyone entrusted with the task of editing a text would be well advised, before applying these methods, to think not only twice but - in Dearing's curious Latin¹ - tris.

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¹. pp. 74, 90, 91.
METHOD OF P. CANIVET AND P. MALVAUX

Reference

"La tradition manuscrite du Περί τῆς Θελας ἀγάπης", Byzantion (1964), pp. 385-413

This study was undertaken as a step towards the preparation of a critical edition of the above-named work 1 of Theodoret, Bishop of Cyrrhus (c. 393-c. 458 A.D.). The investigation was carried out in close collaboration between its two authors, Canivet being an accomplished scholar of patristics and Malvaux of mathematics. Out of 30 mss known to contain that discourse, Canivet collated 21, out of which he extracted 15 as a basis for this piece of research. These fifteen mss are denoted by the sigla given in Table B.11.2.

TABLE B.11.2

Mss employed in the investigation by Canivet-Malvaux

<table>
<thead>
<tr>
<th>Siglum</th>
<th>A</th>
<th>B</th>
<th>C</th>
<th>E</th>
<th>F</th>
<th>G</th>
<th>H</th>
<th>N</th>
<th>P</th>
<th>Q</th>
<th>R</th>
<th>T</th>
</tr>
</thead>
<tbody>
<tr>
<td>Century</td>
<td>9</td>
<td>11</td>
<td>10</td>
<td>10</td>
<td>12/13</td>
<td>11</td>
<td>15</td>
<td>11</td>
<td>11</td>
<td>12/13</td>
<td>13</td>
<td></td>
</tr>
<tr>
<td>W</td>
<td>Y</td>
<td>Z</td>
<td></td>
<td></td>
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<td></td>
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<td></td>
</tr>
</tbody>
</table>

Canivet found that these mss resisted the usual methods for stemmatic classification, and so the data was handed over

1. See Migne, P.G. lxxx11, 1497-1521.
to Malvaux, who attempted to obtain a stemma in the following way.

Malvaux begins by stating that his approach is based on the axiom that textual similarity may be used to estimate historical relationship (p. 390): "La méthode proposée postule essentiellement le principe suivant: deux manuscrits copiés sur un même manuscrit (a), directement ou par l'intermédiaire de manuscrits disparus, ont des chances de diverger entre eux moins souvent que l'un ou l'autre de ces deux manuscrits et un troisième copié sur un manuscrit (b), les manuscrits (a) et (b) ne dérivant pas l'un de l'autre." This ought to put us on our guard, because it was shewn by Greg (p. 59) that textual similarity between mss is not a sure guide to the degree of historical connection between them, and in Thes., p. 3:7 a counter-example is given which shows how unsafe that axiom is.

But let us return to consider the method itself. The different ways in which the mss grouped themselves in different passages were noted, and thence it was observed how often each grouping repeated itself. There were found to be 585 places where the ms diverged, and 200 different groupings. On pp. 395 ff. are listed all those groupings which are attested twice or more; there are 48 of these. From the data over the 585 variant passages, the distance

---

1. According to the terminology used so far, these are absolute distances. Apparently all 15 mss are well preserved over this discourse.
between each ms pair was calculated; these are presented in a table (p. 395). Thence Malvau proposes to identify a number of small groups. For example, the three mss AYZ are considered to form a group because these three mss are far closer to one another than to any other ms. In this particular case, the distance between any pair of mss of which both belong to the group is found to be 44 or less, whereas any distance between a pair of mss of which one belongs to the group and the other does not will be found to be 123 or more. By such reasoning Malvaux identifies five small groups, viz AYZ; BR; CFHP; ENT; GQW. He appears to be certain (p. 398) that each of these groups possesses, ipso facto, an exclusive common ancestor.

The next step is to determine the relationship between the five families. Malvaux argues that at this stage we must discard a number of the variants, viz (a) those in which one ms is isolated from the rest (for example, $\Sigma$:N), (b) those in which all the members of one of the five groups agree against the other mss (e.g. $\Sigma$:ENT). The reason he gives is that a variant $\Sigma$:N must be presumed to be due to an error committed by N (or by an exclusive ancestor of N), and that we may similarly ascribe a variant $\Sigma$:ENT to an error on the part of an exclusive common ancestor of the family ENT. Thus variants of types (a) and (b) are irrelevant to the recovery of the higher regions of the stemma, wherein it will be defined how the five families stand in relation to one another. All these variants are therefore eliminated,
and the distance between each ms pair is calculated anew' (p. 400). Malvaux then considers the varying degrees of similarity which this second comparison reveals between the mss, with particular attention to the bearing of these results on the similarity relations between the five families which the mss are supposed to represent (pp. 401-3), and arrives at the following "stemma probable":

1. Malvaux invites us to think of fifteen "manuscrits fictifs" denoted $A^0B^0$... to correspond to the real mss $AB$... He defines $A^0$ to be identical with A, except that in places where either A or the family of which A is a member (viz AYZ) is isolated from the other mss, $A^0$ shall have the reading common to the majority of the mss. The other mss of the series are similarly defined (p. 399). The new distance $AB$ may be regarded as $A^0B^0$. I can see little advantage in this concept, in that there is no reason for supposing that ms exhibiting texts such as these ever existed; in particular, the remark that "les manuscrits réels peuvent être imaginés copiés sur les fictifs" is quite unjustified.
The dotted line indicates contamination. The relationships set up between the mss within each group (e.g. the suggestion that T is derived from E) are, he expressly states (p. 404), "simply possible" (i.e. the evidence admits not only the possibilities indicated, but others too).

It will now be clear that, as Malvaux states in the passage quoted above, this method does indeed rely throughout on textual similarity as an index of historical connection. In view of the unsoundness of such deductions, the logical basis of the whole procedure is very doubtful. Even so, if we found empirically that it was capable of yielding an acceptable stemma, then it would still be of great value. Let us therefore consider in greater detail the implications of this stemma for the text of Theodoret, as far as we can judge from the passages discussed in the article.

In the main, the readings to which the stemma leads proved to be consistent with Canivet's own judgment. There are passages, however, where a reading which is attested within the group FCHP only and ought therefore, according to the stemma, to be rejected, nevertheless seems on intrinsic grounds to be original. In such cases Canivet either decides regretfully against that attractive reading, or accepts it at the price of having to set up somewhat complicated hypotheses. I have observed the following instances:

(1) p. 405: A passage is discussed where FCHP read ἐγκαταλείπω, while the other mss have either καταλίπω or
καταλέιπω. Canivet gives reasons for which the heavier compound form ἔγκαταλείπω would be "seduisante", but is forced by his stemma to reject it.

(ii) p. 410 Migne has (p. 1510 f.): "ὤσπερ γὰρ ἐγὼ, φησι; σαλευδόμενον οὖν ὑπερεξῆδον σε, οὕτως ἔρεισμα γενοῦ καὶ σὺ τοῖς δονομένοις τῶν ἀδελφῶν σου .... The reading οὖν ὑπερεξῆδον is supported by most of the mss, viz QGWAZYN [EBRT]. However we find in HFTpc [CP] the reading ὑπερεξῆδω (without οὖν), and, as Canivet says, there can be no doubt that that is what Theodoret wrote. Canivet shews convincingly how the reading οὖν ὑπερεξῆδον could have arisen: The verb ὑπερεξῆδω is not common, and could easily have been mistaken for a part of ὑπερορῶ; the insertion of οὖν would then have been necessary in order that a satisfactory sense be obtained. But how are we to account for the presence of οὖν ὑπερεξῆδον in both branches of the stemma? Canivet is forced to suppose that this error first arose in (abe) and then contaminated (QGW).

(iii) p. 411: This variant occurs in a passage where Theodoret is developing a theme found in 1 John 4:19, viz that we should love God because it was He who first extended his love to us, and not vice versa. Migne's text has οὖν ἣμετες ἡγαπήσαμεν πρῶτοι, ἀλλ' ἡγαπηθέντες ἀντιηγαπήσαμεν. The reading πρῶτοι is supported by BRAZYETN, while QGW ....

1. The square brackets are necessary because phonetic confusion between ι and ει has given rise to the reading οὖν ὑπερέξειον in some mss.
2. CP have ὑπερέξειον, cf previous note.
have πρῶτον, evidently a corruption thereof. However, FCHP read πρῶτεροι which, as Canivet states, has much to commend it, in that a change from πρῶτεροι to πρῶτοι could be explained far more easily than the reverse. Firstly, in contexts such as this one, where two parties are involved, πρῶτεροι is usual in classical writings, but πρῶτοι tended to be preferred in later times. Secondly, the verse from 1 John on which this passage is based has ἄτι αὐτῶς πρῶτος ἡγάπησεν ἡμᾶς, and this would tend to make a copyist who was familiar with the text of 1 John write πρῶτοι rather than πρῶτεροι. Canivet concludes: "πρῶτεροι, lectio difficilior, sera donc retenu"; he suggests that (abe) had the error πρῶτοι, and that (QGW) came to share it either on being contaminated thereby or through an independent assimilation to the N.T. text.

(iv) p. 405. This is an instance where the mss offer four different readings:

οὐκ ἀφαίρεται τὴν δύναμιν, ἀλλ' αὕξει τὴν δύναμιν F
" " " " " " τὴν ἐφεσίαν CHP
" " " " " " τὴν Ἰοχανν EN
" " " ἀλλ' αὕξει τὴν δύναμιν QGWAZYBR

Canivet chooses the reading of F, because τὴν δύναμιν occurs twice therein and the other three readings can each be regarded as an attempt to avoid the repetition. He must surely be right; but this is not the reading to which the stemma would have led us, viz the fourth of those listed above, which is the only one to be attested in both branches.
In order to keep F's reading, we must suppose that an xca of QGW on the one hand, and an xca of AZYBR on the other, omitted the former τὴν δοξαμαίνω independently.

Thus, among the modest number of passages which Canivet discusses (about twenty), we find as many as four in which the stemma does not yield that one of the available readings that is best on intrinsic grounds. It is natural to ask whether we can obtain a more satisfactory stemma by employing Froger's method, the logical basis of which we have checked in detail (Ch. 1).

Virtually all the material we need can be found on pp. 395 ff of the article, where a list is offered of all groupings which occur more than once. Not all the groupings listed are stemmatically consistent; for example, we learn on p. 397 that four instances occur of each of the two groupings

AYZBR - CFHP ENT GQW
AYZ CFHP - BR ENT GQW

In order to choose a construction domain, we list the two-way splits in decreasing order of frequency until we reach one which is stemmatically inconsistent with one that has gone before. In Table B.11.3, A is used as the base ms, and each two-way split is denoted by whichever of the two ms sets does not contain A (i.e. by the variant set).

---

1. On this concept, see Thes., p. 1:33; there the reader will find the theoretical background to the procedure which we are about to put into practice.

2. i.e. a selection of the variant passages, which is to be the basis of the stemma. See p. 1:16.
We are now at the "noise-level", because some of the groupings which occur four times conflict with some of those above, viz:

BRGHPENTGW (no. 49), which conflicts with BRENt;
BRENTGW (no. 38), which conflicts with CFHPGW;
CFHPENTGW (no. 29), which conflicts with BRENt.

Hence we have a construction domain embracing all groupings attested more than four times. That domain consists of 356 two-way-split occurrences, out of a total of some number between 414 and 552\(^1\) - i.e. between 64%.

---

1. The list of splits on pp. 395 ff covers 414 two-way-split occurrences (out of a total of 447 variant passages wherein the miss split into two or more groups - according to one of the 62 groupings there listed). In addition, there are 138 variant passages not included in that list (see p. 397). As we are not told how many of these 138 splits are two-way, we can say only that the total number of two-way-split occurrences lies between 414 and 552.
and 86% of the total number of places throughout the
discourse where the mss were observed to divide into
two groups. The first steps in applying Froger's method
to the collection of variant sets shown in Table B.11.3,
are to add to the collection the set AYZBCFHPENTGQW
consisting of all the 15 mss, and then to draw a diagram
(Fig. B.11.13) wherein each set is placed below any set
wherein it is included. Note that it was necessary to
vary the order of the mss within the sets in order that
they could be so arranged.  

---

We must now eliminate, for each ms, every occurrence
thereof in the diagram except for that one which is placed
the lowest. Hence we obtain the tree of Fig. B.11.14:

---

1. The whole procedure is explained in Thes., pp. 1:35 ff.
This tree must now be properly orientated. If we pick it up along the arc denoted (a), we shall obtain a tree which is essentially the same as Malvaux's (see Fig. B.11.12); the only differences concern the relationships within the five families, about which Malvaux did not wholly commit himself (p. 11:53). Thus Froger's method has led to virtually the same network as Malvaux's, on a more satisfactory logical basis and with far less effort; in particular, distance tables do not have to be computed. However, we know that objective evidence — which is all that has been employed so far — will not suffice to determine the true orientation, and so it may be possible to find a better "topmost point" than (a).

When we turn — as we now must — to questions of the rightness and wrongness of variant readings, two facts emerge which shew that the network must be lifted up from
a point on the arc (b):

1) There are a number of places, as we have already seen, where CFHP alone have the original reading, and all the other mss agree in the same erroneous reading. Hence Ω (the archetype) must lie along the arc (b) or below it.

2) On the other hand, CFHP sometimes agree in error. Thus on p. 409, Canivet refers to a passage where the true reading is doubtless ἡλίωνος, and CFHP agree against all the other mss in reading νηδος. Again, on p. 408 he discusses a passage where the mss show a three-way split:

<table>
<thead>
<tr>
<th>πῶθεσις</th>
<th>BRAZYTEN</th>
</tr>
</thead>
<tbody>
<tr>
<td>πῶθεσις</td>
<td>FCHP</td>
</tr>
<tr>
<td>γνώσεως</td>
<td>QGW</td>
</tr>
</tbody>
</table>

Canivet argues in favour of πῶθεσις, which yields a figure of drinking that leads to what he calls "l'ivresse spirituelle"; πῶθεσις is an extremely rare word, which Theodoret would hardly have used. Thus, here too, CFHP have a common error. Hence Ω cannot lie below the arc (b).

---

1. Some of the other mss have different incorrect readings (e.g., ἡλίωνος AZY), but that does not alter the fact that the error common to CFHP indicates that they have a common (though not necessarily exclusive) ancestor later than the archetype.

2. For a full discussion of the merits of each reading, see Canivet. Note also that as πῶθεσις is not the only error attested here, the provisions of the last note apply here too.
Thus $\Omega$ must lie along (b); hence the following stemma:

Unfortunately, however, we have not quite finished yet. This stemma needs to be modified in four ways:

1) $P$ (dated cent. x1) cannot be an ancestor of $C$ (cent. x). We can deal with this by adding to the construction domain the 3 occurrences of $\Sigma : P$. This will yield an extra variant set \( \{ P \} \), and the ms $P$ will be put on to a separate branch (see fig. B.11.16 below).

2) Similarly $G$ (cent. x11/x111) cannot be an ancestor of $Q$ (cent. x1). We shall therefore admit the two occurrences of $\Sigma : G$ to the construction domain.

3) YZ have been assigned the same position in fig. B.11.15, because in all the passages of the construction domain these two ms are identical. However, they are not identical throughout the discourse; on p. 395 we are told that they differ 22 times, and that each has 2 unique
readings. It therefore seems expedient to include in our construction domain the variants Σ:Y (2 occ.) and Σ:Z (2 occ.).

4) One of the passages discussed by Canivet (p. 408) makes it unlikely that C is derived from F, even though chronology admits the possibility (both C and F are assigned to cent. x). Here Theodoret writes that the closer one comes towards achieving intimacy with God, the more one's desire for that intimacy is heightened rather than being satisfied. He continues (Migne, p. 1501):

Τοιούτος ἦν Μωσῆς ... δε πάλλαμις, δες ἐφικτον ἄνθρωπον, τῆς θείας θεωρίας δεξιωθεὶς ... οὐ μόνον κόρον οὐκ ἔλαβεν, ἀλλὰ καὶ σφόδροτέραν καὶ σφοδροτέραν τὴν ἐπιθυμίαν ἐκτῆσατο. Καθάπερ γὰρ τινα κάρον ὑπὲ τῆς τοῦ ἔρωτος ἐκεῖνου δεξιώμενος μέθης, καὶ τῷ φίλτρῳ λιαν ἐκβακχευθεὶς, τὴν μὲν οἰκείαν ἡγνώσας φύσιν, ἐπεθύμησε δὲ ἴδεῖν, ἀ μὴ θέμις ἴδεῖν.

The word which concerns us here is κάρον, accusative of κάρος, which Liddell and Scott translate: "heavy sleep, torpor, such as follows drunkenness"; here it denotes a state of ecstasy in which Moses ran to excess, as it were, and asked to see the glory of the Lord. Now C has κάρον, but F reads κόρον, acc. of κόρος, to which Liddell and Scott attribute two meanings: "1: satiety. 2: insolence." It is very likely that κάρον is original, because it is easy to explain a scribal change κάρον—κόρον; the noun κόρος, in the sense "satiety," has just been used

1. Exod. 33:18.
2. C is accompanied by ἈΔΥΒΡΗΤΙ, and F by ΗΡΓΩῼ. However, all that concerns us here is the relationship between C and F.
several times, and the very form χόρον occurs, as we have seen, in the previous sentence; but I doubt whether a scribe could have obtained χάρπον from χόρον by conjecture. Again, χάρπον gives an excellent sense; but if we read χόρον, "satlety" does not fit, and although we get a tolerable sense if we take χόρον as "insolence", the fact that Theodoret uses the word, everywhere else in these pages, to mean "satlety", makes it unlikely that he intended "insolence" here. Hence we shall not be able to account for χάρπον in C if C is wholly dependent on F. This difficulty is met if we add the variants Σ : F (attested three times) to the construction domain.

It will be objected that by making these four adjustments we are "cooking" our results. The charge cannot be denied, but I would plead for leniency on two counts. First, only the relationships within the families are affected, and in the majority of variant passages to which the stemma will be seriously applied, these "within" relationships are irrelevant because the members of each family do not differ among themselves. Second, the fact that we have observed certain groupings with frequency 4 which seem to be due to contamination, coincidence in error, and so on, and which must therefore be left out of account in the construction of the stemma - this fact does not necessarily mean that all groupings which are attested four times or less are due to those causes. The function of the noise-level is to fix, in an objective way, a boundary which implies a presumption that only those groupings whose frequency exceeds that noise-level are to be admitted to the construction domain; but the evidence
available to us may be found, in certain cases, to contradict that presumption¹ and we must then be allowed to override it.

Thus we arrive at the following stemma:

---

1. It is quite possible that, had I known more about the readings of the mss throughout the discourse, I would have found further adjustments necessary, e.g. to make B a collateral of R rather than an ancestor.
What are the implications of this stemma, as opposed to the one proposed by Canivet and Malvaux (see fig. B.11.12)?

First, in passages wherein CFHP agree against the other mss, we now have the option of accepting the reading of the former. If the reader will refer once more to the passages discussed on pp. 11:53, he will find that we can now adopt the attractive ἵγνωται ἠπώ, and that ἵππαρδὼ and πρόερω can be regarded as original without our needing to postulate contamination or coincidence in error. Again, when F, CHF and the other mss all differ from one another, it will now be open to us to adopt the reading of F without having to overrule the stemma, whereas in passage (iv) above we had to reject the reading prescribed by the stemma in order to follow F.

Second, in passages wherein FCHPQGW agree against AYZETNBR, we are now enjoined to prefer the former, whereas the stemma of Canivet-Malvaux allowed us to choose freely between the two readings. It turns out, however, that in both the places discussed by Canivet where that grouping appears, the reading which he found preferable was that of F etc. Thus he states (p. 407) that

δροῦς FCHP GQW is superior to νόμους AYZ BR ETN and τὸ θέλος " " " τὴν θέρμην 1 "

1. We mention here also three passages (pp.407 f.) wherein the mss differ with regard to what Canivet calls "enclave"—a term with which I am not familiar. Canivet's judgment is against adopting the "enclave" in any of these places. This is interesting because (i) FCHP are the only mss to be consistent, in all three passages, in not practising the "enclave", and (ii) in the third passage, FCHP are supported by Q (and GW?). Thus we find once again that Canivet gave preference to the readings of FCHP, especially when they are joined by Q.
Thus the loss of our former option does not seem to entail any grave disadvantage; or, to put it another way, we have no evidence of an error common to the ms FCHPQGW alone. This tends indirectly to support the new stemma against that of Canivet-Malvaux.

Apart from the case of \( \varepsilon \gamma \chi \alpha \tau \alpha \lambda \varepsilon \lambda \iota \nu \omega \), there is only one passage among those discussed by Canivet wherein the new stemma leads me to disagree with his choice of reading. On pp. 411f. he deals with a variation within a quotation, introduced as such, from 2 Cor 5:14. The mss offer:

- \( \text{QGWBR} \): \( \acute{a} \pi \varepsilon \theta \alpha \gamma \alpha \nu \varepsilon \ \iota \nu \alpha \ \iota \zeta \omicron \nu \tau \varepsilon \varsigma \)  
- \( \text{ETN} \): \( \acute{a} \pi \varepsilon \theta \alpha \gamma \alpha \nu \varepsilon \ \iota \nu \alpha \ \iota \zeta \omicron \nu \tau \varepsilon \varsigma \) 
- \( \text{FCHP AZY} \) (so text of N.T.): \( \iota \nu \alpha \ \iota \zeta \omicron \nu \tau \varepsilon \varsigma \)

In the stemma of Canivet-Malvaux, both the first and the third of these readings are attested in both branches. Canivet is suspicious of the third, which may be due to assimilation to the N.T. text, and he would regard the short reading of QGWBR as that which Theodoret wrote. However, if we adopt the new stemma, then the only reading to be attested in both branches is the third. Although its agreement with the N.T. text may be held to count against it, there is much to be said in favour of this reading. First, we have a Bible commentary which is attributed to Theodoret (see P.G., vols 80-82), and before commenting on 2 Cor 5:14 he quotes the whole verse in a
form agreeing with the N.T. text, and hence with the third of these readings above\(^1\). Second, the other readings available can be explained very easily in terms of eye-skip; a jump from \(\textit{ἀπέθανεν} (1^o)\) to \(\textit{ἀπέθανεν} (2^o)\) will account for the reading of QGWBR, and a jump from \(\textit{ἀπέθανον} \) to \(\textit{ἀπέθανεν} (2^o)\) for that of ETN.

The article of Canivet-Malvaux does not offer enough material to enable us to go further in comparing the respective merits of the two stemmata in other passages. However, I hope that enough has been said to shew that Froger’s approach is by far the less laborious, rests on more secure logical foundations, and leads to more acceptable results.

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1. I have consulted the text of Migne (which is based on Sirmond) and the edition of J.A. Noesselt, Halle 1761 (which is also based on Sirmond but is claimed to have some independent value in that mss also were consulted). I do not understand why Canivet regarded the evidence of Theodoret's commentary as confirming his own choice of the first reading: "d'ailleurs c'est cette leçon brève qu'on lit dans le Commentaire suivi de Théodoret sur 2 Cor. (P.G. 82, 409 B 10-11)".
Method of J. van Leeuwen

References

"Pindaros' tweede olympische ode" (1964), by J. van Leeuwen: Appendix A, entitled "Schetsen van een computeronderzoek betreffende de overlevering van handschriften die de ode bevatten", in vol. 2, pp. 305-324, written in collaboration with S.G. van der Meulen.

Mr. J.C. Griffith tells me that in the March 1970 number of Calculi there appeared a notice of a dissertation "describing the use of computers to develop (sic) the relationships of the MSS containing the Second Olympian Ode of Pindar". I myself have not seen either the notice or the dissertation, and so my comments cannot go beyond the one work referred to above, which is of course written in Dutch.

The work of J. van Leeuwen represents another attempt to derive a history of the mss from the textual data. It is of special interest as the earliest published text-critical analysis (to my knowledge) embodying results which were obtained by the use of a computer.

In this study, van Leeuwen confines himself to the second Olympian Ode of Pindar; that poem is quite short (604 words) but rich in variants. Out of all the ms evidence, he selects for the analysis twenty-four of the better-known witnesses.

The study tacitly pre-supposes a particular text of the ode, which we may call the "basic" text. For this text, at every point where the mss diverge, one particular reading is adopted from the outset. Any reading which is attested among the mss but is not the one adopted in the basic text is called a "variant". Van Leeuwen states that the ode yields 186 such "variants".
is chief objective now is to detect which readers (if any) of the collection of extant mss under analysis are descended from other members of that collection, or in other words to identify whatever ancestral relations exist among our extant mss. This is achieved by invoking the following hypothesis - and, as we shall presently see, a generalised form thereof. The hypothesis itself runs:

If we have two mss X and Y such that Y contains all the variants (in van Leeuwen's sense) found in X and other variants besides, then we may postulate that Y is derived from X.

In this form, we may denote it H(0). We may however broaden it by introducing a small number t, to be called the "tolerance and then setting up the hypothesis:

If Y contains all the variants found in X, except for a number not greater than t of those variants and contains other variants besides, then we may postulate that Y is derived from X.

We may denote this hypothesis H(t).  

The greater the tolerance, the more ancestral relations will be apparent; more specifically, if we increase the tolerance from a given level, all the ancestral relations postulated at the earlier level will remain, and others may

1. Van Leeuwen is at pains to point out (p. 308) that all such inferred relationships are to be regarded as possible, not as proven.
well be added to them. We may therefore take various ms pairs, and various values of t, in order to see whether $H(t)$ is fulfilled and an ancestral relation posited. This task was assigned to the computer.

An important point is that van Leeuwen sets up a list of twelve properties which a variant might or might not possess (e.g. that it is attested in a papyrus, or alters the metre, or consists of an omission of a whole word), and he assigns to each variant a "key-word" which records whether or not that variant possesses each of those twelve properties. It is therefore possible to conduct the tests on the whole field of variants available, or to restrict one's attention to those variants which possess some particular combination of properties; in the latter case, the computer will readily pick out by means of the key-word those variants which satisfy the particular conditions that are set. It follows from the nature of $H(t)$ that, for a given value of t, (1) whatever ancestral relations are postulated when we consider a given field of variants, will remain if we subsequently confine ourselves to a subset drawn from that field; and (2) the smaller collection may well yield other ancestral relations which were not yielded by the larger.

1. The key-word consists of a twelve-digit binary number, such as 011110011100. The total number of possible key-words is said to be $2^{12} = 4096$, but is in fact somewhat less, in that not all the properties are independent (e.g. "consisting of an addition" and "consisting of an omission") and thus not all the theoretical combinations are possible.
Thus the computer was programmed to examine all the possible ms pairs and to pick out all the ancestral relations for given values of t and over specified collections of variants. The computer produced, for each experiment, a matrix showing whether or not an ancestral relation exists between all the possible pairs of mss. Van Leeuwen remarks (p. 307) that there is no reason why instances should not be observed, by these means, of a ms being derived from more than one parent, and such instances did in fact appear. From these relationships a diagram which purports to show the textual history may be drawn.

A series of experiments was thus carried out, with different values of t (never exceeding 12) and differing sets of variants (the field being in one instance narrowed to such an extent that not one of the mss contained more than 19 of the variants admitted to the analysis - p. 311). In the first experiment, the tolerance was zero and all the 186 variants were considered; this yielded the relationships shown in fig. B.11.17, which however present certain
unsatisfactory features. These same relationships were yielded by all the other experiments; indeed, it follows from our earlier remarks that matters could not have been otherwise. Beyond that, however, results from different experiments display great inconsistency. In his conclusion (p. 324), van Leeuwen did not appear to be entirely satisfied with his results; he accounted for this disappointment on the grounds that the modern critical editions on which he had depended for his data did not report the manuscriptings in detail sufficient for his purpose. The method itself, however, he regards as a promising one, "die verder ontwikkeld dient te worden".

Let us now consider the assumptions which this method pre-supposes. The central hypothesis is of course $H(t)$. We may start with $H(0)$ and suppose that we have two mss $XY$, such that $Y$ contains all the variants found in $X$ and others also. How are all we justify the inference that $Y$ is derived from $X$?

We shall make no progress unless we assume that all - or at least the great majority - of the variants are errors.

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1. The dates are surprising: cod. I is assigned to cent. xv, whereas its "descendants" CDOQ are thought to go back to the end of cent. xiii or the beginning of cent xiv. Van Leeuwen remarks (p. 32) that cod. I contains many variants which have not been reported in critical editions, and that any apparent ancestral relationships between I and another ms cannot be relied on. If we do discard the, however, there is very little left.
For let us suppose that in a substantial proportion of the variant passages, the reading (or one of the readings) defined to be a "variant" is not an error, and hence the reading of the basic text is an error. $H(0)$ leaves open the possibility that $Y$ may contain any number of variants not present in $X$; many of these might be variants which are not errors, and on that assumption there will be an indefinite number of passages where $X$ has an error and $Y$ the true reading. In such a situation, the desired result (1) is admittedly a possibility - if we assume an indefinite number of successful corrections - but a far

\[\begin{align*}
X & \quad Y \\
Y & \quad X \\
(1) & \quad (11) \\
\end{align*}\]

likely relationship is that given by (iii).

\[\begin{align*}
X & \quad Y \\
Y & \quad X \\
\alpha & \quad \frac{\alpha}{\alpha} \\
(11) & \quad (iii) \\
\end{align*}\]

It follows that the selection of readings for the basic text must not be arbitrary but must in fact represent an attempt to compile the text of the original (or rather the archetype). Thus "variant" becomes little more than a euphemism for "error". On that footing, it is clear that if $Y$ contains all the errors found in $X$ and others too, then (1) is a very strong possibility. Indeed, if we adhere to the assumptions of Ch. 1, we may go so far as to say that it is the only possibility. No. (11) is ruled out by assumption V ("no successful correction of exemplar"). We may also exclude (iii); for let us suppose that (iii) is the true relationship. Now $X$ shares all its errors
wit Y. This cannot be coincide tal (IV); these errors must have been present in their latest co on ancestor α. Therefore all the errors of X were already prese t in α; hence no errors were committed in the copy1 g process from α to X. T is contradicts VI; hence (111) can be ruled out. We are left with (1) alone.

What can be said of H(t), with t greater than zero? Suppose now that Y co tains all but t of the errors found in X and others also. Then (1) is again a strong possibility; we shall have to suppose that B managed to correct away these t errors, and that may be a reasonable hypothesis. On the other hand, the facts can also be accounted for by (111): perhaps the errors common to X and Y go back to α, while the t errors found in X alone arose in the copying process from α to X. In trying to decide between these two possibilities, we shall have to consider the nature of the readings themselves; but the reater t is, the more the balance of probabilities will incline in favour of (111), and the less reliable the hypothesis H(t) will be.

This is then the logical background to v n Leeuwen's procedures. With regard to the method itself, I would offer the following comments.

We may first consider the implications of the point m de above, that the method will not yield results unless, in the gre t majority of the passages where the mss diverge, the reading chosen for the basic text is the oldest of all the readings there attested by the mss. Hence the computer
analysis will be in some degree dependent on these authenticity judgements which will have already been made throughout the text. This in turn involves two grave disadvantages:

1) The historical diagram eventually obtained cannot be utilised in order to help us choose between rival readings. For at any passage to which we might seek to apply the diagram for that purpose, a previous decision has already been made on this very question and has had some effect in the compilation of the diagram, hence the use of the diagram to decide which is the oldest of the variant readings available would entail to some extent a circular argument⁷. This is acknowledged from the outset by van Leeuwen (p. 305). Thus such a diagram cannot fulfil what would normally be one of its principal functions; it is, at the most, of purely historical interest.

2) Scholars will vary in their authenticity judgements, and thus an analysis by one investigator would be quite unacceptable to another who disagreed with many of the readings adopted for the basic text by the former. Perhaps the text of Pindar is generally agreed on; but in the study of biblical texts, with which this Thesis is primarily concerned, the degree of uncertainty – and consequent dis-

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1. In particular, if the diagram tended to confirm that our original judgment was correct in a given passage, the value of that confirmation would be dubious. We shall enlarge on this point later (pp. 11:78) in connection with the work of M. Béve et al.
agreement - about the relative merits of the available readings is often considerable.

It is intrinsically difficult to define the extent of this latter problem. Some data may be drawn from a recent edition of the Greek New Testament, which gives, for every variant passage treated, an evaluation of the degree of likelihood in favour of the reading adopted. One of the four letters ABCD is appended to each passage as an index of that likelihood: A signifies that "the text is virtually certain", while D means "that there is a very high degree of doubt concerning the reading selected for the text" (pp.xf). As this edition is intended primarily for translators, this treatment is accorded to only a minority of the variant passages that actually exist, viz those wherein the divergences are meaningful in the task of translation. The numbers of B, C and D instances which I counted up in the Gospels are presented in the following table:

1. The same is apparently true in other fields. Thus D'A.S.Avalle, "Introduzione alla critica del testo", Turin 1970, uses the adjective "adiaforo" of variant readings "fra cui è impossibile decidere con l'aiuto dello stemma oppure in base ai criteri interni..." (p.27; my italics). It emerges from his discussion that in Romance texts such situations are not uncommon - even apparently when one has succeeded in constructing a stemma.


3. as estimated by the four editors, with the participation of A.Voobus during part of the work.
TABLE B.11.4

<table>
<thead>
<tr>
<th>Category of doubtful reading</th>
<th>Matthew</th>
<th>Mark</th>
<th>Luke</th>
<th>John</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>B</td>
<td>77</td>
<td>54</td>
<td>47</td>
<td>53</td>
<td>231</td>
</tr>
<tr>
<td>C</td>
<td>92</td>
<td>59</td>
<td>97</td>
<td>74</td>
<td>322</td>
</tr>
<tr>
<td>D</td>
<td>8</td>
<td>8</td>
<td>26</td>
<td>9</td>
<td>51</td>
</tr>
<tr>
<td>Total (B+C+D)</td>
<td>177</td>
<td>121</td>
<td>170</td>
<td>136</td>
<td>604</td>
</tr>
</tbody>
</table>

| Length of work¹              | 18278   | 11229| 19404| 15420| 64331 |

| Frequency of doubtful readings²| 103     | 93   | 114  | 113  | 107   |

Here we find on average about one passage per one hundred words in which the choice of reading is doubtful. The true frequency is probably higher, in that only a small minority of the variants is presented in this edition. An alternative estimate of one passage in about fifty words may be derived from a statement by E.A. Hutton³. At all events, over a work containing many tens (or even hundreds) of thousands of words, the number of passages where scholars may be expected to differ in their choice of reading will be considerable — even though they will

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2. i.e. the length divided by the total number of doubtful passages. Thus in Matthew we have a doubtful reading once in 103 words, on average.
3. "An Atlas of Textual Criticism", Cambridge 1911, p. x: "...our latest Greek Testament comes out with nearly 3000 passages marked as still uncertain". As Norgenthaler gives the length of the N.T. as 137328 words, we may infer a frequency of one doubtful passage per 50 words (to one sig. fig.). Presumably this estimate is on the high side, in that scholarly activity since 1911 must have removed much uncertainty.
constitute but a tiny proportion of the whole text.

For the Old Testament, such statistics are not available, but my overwhelming impression is that the degree of uncertainty is far greater. The situation is admittedly different in that a great number of variant readings are not furnished by extant Hebrew mss but inferred from ancient Versions in other languages and thence presumed to have been present in the lost Hebrew exemplars from which those versions were made. What matters however is that variant readings exist, between which we are continually called upon to decide; and they have been very differently evaluated by different scholars. Over the Psalter, for example, the commentaries of F.Wutz (1925) and H.Gunkel (1926) diverge greatly in their appraisal of the available readings, and both contrast sharply with a contemporary study such as that of M.Dahood (1970).

Thus the fact that judgments as to the authenticity of readings are involved in the analysis has given rise to two charges: (1) that the results cannot be utilised subsequently for the evaluation of readings - as van Leeuwen concedes; and (2) that an analysis carried out by one scholar would be wholly useless to another who differed considerably from the former in his appraisal of rival readings. The charges themselves have been discussed above; but even if they are upheld, I can nevertheless envisage a possible objection: Is it fair, one may ask, to berate van Leeuwen's method on these grounds, when it
has been shown that both the stemma and the map (as augmented by a "test point") require, for their construction, judgments of authenticity as well? The objection can, I believe, be countered. Regarding (1): the introduction of authenticity judgments in the compilation of a stemma or a map is done in such a way as to allow subsequent decisions between rival readings to be made on the basis of our results legitimately. To turn first to the stemma, the number of passages wherein, in order to achieve orientation, we must decide which variant readings is the oldest, is quite small¹, and - ideally at least - these decisions will represent what has just been termed an A degree of confidence. The stemma, once it has been orientated on the basis of these passages, will be used not in order to confirm our initial judgments but to guide us in other passages². This involves no circularity. In the case of the map, we are not in so strong a position, in that it will often prove necessary to make authenticity judgments in a much greater number of passages in order to locate Ω reliably; nevertheless, there are ways of controlling the circular element (pr. 5:15 f.), and indeed

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1. It can be as small as 2, and need never exceed the number of mss.

2. As Dearing puts it: "A correctly drawn stemma makes clear the allowable generalizations from demonstrably superior readings to readings whose superiority is not otherwise demonstrable" (Novum Testamentum, 1967, p. 295). This same point has already been discussed (Thes., pr. 7:43 f.).
a particular textual tradition may have characteristics that allow us to locate Ω by some independent means - as was demonstrated in our study of the Peshitta Psalter.

In contrast, van Leeuwen's approach involves decisions - some of which may be taken with a D degree of confidence - in every passage throughout the text under analysis, and so van Leeuwen was right to renounce any prospect of applying his result to the subsequent evaluation of variant readings. As for (2): the stemma and the map both stave off the introduction of authenticity judgments until much of the work - and indeed, in terms of computation, the greater part thereof - has been completed, namely the unorientated network, and the map apart from its "best point", respectively. Thus two scholars who disagreed sharply in their evaluation of variants would not need to part company before the stage at which authenticity judgments needed to be brought in; but if instead an analysis like that of van Leeuwen were being attempted, then each would be bound to reject the other's findings in their entirety.

A further criticism which can be made against this procedure is that it will fail to detect many of the

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1. In our location of Ω for that tradition (pp. 7:12 ff.), we never had occasion to decide whether one variant reading was older or better than another. Instead, we asked such questions as which (if any) reading could be identified as lying appreciably closer than its rivals to the Masoretic Text, or to the Septuagint - questions on which one would not expect much disagreement between scholars.
relationships which may exist between the mss and coll hav been re dily identified by other methods (e.g. th t of Fro er). Van Lee wen's ethod limits itself to instances of o e extant ms being descended from another; it does not set out to investigate the possibility of lost common ancestors¹, or "lost point ms" as we have termed them². Yet lost ancestors of this sort have been postulated in virtually every stemma which has ever been drawn up to describe the mss of an actual (as opposed to hypothetical) tradition. A method which is not equipped with any means of detecting such ancestors is hardly fit for general use³.

Complementary to the last point is the charge that van Leeuwen's procedures are likely to suggest relationsh ps which are in fact non-existent. The main danger lies in what seems to be an abuse of the valuable opportunities which the key-word offers of studying specified types of variant. When the study of a particular

1. "Er is een moeder-dochterrelatie tussen de mss. aangenomen. Niet echter wordt, zoals anders in de handschriftkunde gebeurt, gewerkt met hypothetische tussenschakels" (p. 306).

2. Compare a similar criticism against uentin's method (p. 11:18 ff.).

3. Admittedly, one diagram is presented (fl. 19, p. 319) wherein a sin le hypothetical ancestor is postulated (Θ). It is not stated, however, why this was do e just in this c se, or at all, and it is difficult to see how this diagram is related to the matrix output by the computer (fig. 18, p. 318). Incidentally, I find it hard to agree with van Leeuwen's assertion t at this diagram is consonant with the views of Turyn (p. 321).
class yields an additional ancestral relations which did not suggest themselves were the whole collection of variants was considered, it is difficult to see what these newly discovered relationships mean. For example, van Leeuwen carried out an analysis for the class of variant readings which consist of an addition (sc. to the basic text). This analysis (fig. 4, p. 311) reveals 57 ancestral relationships over and above the six obtained from a study of the whole field (see our fig. B.11.17). To take one of these at random, cod. $\emptyset$ (0" in the computer print-out) is now represented as an ancestor of P. What was actually observed, however, was that all the variants which consist of an addition to the text and occur in $\emptyset$ (a total of 3), occur also in P. We are not told whether P contains any of the 17 variants which are found in $\emptyset$ and do not consist of additions to the basic text. To argue that this situation suggested that $\emptyset$ was an ancestor of P rather than a descendant or a collateral - and so on for all these 57 newly postulated relationships - would involve the proposition that a scribe could be expected, in copying his exemplar, to reduce any error consisting of an addition to the text, and to correct away any error which was not an addition. This proposition, which would be involved mutatis mutandis in any study of a particular class of variant, is of course without foundation.

1. Seventeen, because $\emptyset$ is stated to contain 20 variants altogether (p. 310).

2. One can conceive of exceptional cases wherein the proposition may be justified, e.g. the class of variants that do not consist of gross mistakes, or are not solely concerned with orthography.
relations is revealed by such an experiment cannot be accepted as authentic links in the history of the text. To increase the tolerance from zero in such circumstances, as van Leeuwen does in several instances, can only increase the number of "ghost" relationships and thereby compound the confusion.

In conclusion, van Leeuwen's enterprise in applying the computer to the study of mss must be admired, but his procedures and their logical basis are questionable.

1. Van Leeuwen does at em t in one respect to avoid such results (p. 309): if the tolerance is specified to be t, he excludes from consideration any ms which contains less than 2t of the variants of the class concerned. This step however is a mere token which does not guard effectively against the danger.
ON THE PRACTICAL VALUE OF THE METHOD OF DOM J. FROGER

References


Much reference has already been made in this thesis to the method proposed by Dom Jacques Froger for obtaining a stemma, but we have not yet had an opportunity of considering successful applications thereof to real texts. This is because our interest lay, until now, in the theoretical basis of the method (Ch. 1) and in demonstrating its inapplicability under certain conditions (Ch. 2). This seems a convenient point at which to include a review of those studies known to me wherein Froger's method is claimed to have been effective.

Froger's own investigation (1962) deals with a letter which is stated in two extant mss to have been written by Notker, called Balbulus (who died in St Gall in 912), to his colleague Lambert, concerning a series of letters that are found in early manuscripts of plainsong and give directions for the execution of the music. The text is short (under 300 words) but rich in variants (over 100); ten mss are available.
The analysis itself appears to represent a highly successful application of the theory. Out of 99 variants admitted to consideration\(^1\), no less than 89 were found to be stemmatically consistent. One difference between Froger's policy and our own deserves to be noted: he admits to the construction domain any grouping, however infrequent, as long as it is consistent with the other groupings admitted\(^2\) (pp. 54f). Thus he rejects the variant \(\Sigma:BR1-K-D1-L-D2-BR2\) (attested twice\(^3\)) but accepts \(\Sigma:D1\) (found only once). This is not unreasonable; every investigator must be allowed to make up his own mind - while remaining as objective as he can - regarding the admission of low-frequency groupings. From the groupings utilised for construction, Froger obtains a network (p. 54). The character of the text is such as to allow him to identify confidently some of the variant readings as errors (pp. 55 ff.); it was thus possible to orientate the network and arrive at a stemma (p. 62).

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1. See p. 53. The prologue and epilogue were excluded from the construction domain, as were a few passages showing no more than trivial variations of spelling etc.

2. We, on the other hand, having once been forced to reject a grouping of frequency \(x\), would draw the line there and not admit another grouping of frequency less than \(x\).

3. Some of Froger's sigla consist of two or more symbols so I have separated different mss by dashes.
There is one point about which I am uneasy. In the unoriented network (p. 54) SG is at a node from which four arcs branch out:

```
      SG
     /   \
    FL  BE  K etc.  BA etc.
```

but when orientation is performed, SG has moved (p. 62) along the arc leading to FL:

```
      SG
     /   \
    FL  BE  K etc.  BA etc
```

This move seems questionable. Froger justifies it by claiming to have discovered "un nouveau groupe fautif, SG FL" (p. 59). The evidence for this new group is confined to a single passage, at the end of the explanation of the manner in which the letter Z is employed. In the other ms extant, the closing words are "ad alia requirere, in sua lingua [so Greek] zitise" (zitise is a transliteration of ζητίζει - for detailed comments see Froger, p. 56); however SG adds "require" and FL "requirere", which Froger regards as glosses and thus as errors linking these two ms. But this is hardly enough to warrant the displacement of SG, because (i) the word "require" in SG looks as if it may have been added by a later hand (p. 40, n. 2), and (ii) the mysterious "zitise" could easily have been glossed by "requirere" (which occurs a few words earlier) by two scribes independently. Now it is particularly important that the location of SG be placed beyond doubt, because only in SG and FL is the letter attributed to Notker, and so the position of SG within the stemma is crucial to any decision regarding the authorship of the letter.

The stemma which Froger finally adopts is consistent with the known dates of the ms. Froger is able to account plausibly for all the few inconsistent groupings in terms of coincidence in error etc. It turns out that there is no solid evidence that this tradition was ever affected by contamination (p. 63). After a discussion of the textual history - one of his conclusions being that Notker is indeed the author (p. 66) - he reconstructs the text itself. On
the whole (apart from the reservations expressed in the
last paragraph) I am indeed impressed by this
implementation of Froger's theory.

The chronicle studied by Nīță deals with the history
of Romania from 1290 to 1690, and is naturally a far
longer work. In the critical edition from which the data
for this analysis was drawn, it takes up 196 pages.
Apparently the work survives in 48 mss (G-S, pp. xxxiii-lv),
which fall into eleven well-defined groups each of which
is comprised of up to nine mss that so resemble one another
in text that a single siglum suffices on most occasions to
denote the whole group; not all these groups are extant
over the entire chronicle.

Nīță confines himself to four samples, each of ten
pages or less. There are altogether eight mss that are
involved in at least one of those four experiments; their
sigla are given as ABCGKLOV. No one of the four
experiments features more than seven mss.

Stemmatic inconsistency presents a far graver problem
here than in the Notker study. Thus we may consider Nīță's

1. This is what I understand from statements made by
Nīță in the course of his paper, and from my own
perusal of the critical edition itself.

2. or ms groups?
third sample, from pp. 106-115, which yields 100 variant passages; seven mss are there considered, viz ACGKLOV. His table of groupings, relative to V as base, begins:

<table>
<thead>
<tr>
<th>Grouping</th>
<th>Frequency</th>
</tr>
</thead>
<tbody>
<tr>
<td>AK</td>
<td>20</td>
</tr>
<tr>
<td>AGKO</td>
<td>9</td>
</tr>
<tr>
<td>CL</td>
<td>9</td>
</tr>
<tr>
<td>GO</td>
<td>5</td>
</tr>
<tr>
<td>ACGKLO</td>
<td>4</td>
</tr>
<tr>
<td>AKL</td>
<td>4</td>
</tr>
<tr>
<td>ALO</td>
<td>4</td>
</tr>
</tbody>
</table>

We have now reached the noise-level, for AKL and ALO are inconsistent with AGKO and CL. Hence he draws the following network:

![Network Diagram](adapted from Nita, p. 407, fig. 11)  
The dotted curve, which I have added, is explained in the text.

This network rests on the first five groupings in his

---

1. Cases of unique readings have been excluded as a matter of policy (but surely the fifth group in the table may be expressed as $\Sigma:V$?).

2. Nita does not use the term. His policy on this point is (p. 404): "the small-weight groups are left on one side for the moment, and the graph of the linkage is constructed from the large-weight groups only; if some small-weight groups do not present contradictions they can be introduced at a later stage." He does not however tell us explicitly how to set the border between large and small weight.

3. Nita has also tacitly included six "unique" groupings, namely $\Sigma:A$, $\Sigma:K$, etc. (but not $\Sigma:V$; cf n.1), in that he makes ACGKLO all terminal (and does not, for example, represent C as an ancestor of L).
list, which cover altogether 47 variant passages. But there remain 53 more variant passages recorded in the same table, which show groupings each of which is inconsistent with at least one of the five "permitted" types. This is a far less satisfactory state of affairs than we had before.

Let us now consider the networks obtained by Nip from his first and second samples. The first deals with ACKLV only; here the network is more satisfactory in that it accounts for the groupings evinced in 97 variant passages out of 127. But the second sample, involving ACGKLV, yields a network accounting for only 41 passages out of 95. Both networks are consistent with fig. B.11.18 (or, in more technical language, are subgraphs thereof), and to that extent the three confirm one another; however the high proportion of contradictory groupings is disturbing, and raises the spectre of contamination.

1. His fourth sample involves only four mss, denoted A'BGK. I must confess, however, that the meaning of Nip's arguments concerning this experiment eludes me. The reader can judge how lucid they are by trying to read his explanation of the stemma which he derives for these four mss: "K1 is a 'basic manuscript' (the 'relative ancestor'), K4 is the 'non-existent ancestor', and K is a putative manuscript" (p. 409). What is one to understand by such a term as 'non-existent ancestor', which is not mentioned, let alone defined, anywhere else in the paper? As this fourth sample is not brought up before the last pages of Nip's paper, and his basic arguments are complete without it, I think it best that we limit ourselves to the first three.
If we grant that the network of fig. B.11.18 is sound, our next task is to orientate it. This can be done by referring to the prolegomena of the critical edition, where Grecescu and Simonescu have themselves set up a stemma, which is shown in fig. B.11.19 - (a) in full, as it appears in G-S, p. lviit, (b) in a reduced form confined to the seven ms groups considered in our last figure.

Note: The superscript figures in the bottom row give the number of ms in each group. The dotted curve, which I have added, is explained in the text. Only the ms in the bottom row are extant.

If we accept the orientation of this stemma, then the network of fig. B.11.18 must be "picked up" at the point indicated by the arrow. This will yield fig. B.11.20
which is almost identical with fig. B.11.19(b), apart from one difference: Nita's stemma postulates that CL have a common ancestor not shared by V. On this point Nita seems to be right, to judge from his tables, for, over the three, Σ:CL is the second most frequent grouping.

This point merits some discussion, though a lengthy excursus would lie well beyond my own competence and the scope of this Thesis. Apparently G-S arrived at their stemma not by a consideration of common errors but by the use of various ad hoc arguments, mainly concerned with the differing amounts of historical material to be found in the different mss.

The mss are initially divided into two great families, on the grounds that in the former (CLUV) the record of the history stops a few months before the death of Serban Cantacuzino and is not resumed until 10-20 years later, whereas the latter (AKBSTGO) has no such gap and gives a continuous record up to and beyond his death, as far as January 1690. G-S believe that the content of the latter set of mss corresponds to that of the original, while the former set present an abbreviated version, to which another work has been appended (p. lvii).

Within these two families, the policy of G-S is apparently to presume that all ms groups are independent of one another unless an argument is brought to the contrary. No such argument is brought concerning the group CLUV; but within the other group, A is affiliated with K because of their striking similarity in text, and G with O because they both omit any reference to the

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1. I am grateful to Miss Laura Gurdikyan for her help in reading the Romanian text.
2. "A, foarte apropiată de K" (p. lvii).
life of the patriarch Nifon, while the remaining members BST are classified together for no better reason, apparently, than that they have no such striking peculiarities.

It would now seem that Nija is right in asserting that CL have a common ancestor not shared by V, in that the contrary view of G-S, that CLV represent three independent lines of descent, rests on what is no more than an arbitrary pre-supposition.

There is a further point, regarding the orientation fixed by G-S. If the group AKBSTGO corresponds more or less to the original, in respect of the extent of its material, then the archetype need not necessarily be located at X in fig. B.11.IQ (a) but anywhere within the closed curve there shown. In terms of fig. B.11.IQ too, the "pick-up" point does not have to be as indicated by the arrow, and may lie elsewhere within the curve.

Despite my unfamiliarity with this field, my impression is that Nija was right to believe that there was room for an analysis of this data based on Froger's method. In his hands this approach has advanced our knowledge, and further application of Froger's method to this text is likely to achieve still more. Such opportunities may however be limited by the relatively serious effects of contamination on this tradition.

1. "La subgroupele B5, S6, T2 nu e ceva deosebit de remarcat" (loc. cit.)

2. In favour of the view of G-S it could perhaps be urged that some of the readings in which CL agree represent an effort to modernise the archaic language of the chronicle, e.g.

   p.5, 1.10 pofteste CL, pofteste rell.

   such as could have been made by any number of scribes independently. There are however many more agreements between CL against the other mss that can hardly be explained as being due to mere coincidence, e.g.

   p.9, 1.16 pismă neschimbată CL, pismă (without any addition) rell.
Our overall impression of both investigations must be favourable. Each leads to results that are generally consistent with those of earlier scholars; and at the same time each can be said to have broken some new ground. However, I doubt whether Froger's method will take us far in unravelling any but the simplest of the biblical, classical and patristic traditions that occupy the scholars of today. We have seen the method applied successfully to collections of no more than ten mss - or, if we include our own study based on the Canivet-Malvaux data, no more than fifteen. The case is very different when there are many more mss; for if the text in question aroused such interest that a great number of copies now survive (say 50 or 100, which are yet a small remnant of those that ever existed), then it seems all but inevitable that it also aroused such interest as to make many scribes anxious to compare several exemplars and to note variant readings in the margins. We have already seen (pp. 2:40 ff.) how Froger's method broke down when applied to the traditions of Cyprian's De Unitate and Aeschylus' Persae, each of which survives in more than 100 mss; I fear that the same will occur virtually whenever one attempts to use the method in order to classify a rich tradition. Indeed I would go so far as to say that in most cases wherein the number of extant mss exceeds say thirty, contamination is likely to have progressed so far that a stemma would be too much of an over-simplification to be useful. Speaking as one who has devoted a whole section of a thesis to the properties of stemmata (Sect. A), I
would not be at all sorry to be proved wrong, but I feel convinced that the role of the stemma is seriously limited. Nevertheless, in those traditions wherein the construction of a stemma is admitted and Froger's method is effective, it is the best method available.
OTHER METHODS FOR CONSTRUCTION OF TREES,
DUE TO P. BUNEMAN AND TO OTHER INVESTIGATORS

Reference

P. Buneman, "The recovery of trees from measures of dissimilarity", in MAHS¹, pp. 387-395.

In this section let us examine some further approaches to the problem of expressing ms relations in the form of a stemma, by numerical means.

The article by Peter Buneman presents a method for deriving a tree (without fixing its orientation) from a table of dissimilarity measures between ms pairs (for which ms distances will serve). The rationale underlying the method is set out in mathematical language which will seem horrendous to most textual critics but does provide a rigorous logical background. Regrettably, the article does not tell us of any attempt to apply this method to a real (or even a model) text, but it is nonetheless of considerable interest.

¹. i.e. Mathematics in the Archaeological and Historical Sciences; for details, see p. 11:34.
Since I am convinced that Buneman's paper, which is
couched in technical terms, is unlikely to mean much to
the average philologist, I feel that my first task is to
explain the mechanism of his procedures as simply as I
can. What we must do basically\(^1\) is to identify certain
two-way splits which I shall call "permitted". Consider
any two-way split; it will divide the collection of
extant mss into two mutually exclusive sets\(^2\). Let us
choose two mss from one of the groups; they may be
labelled \(\alpha\) and \(\beta\). Similarly let us choose two mss from
the other group, labelling them \(\gamma\) and \(\delta\). There will
usually be many different ways of making these choices.
Let us now denote the distance between any two mss XY by
d(X,Y). Then the split which we are now considering will
be "permitted" if, and only if, the sum \(d(\alpha,\gamma)+d(\beta,\delta)\) is
greater than \(d(\alpha,\beta)+d(\gamma,\delta)\), no matter how we choose
\(\alpha,\beta,\gamma\) and \(\delta\). Buneman has shewn that all splits that
are identified as "permitted" in this way will be mutually
consistent. It should therefore be possible to derive
a tree.

These tests can be conducted by hand for a small
number of mss. For example, let us consider once more
the stemma and distance table which we discussed on p. 3:7:

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1. The following exposition rests on the formula for \(\mu_r\),
at the foot of p. 390.
2. From what follows it appears that we are concerned only
with splits that yield two groups of which each contains
at least two mss.
There we observed that one could not have deduced the network from a cursory inspection of the table. But by Buneman's method the network could be recovered. For the split AB:CD is "permitted"; whether we choose $\kappa = B$, $\beta = A$, $\gamma = C$, $\delta = D$, or any other possibility, $d(\kappa, \gamma) + d(\beta, \delta)$ will exceed $d(\kappa, \beta) + d(\gamma, \delta)$. This is not true, however, of the splits AC:BD or AD:BC. Thus we could reason from the table back to the network (a) - though we would not know the true orientation. With a large number of mss, computer assistance would be needed; Buneman tells us (p. 393) that there is a "reasonably fast" program which does not involve a search through all the possible splits, and he hopes to publish details thereof in the near future.

Let us therefore try to ascertain more specifically with what degree of contamination Buneman's method should ideally be applied. When contamination has been so slight that Froger's method gives a satisfactory tree, then it would be unduly circuitous for us instead to calculate and
process manuscript distances\(^1\). At the opposite extreme, however, when contamination has been exceedingly serious, application of the method could be objected to on other grounds; for a stemma represents each ms, whether extant or not, as a copy of a single exemplar, whereas many of the mss concerned were by hypothesis derived from two or more exemplars. To force a heavily contaminated tradition into a stemmatic mould could involve considerable distortion, and we may well ask how meaningful the resulting network would be.

This last point deserves to be amplified by an example. Let us suppose that of the 22 mss of Quentin's model tradition (p. 5:3) there survived only CDRS. The source-complex of these four mss consists of the set ACDEFGLMNOPQRS, whose inter-relations are shown in fig. B.11.22a. With the loss of all mss except CDRS, this diagram can be simplified to some extent (b), but is far from being a stemma. What would happen if we were to apply Buneman's method to these four mss? On the basis of a table of the absolute distances between them (c), we find the split CD:RS to be "permitted", as the reader can verify, and thus we arrive at the network (d).

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1. as Buneman himself admits (p. 394): "Computing a DC [dissimilarity coefficient] is not the only way open to us for finding a tree. For protein chains one can avoid a DC and define a set of splits in terms of the amino-acid sequences themselves; and these splits turn out to be compatible. The same thing can be done with manuscripts... It is not surprising that by avoiding a DC, one can build trees that give much better descriptions of the data".
Can this network be treated in the usual fashion? To be specific, can it be orientated and then used to choose between rival readings? Certainly the orientation is far from straightforward, in that we have some passages such as aeque et/atque (1. 3), prius/prior (1. 5), where C is the only ms of the four to preserve the true reading, and others at the same time such as venerant/venerunt (1.7) and metuens/dicens (1. 12), where D alone is sound. It turns out that any orientation of this network will yield a stemma that suggests an incorrect choice of reading on an unacceptably high number of occasions.

1. The orientation that offends the least in this respect gives

which would lead us astray on three occasions, where D alone has the true reading (see lines 7,12,13).
But between these two extremes of contamination level, there may well be a range over which Buneman's method is indeed applicable. We must in fact ask: Can there exist a situation in which the malady of contamination has progressed far enough to cause Froger's method to break down, but not far enough for a stemmatic model to be more misleading than helpful? The question cannot be answered a priori; what is required is a series of experiments in which Buneman's method is applied to a variety of textual traditions. The method would prove to be of enormous value if it could be shewn capable of providing a serviceable tree in cases of moderate contamination\(^1\), where Froger's method yields either no tree at all or a tree that is not wholly certain or satisfactory. I very much hope that such experiments will be carried out, and await the results with keen interest.

The problem of constructing a historical tree is not confined to textual criticism; similar questions crop up not only in biology but also in other disciplines (e.g. in medicine, regarding the relationships between protein sequences).

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1. This term itself suggests a desideratum in text-critical study, namely a numerical index of the level of contamination. Although scholars use terms like "heavy contamination", "virgin tradition", and so on, no precise (i.e. quantitative) gradation has yet been devised (though our remarks on p. 244n are relevant). If such an index could be readily calculated for any given tradition, and if moreover experience were to show that traditions which gave an index lying within a particular range were amenable to Buneman's method, this would offer the investigator some valuable guidance.
and procedures - most of which are so elaborate as to have been worked up into computer programs - for producing trees are continually being devised by workers in such fields. It is natural to seek to apply such programs to the study of manuscripts; but before doing so, the investigator must be satisfied that the program does not depend on any assumptions which, although they may be valid in the particular context for which that program was originally intended, can no longer be justified when the original concepts are replaced by their counterparts in textual criticism. The applicability of every such program to our own problem must be judged on its performance in an adequate series of experiments on real textual traditions; wherefore, as I am not the first to observe, by their fruits ye shall know them.

1. e.g. on rates of production and decease, the likelihood of one individual developing from two or more immediate ancestors, the likelihood of the same trait developing independently in two individuals, and so on.

2. I understand that the Rev. Mr A. Q. Morton claims to have traced the history of certain mss of Aristophanes on the basis of a procedure that has been employed to provide trees depicting biological evolution, namely R. L. Bartcher's "Fortran IV program for estimation of cladistic relationships using the IBM 7040", published as Computer Contribution 6, State Geological Survey, University of Kansas, Lawrence 1966.
THE ORIENTATION THEORY OF DR. J. HAIGH

References

"The manuscript linkage problem", in MAHS\(^1\), pp. 396-400.

The contribution of John Haigh deals not with construction but with the orientation of a network that is already known. In other words, his method sets out to select the right point from which to "pick up" a given network.

In an abstract\(^2\) of his paper (1971), Haigh states the problem thus: "A population which develops as a linear birth process from one individual gives rise to a family tree ... which shows the relationships of the ... members to each other. If this tree is given, but with no a priori information of which point is the root (the original member) we seek to utilise our knowledge of the ... manner in which the tree grew to estimate the root".

This entails a model of the history of a ms tradition as a pure birth process, in which no members are lost. On this assumption, every ms that has ever existed of the text concerned, including the original itself, is extant — or,

\(^1\) i.e. Mathematics in the Archaeological and Historical Sciences; for details, see p. 11:84.
\(^2\) I am grateful to Dr F.R. Hodson for letting me have a copy of this abstract. The paper was originally read before the Anglo-Romanian Conference, Mamaia 1970.
at the very least, has been assigned a definite location in the tree - and we are now called upon to identify the original.

Before commenting on this view of ms propagation, I should like to explain the mechanism of the method. We begin with an arbitrary orientation; suppose for example that we are given the tree of fig. B.11.23a, with an assurance that A...G are the only mss that have ever existed of the work concerned. With each ms we associate

![Diagram](image)

(a) (b)

Fig. B.11.23

a number, viz the number of mss below it; for this purpose a ms is counted as being below itself. (A glance at the appended figures in fig. B.11.23a will dispel obscurity.) Identify the smallest number thus obtained which is not less than \( \frac{1}{2}m \), where \( m \) = no. of mss; the point with which this number is associated will be the likeliest site for the original (the "maximum likelihood choice as root"), according to Haigh's background theory\(^1\). In our

1. Occasionally we may find that there are two adjacent points, both associated with that number; these two are then equally likely sites.
example the required number is 5; this points to E as the original member (fig. B.11.23b). The effect of this procedure, Haigh explains, is to locate the original at or near the "centre" of the tree.

This holds out the prospect of orientation without any need to come to prior decisions on the rightness and wrongness of variant readings. But does Haigh's model provide an acceptable approximation to reality? The effects of ms "death" surely cannot be simply ignored.

In most cases the textual critic is all too aware that a great number of mss have perished. Indeed our work in Sect. A agrees with the conviction of many scholars¹ that the extant mss form only a very small remnant of those that have ever existed. In particular, the original itself has almost certainly perished; we shall usually have to think, when we try to orientate, of an archetype instead, which is not the same thing at all. Thus we have to question the relevance of Haigh's theory to the study of manuscripts.

1. e.g. M.Bévenot, "St Cyprian's De Unitate...", London 1938, pp. 1 ff: "Everyone is aware of the ravages of vandalism which in one form or another, at various times destroyed so many priceless libraries in monastery and cathedral, in palace and in university... It is only too clear... that what we now have is only a remnant of all the copies which century by century were made of his [Cyprian's] works... There are some 150 in existence today only because there were many times that number in former ages".
Yet in his paper (1971), Haigh did attempt to classify the mss of three textual traditions, named as Caedmon's Hymn, Bede's Death Song and Cicero's Letters to Atticus. As an example, let us consider his treatment of the third of these. He starts out from the stemma proposed by D.R. Shackelton Bailey (see fig. B.11.24, to which the requisite numbers have been attached), and concludes (p. 399) that the likeliest site for the archetype is $\Phi$. He reasons similarly concerning the other two traditions. But I fear that such an argument involves a fundamental misunderstanding of what a stemma really means. It has never been suggested that the seventeen mss, real and inferred, which appear in the figure, are all that ever existed. Every arc may contain an indefinite number of arc mss; there may moreover have been any number of traceless mss. A

2. For an explanation of these terms, see p. 1:12.
3. As a matter of fact, in this case we have also a considerable number of other extant mss; this particular stemma, as Shackelton Bailey makes quite clear, refers only to the $\Sigma$ family of mss of the Letters.
procedure which allows us to identify the original member in a population resulting from a pure birth process, cannot be pressed into service without further ado in order to identify the archetype in a population resulting from a birth-and-death process in which the death component was far from negligible.

What of the results themselves? In none of the three experiments was the estimated root more than two arcs distant from the "true" site. Haigh says that all these locations are "not too far removed from the agreed root" (p. 400), but this seems unduly optimistic. A discrepancy of that order, though it may seem slight on a diagram, can lead to a substantially different policy in the choice between rival readings. It is sufficient to recall the controversy raised by Bédier; much of the debate (pp. 2:53 ff) centred on the choice between four different "pick-up" points (indicated with arrows in fig. B.11.25) for a network which itself was not in question. All four are quite close, in terms of arcs, but they would have very different consequences for

![Diagram](image)

the text, as Bédier shewed. Again, if we were to decide to use Haigh's method in order to check our Theodoret stemma (fig. B.11.16), we would find that it did not confirm
our choice of "pick-up" point, and led to correspondingly different conclusions.

We conclude then that the application of Dr Haigh's theory to textual criticism does not commend itself either by its rationale or by its results. This does not of course detract from the interest of his work as a valuable contribution to the theory of stochastic processes, one which may well be applied with advantage to problems in other spheres.

1. Instead of our two-branched

Haigh's method points to either of two three-branched trees:

either of which - particularly the second - would considerably diminish the status of those attractive readings that are attested within the FCHP group alone.
Preliminary Note

In this part we shall discuss numerical treatments which do not have as their object the construction of a stemma. There have been a great number of studies of complex traditions in which statistics of one sort or another have been drawn up and appealed to more or less intuitively. For example, we sometimes find that the investigator has counted up the number of times that the mss divide themselves in a particular manner; sometimes we are told instead how often a particular ms agrees with various others, and we may even be offered an extensive table of distances or similarities. In most cases however the argument from such figures is more or less straightforward, and of little mathematical interest beyond the bare fact that statistics have been compiled. There seems to be little point in my compiling a catalogue of such studies; I shall rather be concerned with treatments that go somewhat further in their mathematical content.

1. such as F.H. Scrivener, "A plain introduction to the criticism of the New Testament", Cambridge 1874, p. 146 (on character of Cod. Zacynthius, denoted Ζ); or as recently as P.B. Dirksen's work on the Peshitta to Judges (1972; see Thes., p. 7:71).

2. Instances can be found as early as Quentin's Mémoire; recent examples appear in Colwell, pp. 56 ff. (see References below: this article was first published in 1963), and in the Isaiah volume of the great Rome Vulgate (1969; see p. xxx).
METHODS OF THE "MAPPING" TYPE

References

E.C.Colwell, "Studies in Methodology in Textual Criticism of
the New Testament" [= New Testament Tools and Studies IX],
Leyden 1969 (a revised collection of essays which had
been published by the author at various times). See
in particular Ch. 2.
See also Dom J. Froger, op. cit., pp. 132 ff.

The earliest study which I feel deserves to be mentioned
here is Hutton's "Atlas" (1911). Starting out from the
time-honoured division of N.T. mss into three groups
(Alexandrine, Western, Syrian), he identified passages where
the mss offered "triple readings", i.e. three alternative
readings of which each appeared in one of the three groups
so consistently that it could be regarded as the characteristic
reading of the respective group. He would then proceed
to characterise a given ms by considering its behaviour in
relation to these three great families over his collection
of passages where "triple readings" were available.

What I find significant here is not so much the method
itself but the concepts which emerge from it. Hutton's
study can be thought of as an attempt to "place" a given ms
with reference to three pre-determined "fixed points", viz

1. It was proposed by Griesbach - though it is being
   abandoned today (Colwell, p. 27).
2. For an appraisal of this method, see Metzger, pp. 180 f.
t e "pure" forms of the t ree types of text; and t e title "Atlas", I believe, confirms t is view. Thus althou h Hutton does not ma e any attempt to draw a map to illustrate the relations between the N.T. mss, the concept of a map seems to have been latent in his work.

E.C. Colwell, also writing on the N.T., doubted the validity of the tripartite division on which Hutton's approach depends, and proposed that consideration be extended to "multiple readings", i.e. places where the minimum support for each of at least three variant forms of the text is a well-defined entity such as a previously established group (like Fam. 1), or an ancient version. Thus he too advocates that a given m s be "located" (his term), but sets up a rather greater number of "fixed points". Some of the terms in w ich he writes reinforce the impression that he thought in "mapping" terms: "We need a compass, a pathfinder, to guide us through the forests to the particular clump of trees to which our manuscript is closely related" (p. 26). Colwell may therefore be regarded as another who prepared the ground for the representation of a textual tradition in the form of a map.

Froger's book is ainly devoted to traditions which admit the formulation of a stem a; but in cases wherein contamination has made this impossible, he su ests (pp. 132ff.)

1. For an appraisal of this method, see Metzger, pp. 180 f.
that we compile what amounts to a two-dimensional map. This map, like those which we ourselves have drawn, was to be based on a table of "distances". Unlike us, however, he proposed that the map be derived from the distance table by free-hand drawing, which he thought would suffice to yield a map which reflected satisfactorily the proximity relations between the mss. In order to illustrate his procedure, I reproduce in Fig. B.11.26 the hypothetical example which he treats. The resulting figure may prove

<table>
<thead>
<tr>
<th>A</th>
<th>B</th>
<th>C</th>
<th>D</th>
<th>E</th>
<th>F</th>
<th>G</th>
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<tr>
<td>10</td>
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</table>

2. AG
3. BF
4. BD, DF, EG
5. DE, DG, FG
6. AE, EF
7. AD, AF, BE, BG
8. AB

**Explanations**

- **We have six mss ABDEFG which resist presentation within a stemma.** A table of the distances between them is given in (a). The range of values which these distances cover is then divided, in a convenient and more or less arbitrary fashion, into a small number of intervals (in this case, six); all distances falling within one and the same interval are treated alike (b).

- **We now consider these intervals in turn, and on that basis draw a map (c).** A pair falling within the first interval has been placed within a small area surrounded by a continuous line; a pair yielding a distance which does not surpass the second interval has been similarly enclosed by a dotted line; this leaves only two mss (DE), which have been located free-hand (and, as far as I can see, pretty well arbitrarily). In (c) I have presented the map just as Froger drew it.

**Fig. B. 11. 26**

useful, Froger tells us, in two respects. First, we may compare it with a geographical map on which each ms is located according to its place of origin (if known); this
con arison may tell us something about cultural relations between the localities represented. Second, if any particularly well-lit clusters emerge, we may simplify our _apparatus criticus_ by confining ourselves to one sole representative of every such cluster. Froger does not suggest that the map in itself could guide us in formulating a textual history or in discriminating between rival readings.

The principal objection to Froger's proposal is that only in the simplest of cases can a satisfactory map be drawn free-hand. We have seen in Ch. 4 that a map in a small number of dimensions cannot be drawn to yield exactly the distances actually observed between the mss, and that it is therefore necessary to compile a map which will reflect the data as well as can be achieved. The elaborate network of compromises which this entails is simply too complex to be _adequately_ treated by intuitive means. Froger would no doubt have discovered this for himself if he had applied his proposals to the mss of a real text.
The Theory of "Disconnexions", due to M. Bévenot

References

See especially pp. 133-135, 148-150.

The views of Professor Maurice Bévenot, S.J., have been referred to more than once in the course of this Thesis, and it will already be apparent to the reader that they have profoundly influenced my own thinking. However, we have not yet had an opportunity of considering his treatment in detail.

To my mind, Bévenot has introduced an original view of the concept of historical connection between witnesses to a text. Let us consider a pair of mss. The two may be intimately connected, e.g. if they are copies of the same late ms and have thus had a lengthy common ancestry; at the other extreme, the connection may be slight, e.g. if the respective source-complexes¹ of the two mss have but little in common, or (in more readily understood, albeit question-begging, terms) if they belong to different branches of the tradition.

1. The source-complex of a ms M is defined to be the set of all mss from which M is derived, including M itself (p. 2:6).
Connection would be at its lowest conceivable level if the only ms common to the two source-complexes were the original itself. We are unlikely to encounter such a case in practice; nevertheless, different ms pairs will vary greatly in the degree of connection which exists between them. It is of particular interest to identify the pairs which are least connected, in that the comparative independence of their histories will favour the supposition that, where they agree, they are reproducing the reading of a common source which is remote, if not the original itself.

Bévenot applied these ideas to the eighteen mss already discussed (pp. 6: ff.) of Cyprian's De Unitate, and, by means of procedures which we shall consider shortly, he estimated the degree of connection between each pair. He identified several pairs which were but slightly connected, and termed them "opposed". Moreover, it proved possible to identify some groups of three mss, each of which was opposed to the other two ("triple oppositions"); a reading attested by three mutually opposed mss, he urged, was likely to be original (TM, p. 148). Going back to the mss of the De Unitate in the light of these triple oppositions, he was gratified to find that in almost every variant passage the reading he had initially chosen for the resultant text was confirmed.

Later he noticed that the three members of one of these triads were each opposed to a fourth ms. He examined the behaviour of the "team" consisting of these four mutually opposed mss over the text of another treatise of Cyprian,
the De Lapsis, and, to provide a valid basis for comparison, he then collated seven more mss of that work. He reported: "I found, in general, that the joint evidence of the four was never upset but generally confirmed by the readings of the other seven MSS used, and that the agreement of any three of the four almost invariably carried the day" (PCEC, p.7).

To the theory of "disconnexions" as expounded so far my own work is greatly indebted, in that the method proposed in this Thesis also involves a search for a small team of mss which combine purity and mutual independence to the greatest extent that the material allows. Where I part company with Bévenot, however, is in the procedure for identifying the team.

Bévenot began by compiling, on the basis of the witnesses at his disposal, a text intended to approximate as closely as possible to the original; this he called the resultant text. He then counted, for every possible ms pair, how many times the two mss agreed in a reading which was not the one adopted for his resultant text. (There were some exceptions to this rule, but they do not affect the argument 1.) The number of times that two mss agreed in departing from the resultant text was then taken as an index of the degree of connection between them, and on that basis the "oppositions" were identified 2.

1. See TN, pp. 124 f.
2. Hence our own use of the term "connection measure" to denote the number of times two mss agree in error (p.3:4).
Thus Bévenot's statistics were drawn up after the selection of readings for the resultant text had been made, and to some extent they depend on those earlier judgments. We encountered such a situation in our discussion of the work of van Leeuwen (pp. 11:75ff), and the points which were raised then apply with equal force here – namely (1) that to utilise these statistics for the subsequent evaluation of rival readings within the same work would entail a circular argument, and (2) that an analysis carried out by one scholar would be wholly devoid of value to another who differed to any great extent from the former in his initial choice of readings for the resultant text. Yet Bévenot employed the oppositions which he deduced from the text of the De Unitate, for two purposes: (a) to re-evaluate variant readings within the De Unitate itself, (b) to identify a team of mutually opposed mss whereby the text of the De Lapsis might be determined. To (b) we shall come presently; but, in view of our arguments above, (a) appears to be a somewhat dangerous procedure.

To raise this point might seem hypercritical, in view of the virtually total confirmation which the triple oppositions present for Bévenot's resultant text. It seems to me, however, that the circularity which enters into that confirmation seriously lessens its value, as will be apparent from the following hypothetical example.

Let us consider once more the model text discussed by Dom Quentin. Suppose that (to depart from Quentin's own apparatus) we have only six mss ABCDEF, and fifteen places
1. Anastasia primo diram et immitem custodiam a viro suo Publio passa'est,
2 in qua tamen a Chrysogono, confessore Christi, multus consolata et confortata
3. est. Deinde a praefecto Illyrici in gravissima aeque et diutina custodia macerata
4 est: in qua duobus mensibus restecta est cælestibus escis per sanctam
5 Theodoten, quae prius martyrium passa est. Deinde navis imposita cum ducentis
6. viris et septingentis feminis, ut demergentur in mari, perleta est ad insulas Palmarias
7 ubi martyrium consummavit et omnes qui cum illa venerant
8. variis interfectionibus martyrium celebrarunt
9 Inter quos omnes, unus erat nomine Eutychianus,
10 innocentissimae naturae, qui sublatis sibi, cum dives
11 est, omnibus facultatibus, tacuit, nihil cogitans nihilque
12 metuens, nisi hoc, ne facultates ac divitiæ sive
13 perderet. Quotiescumque denique fuisset auditus, quotiescumque
14 interrogatus nihil aliud dicerat: Christum
15 mihi non tollet etiam qui caput abstulerit

THE APPARATUS CRITICUS

1. diram AB; duram CDEF
2 et BCEF; atque AD
3 cœnde CDF; dein ABE
4 sanctam BCE; beatam ADF
5 ducentis CDEF; trecentis AB
6 mari AF; mare BCDE
7 illa DF; ea ABCE
8 martyrium ABCE; martyria DF
9. nomine DE; ouf nomen ABCF
10 dives DEF; locuples ABC
11. tacuit AEF; siluit BCD
12 metuens ABCP; timens DE
13 cenique EF; enim ABCD
14. Christum ADDF; Iesus CE
15. tollet ACFE; auferet BE
where the mss diverge (one in each line). The text of the whole passage, together with the apparatus, is given on p. 11:117. It will be observed that the variations are intended to be "adiafori" (p. 11:76, n.1).

Suppose now that a scholar X chooses the following readings for his initial resultant text:


The following list gives, for every ms, the number of times that it agrees with each of the others in departing from X's resultant text:

A: B^5 F^5 C^3 D^2 E^1
B: A^5 C^3 F^3 D^0 E^0
C: A^3 B^3 F^3 D^1 E^1
D: F^4 A^2 C^1 B^0 E^0
E: F^2 A^1 C^1 B^0 D^0
F: A^5 D^4 B^3 C^3 E^2

The most opposed ms pairs (with connection index zero) are BD, BE, DE, and they yield a "maximal" triple opposition BDE. Suppose now that X goes back to the mss in order to check the resultant text which he first formulated. He will see that every one of the fifteen readings which he adopted is supported by the agreement of at least two of these three mutually opposed mss, and that not one of the rejected readings is attested by more than one member of the triad at a time; and so he will be satisfied that his initial selec-
dition of readings was wholly correct.

Consider however a second scholar Y who makes a rather different selection for his resultant text, which agrees in only eight of the fifteen variant passages with that of X:

\[
duram - et - deinde - beatam - ducentis - mari -
eam - martyrdom - cui nomen - locuples - tacuit -
metuens - enim - Christum - tolerat
\]

His table of "connection" totals will run:

A: B³ D¹ E¹ C⁰ F⁰
B: A³ C³ E³ D² F¹
C: B³ E³ D² F¹ A⁰
D: E⁴ E³ B² C² A¹
E: E⁴ B³ C³ F³ A¹
F: D³ E² B¹ C¹ A⁰

The ms pairs that are maximally opposed are now AC, AF, CF, whence the triple opposition ACF. Here again, all the readings originally chosen by Y, and they alone, are shared by at least two members of the triad, and so the resultant text which Y first compiled appears to be completely endorsed by the analysis.

Nor does the matter end here. We may further suppose a scholar Z who opts for the following readings:
yielding the following measures of connection:

A: $B^9 C^6 E^3 F^3 D^2$
B: $A^9 C^8 E^4 F^3 D^2$
C: $B^8 A^6 E^4 F^3 D^2$
D: $A^2 B^2 C^2 E^0 F^0$
E: $B^4 C^4 A^3 D^0 F^0$
F: $A^3 B^3 C^3 D^0 F^0$

and once more three mutually opposed pairs (DE, DF, EF), whence the one triad DEF. Like X and Y, Z too will find that his own resultant text is precisely that to which the agreements of these opposed mss would point.

Thus we can conceive of three scholars, each of whom makes a different selection from the variant readings available, there being only five variant passages (out of fifteen) in which all three agree; and each scholar, having compiled the tables and examined the oppositions which emerge, will find total confirmation of his own original text. Both the charges made above (p. 11:116) are thereby substantiated. The former, namely circularity, is clearly borne out by the facts; the evidence of ms

1. viz duram - et- ducentis - Chrístum - tolet.
oppositions tends merely to perpetuate the selection of readings with which one started out\(^1\), whether that selection was judicious, indifferent or even random. The force of the second charge, that two scholars who differed considerably in their initial choice of readings would each find the other's analysis utterly worthless, emerges no less cogently. Altogether, the apparent confirmation which oppositions present of one's original selection of readings, should not be taken too seriously.

We now come to Bévenot's second application of the oppositions which he obtained over the De Unitate, viz to determine the text of the De Lapsis. This is innocent of the former charge but not of the latter: the application of the "team" to the mss of the De Lapsis depends ultimately on the assumption that the resultant text first selected by Bévenot for the De Unitate was largely correct. In this particular case, that assumption is surely justified (even though the confirmation which this text of the De Unitate was thought to have received is illusory), but it is only too easy to envisage a situation wherein such confidence in an investigator's initial selection of readings would be unwarranted. It is also worth observing that this approach to the mss of the De Lapsis introduces a new assumption, viz that ms relations which hold over the De Unitate will

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\(^1\) This is without doubt the tendency, but to assert that any selection of readings for the text of any work will be confirmed in its entirety, would be to overstep the mark.
also hold over the other treatises; our results concerning the instability of ms relationships (pp. 3:22r) suggest caution in making a supposition of this sort. For all that, however, one can hardly fault the text of the De Lapsis at which Bévenot arrived.

To sum up: Although I accept Bévenot's concept of historic 1 connection whole-he rtedly, I do not feel that his procedure yields reliable estimates - i.e. estimates on which the textual critic can legitimately base the sort of inferences which he will wish to draw - of the respective degrees of connection which actually exist between the mss. My own work on the map represents in part an attem t to put the study of connection on a more secure footing. This does not of course detract from the usefulness of Bévenot's basic theory, which I regard as a most valu ble contribution to textual criticism.
SERIATION AND THE WORK OF J.G. GRIFFITH

References


All three articles are by J.G. Griffith; the first is an advance notice of the second.

I have to disclose a personal interest here. Mr. Griffith is writing a book entitled "Numerical Taxonomy and Textual Criticism", in collaboration with me. This has not, I hope, prevented me from an objective appraisal of these articles. Since their appearance, I have seen a great deal of his as yet unpublished work, on which it is of course not my part to comment here.

We now come to another way to describe a collection of mss, namely to represent them in the form of a spectrum. This approach has been adapted for use in textual criticism by John G. Griffith.

Constructing a spectrum means that we arrange our mss in an order such that, as far as possible, the more similar two mss are to each other in text, the nearer they will be to each other in the spectrum. This may be stated in numerical terms, if we are given the distance (absolute or percentage) between every pair of mss. These figures can be arranged in the form of a square table, with all the mss

1. In his articles, Griffith has used a measure of similarity, namely the number of times two mss agree over a given field of variants; this is, as we have said (p. 11:5, n.3), a simple function of absolute distance.
listed (in any order we choose) along the top, and again (in the same order) down the left-hand side (as in fig. B.11.27A); and we shall try to list the mss in such an order that, as far as possible, within each row of the table, the distance figures increase as one moves - in either direction - away from the main diagonal. This process is sometimes called "resolving" the table of distances. The primary feature of the spectrum, then, is the creation of a "near-neighbour" sequence.

Once this has been achieved, a second feature may be introduced: we shall probably find that certain mss adhere to one another particularly closely, and that a number of "clusters" can be distinguished. In its final form, therefore, Griffith's spectrum will consist of a near-neighbour sequence on which cluster-divisions have been superimposed, such as:

VBH, TZLGK, FNO, JU, A, PR.

This procedure is an example of seriation, i.e. the derivation of a "near-neighbour" ordering from a table of measures of similarity or dissimilarity. Seriation as a numerical technique was invented, I believe, by W.S. Robinson:

1. TSJ, p. 124.

who was concerned to shew that a "near-neighbour" sequence of archaeological deposits could also serve as a chronological sequence. It has moreover been widely used (often in conjunction with cluster-division) and has established itself in the field of biology and kindred disciplines; in such contexts it is usually thought of under the heading of "numerical taxonomy". Before the work of Griffith, however, it had never been applied to the study of mss.

1. Some remarks in earlier writers show a certain kinship with Griffith's ideas. Thus B.H.Streeter, "The Four Gospels: A Study of Origins", London 1924, p. 106, observes: "The remarkable thing is that the texts we have examined form, as it were, a graded series. Each member of the series has many readings peculiar to itself, but each is related to its next-door neighbour far more closely than to remoter members of the series". Streeter, however, did not try to obtain his series from a numerical table. Again, in some other numerical studies of textual criticism, the investigator has arranged his table of ms distances etc. to yield a "near-neighbour" sequence, but has regarded it merely as a pleasingly regular way of presenting the data, and not as an important result in its own right; so Canivet-Malvaux (p. 400), and possibly the editors of the Vulgate Isaiah (p. xxx). Thus Malvaux, having arranged his mss in a particular order and constructed a "triangular" table of distances, remarks: "Le rythme signalé des distances allant croissant de la gauche vers la droite et en décroissant du haut vers le bas est d'une régularité remarquable"; but whereas Griffith would consider such a result to be of great interest in itself, Malvaux saw it as a side-show which could not be allowed to divert him from the pursuit of what was for him the only worth-while goal, namely a stemma.
Over these various applications of seriation, a number of different procedures have emerged for deriving the ordering from the figures. In simple cases this can be performed by trial and error; there are available moreover a number of mathematical procedures, mostly in the form of computer programs. In TSJ, Griffith worked by trial and error; for his NTG study, however, he developed, in consultation with Mr J.R. McKenzie of Jesus College Oxford, an arithmetical procedure (called the "snowball" method) which does not require computer assistance. Indeed, it is an important general point in favour of seriation that it tends to involve far less computation than most of the other methods commonly practised, in particular MDS.

Whichever procedure is used, it is important that at no point do we have to make up our minds regarding the rightness or wrongness of any reading. Thus the spectrum, like the map, rests on a thoroughly objective basis.

In TSJ, Griffith investigated the textual tradition of the whole of Juvenal's sixteen satires, which he divided into eight sections. Fifteen mss, dating from cent. ix-xii, entered into the analysis; not all of them, however,

contain the entire text. His NTG study dealt with four samples from the Gospels, viz


Fourteen mss (or more specifically, thirteen uncials and 'one papyrus) were selected for the Luke samples, and fifteen (the same uncials plus two papyri) for the sample from John.

Thus in both TSJ and NTG he obtained, over the different domains from a single corpus, a number of different spectra. Although the eight orderings of TSJ were not identical, they were very similar to one another. Griffith shews that this agreement goes too far to be explained by chance alone^1; this fact enhances our confidence in all the orderings, and should be taken to heart by those who assert that where contamination is present, no order or regularity of any kind can be discerned^2. Whatever differences do exist he attributes to changes of allegiance by individual mss over the course of the work.

Here I must sound a warning. Some or all of the differences could perhaps be due to some inaccuracy in the method (e.g. the samples on which the orderings are based may be too short

---

1. He calculates Spearman's Rank Correlation Coefficient between pairs of orderings, and then applies the t-test. For every pair, t is significant at the 1% level.

2. Cf the remarks of P. Mertens quoted above (p. 2:58).
to yield a completely reliable result); from every discrepancy one cannot without further ado deduce a change of affiliation. However, in many cases a study of the readings themselves confirms that such a change has indeed taken place; see his remarks on A (p. 129) and in particular his documentation of the shifting allegiances of G (pp. 136 ff). The position regarding the four spectra of NTG is similar. They differ somewhat from one another, but the extent of their agreement is too great to be due to chance. In both cases, then, the spectrum offers us an objective basis for classifying the mss, and thence—if caution is exercised—a means of detecting shifts of affiliation.

In TSJ the spectrum invited a further interpretation. It was found in all eight cases that the most sincere witness (P) appeared at the right-hand end, whereas the mss on the left-hand side had been considerably disfigured by interpolation. So far as one could tell, it seemed true

1. In Juvenal at least, this enabled Griffith to put the grouping of the mss on a sounder basis than had yet been achieved. Thus he was able to shew that some features of U. Knoche's classification (such as his identification of the \( \Gamma \)- and \( \Lambda \)-groups) had to be corrected.

2. This term does not denote the addition of spurious matter to the text, as one might have expected; it is something of a technical term used in the textual criticism of Juvenal, and, in contrast to "corruption", it denotes scribal changes which distorted the text and were introduced deliberately, e.g. in order to bowdlerise, or to smooth over a difficult phrase.
in general that as one proceeded along the spectrum from the right-hand end to the other extreme, one encountered mss which interpolation had affected progressively more seriously. Therefore the near-neighbour sequence, which is based on measures of similarity, seems capable of serving also as a scale of the degree of interpolation. This result is based on intuition; it has not been shewn rigorously to be true. If accepted, it has important consequences for an editor. The position of a ms in the spectrum, in respect of a given section of the corpus, will help us to decide whether or not to select it for citation in a critical apparatus. Furthermore, a ms which has not yet been collated should first be examined over a sample of passages and provisionally located in the spectrum; its place there will help us to decide whether or not the new witness deserves to be collated more fully. Finally, our confidence in an attractive reading which occurs in only a small number of mss may be increased, should the ms or mss

1. Proceeding from right to left, we find after P its close congener R, where extant. We next find W (again where extant), a ms "in which the malady [of interpolation] has made no formidable progress" ("D. Iunii Juvenalis Saturae... ed. A.E. Housman", p. viii). Immediately to the left of these, we tend to find the members of Knoche's Č-group (GJNU), of which Griffith says: (p. 133): "If P had not survived, this taxon could have led to a tolerable text, though there would have been more scope for editorial imagination and ingenuity". The mss which are found consistently at the left-hand end, however, namely BHV, are all heavily interpolated.

2. See further our remarks on pp. 11:139 f.
attesting it be located, over the particular stretch of


text concerned, in the right-hand section of the spectrum;


of this Griffith offers some examples (p. 133). In NTG,


however, the spectrum was not capable of such an interpretation,


and therefore no analogous inferences were possible.


This is, I hope, a fair summary of Griffith's work. A


number of points call for further discussion; we may begin


with the mechanics of calculation for arriving at a spectrum.


The trial-and-error approach of TSJ seems to have given


sound answers, even if they were not proved rigorously. I


am somewhat dubious, however, about the "snowball" method


employed in NTG. Its workings are explained in fig. B.11.27


(see p. 11:131). What it does basically is to complete a
tour of the mss, visiting them in the order wherein they will
lie (from left to right) on the spectrum. One begins with
the most closely-knit group and goes on to mss that are
progressively more distant from that initial group; at the
same time, there is inherent in the method a mechanism aiming
to provide that whenever one lights, in the course of the tour,
upon a ms which has extant congener and is thus a member of
a particular family, one will go on to visit all the members
of the family concerned, before proceeding to visit any ms
outside that family.
TABLE A

| A | B | C | D | E | F | G | H | K | L | N | O | P | Q | R | S | V | X | Y |
| A | 15 | 15 | 15 | 4 | 25 | 21 | 19 | 27 | 37 | 41 | 10 | 23 | 27 | 44 | 24 | 46 | 45 | 55 |
| B | 13 | 29 | 29 | 17 | 33 | 39 | 6 | 17 | 22 | 30 | 41 | 58 | 56 | 56 | 60 | 25 | 69 | 74 |
| C | 15 | 29 | 31 | 19 | 29 | 37 | 39 | 42 | 29 | 29 | 24 | 38 | 42 | 48 | 31 | 35 | 31 | 52 |
| D | 15 | 29 | 31 | 19 | 19 | 6 | 35 | 42 | 30 | 35 | 35 | 26 | 23 | 26 | 42 | 36 | 40 | 50 | 45 |
| E | 4 | 17 | 19 | 19 | 29 | 25 | 23 | 31 | 40 | 45 | 6 | 19 | 23 | 40 | 46 | 40 | 50 | 38 | 48 |
| F | 25 | 38 | 40 | 11 | 29 | 17 | 44 | 50 | 19 | 23 | 36 | 34 | 38 | 30 | 25 | 28 | 58 | 54 |
| G | 21 | 30 | 37 | 6 | 25 | 27 | 19 | 48 | 56 | 26 | 32 | 17 | 21 | 40 | 42 | 45 | 56 | 56 |
| H | 19 | 6 | 35 | 35 | 23 | 44 | 40 | - | 8 | 56 | 59 | 28 | 42 | 46 | 63 | 62 | 65 | 19 | 67 |
| J | 27 | 13 | 42 | 42 | 31 | 50 | 48 | 8 | - | 62 | 65 | 36 | 50 | 54 | 69 | 67 | 71 | 12 | 73 |
| L | 37 | 50 | 25 | 30 | 40 | 19 | 36 | 56 | 62 | - | 4 | 46 | 45 | 49 | 50 | 30 | 6 | 9 | 54 | 34 |
| N | 41 | 29 | 40 | 37 | 39 | 6 | 17 | 22 | 30 | 41 | 58 | 56 | 56 | 60 | 25 | 69 | 74 | 52 | 48 |
| O | 10 | 22 | 24 | 26 | 6 | 36 | 32 | 28 | 36 | 46 | 51 | - | 14 | 18 | 40 | 52 | 56 | 42 | 48 |
| P | 23 | 37 | 38 | 23 | 19 | 34 | 17 | 42 | 50 | 45 | 50 | 14 | - | 4 | 30 | 51 | 55 | 56 | 38 |
| Q | 27 | 40 | 42 | 26 | 25 | 38 | 21 | 46 | 54 | 49 | 54 | 18 | 4 | - | 26 | 55 | 50 | 62 | 39 |
| R | 44 | 58 | 48 | 42 | 40 | 30 | 40 | 65 | 69 | 30 | 27 | 30 | 26 | - | 36 | 40 | 69 | 19 |
| S | 42 | 56 | 31 | 36 | 46 | 25 | 42 | 67 | 6 | 10 | 52 | 51 | 55 | 36 | - | 4 | 60 | 28 |
| V | 46 | 60 | 35 | 50 | 28 | 45 | 65 | 71 | 9 | 13 | 56 | 55 | 58 | 40 | 4 | - | 63 | 25 |
| X | 35 | 25 | 31 | 50 | 38 | 58 | 56 | 13 | 12 | 54 | 57 | 42 | 58 | 62 | 69 | 60 | 63 | - | 69 |
| Y | 48 | 62 | 52 | 45 | 48 | 34 | 47 | 67 | 73 | 34 | 35 | 48 | 42 | 38 | 19 | 28 | 25 | 69 |

Explanation. Table A gives percentage distances, rounded off to the nearest integer, for Quartz's model tradition; the calculations involved in the "snowball" analysis are shown in Table B.

How to perform "snowball"

Write all the sigmas across the top of a page. For each ms, identify the three smallest totals in its row (see Table A) and add them together; the ms with the smallest score will be the first in the spectrum. Here the "winner" is L, with a total of 46-69-19. We call this ms our first "out-going" ms. Now write down the distance between the "out-going" ms and every other ms under the sigmas appropriate to the latter, and add it to whatever total has already accumulated (if any) in that column. The ms under whose sigmas appears the lowest entry in the resulting row of totals will be the next to stand in the spectrum, and becomes the new "out-going" ms; the old "out-going" ms will play no further part in the calculations. Repeat the process as for the first "out-going" ms, again and again until all the ms have been assigned their places in the spectrum. (As Mr. Griffith has remarked, perhaps a more appropriate name for the procedure would be "musical chairs".)

TABLE B

| A | B | C | D | E | F | G | H | K | L | N | O | P | Q | R | S | V | X | Y |
| L | 37 | 50 | 25 | 30 | 40 | 19 | 36 | 56 | 62 | - | 4 | 46 | 45 | 49 | 50 | 30 | 6 | 9 | 54 | 34 |
| N | 41 | 29 | 40 | 37 | 39 | 6 | 17 | 22 | 30 | 41 | 58 | 56 | 56 | 60 | 25 | 69 | 74 | 52 | 48 |
| O | 22 | - | 15 | - | 4 | - | 19 | 27 | - | 10 | 21 | 27 | 43 | 52 | 48 |
| Q | - | - | - | - | - | - | - | - | - | - | - | - | - | - | - | - | - | - |
| R | 46 | 60 | 35 | 50 | 28 | 45 | 65 | 71 | 9 | 13 | 56 | 55 | 58 | 40 | 4 | - | 63 | 25 |
| S | 48 | 62 | 52 | 45 | 48 | 34 | 47 | 67 | 73 | 34 | 35 | 48 | 42 | 38 | 19 | 28 | 25 | 69 |

FIG. B.12.27
Sometimes the whole process is effective; thus I have obtained reasonable and satisfactory spectra for my data on Cyprian's De Unitate:

P k Y W e G B R b O a J D h T H p m

and the Persae of Aeschylus:

\[ \Delta B H C A M I Y a P V N N d Q K Y O \]

and the Peshitta Psalter:

\[ \begin{array}{cccccccccc}
Ua & Uc & m & O & K & L & N & J & G & C & S & Q & H & E & D & A & T & F & \text{Le} & R & B \\
\end{array} \]

Nestorian \quad Jacobite

"early" type \quad "late" type

It seems however that the mechanism for ensuring that one family is exhausted before we proceed to the next, cannot always be relied on, especially towards the right-hand end of the spectrum. This can be illustrated from an application of this method to the mss of the Vulgate Isaiah\(^1\). Here there are two mutually opposed groups of mss, viz the Spanish (\(C X I^T \Delta^{1L}\)) and the Transalpine (RTSZU); they form, as it were, the two "extremes" of the tradition\(^2\), and neither the text nor the figures give us any reason to suppose that the two groups have in any way become fused

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1. The numerical table of distances employed is that drawn up by the Benedictine editors on p. xxx of their edition.

2. See Ch. III of the Benedictine edition, and also our map on p. 6:
together. But in the spectrum to which the "snowball" method leads, the two groups have become mixed up:

This has happened because these two extreme groups are more or less equally "distant" from the mss at the left-hand end, namely the tight-knit cluster of Theodulfian mss (O).

Again, let us consider the result obtained for Quentin's model tradition 1:

Here again the right-hand end can hardly be correct; for Y certainly does not belong between B and its immediate descendant H.

Yet another pointer to the unreliability of the right-hand end of the spectrum can be found in Griffith's own treatment of Codex Bezae (D). When he had compiled his figures for his first Luke sample according to his customary rules — including in particular the ignoring of all unique

1. For the calculations, see p. 11:131; for the textual history, see p. 5:3.
readings - he applied to them the "snowball" method and obtained the ordering:

A Y E S Θ fam 13 W fam 1 L Φ D p75 B

According to this, D, the only witness to the Western text here included, was embedded in the middle of the Alexandrian group (here represented by L Φ p75 B); and in all the other three samples, we are told (p. 401), D behaves in a very similar manner. Yet the evidence of the readings and of the figures themselves militates strongly against our placing D among the Alexandrian mss. Griffith seeks to deal with this difficulty in two ways. On the one hand, he tentatively suggests that D's position in the spectrum is meaningful: "In dealing with so eccentric a witness as D, caution is obviously called for, but it is perhaps for consideration that before D acquired its distinctive features, its ancestor (or ancestors) was a text closely related to the Φ - p75 - B taxon, something which could not be suspected a priori". In the main, however, he was inclined to doubt the result, and modified it by adding to his field of variants about 45 passages wherein D has a unique reading - against his usual practice. This brought D out at the right-hand end of the spectrum, outside the Alexandrian group, but at the price of a definite taint of "cookery". But the fact is that the original anomalous position of D was due once more to the method1: the tightest-knit family consists of the Byzantine

1. As the reader will soon see, the difficulty disappears when we turn to another method.
mss AYE..., from which both D and the Alexandrian mss are more or less comparably dissimilar - though they are at the same time sharply dissimilar to each other - and that same failing in the method has mixed them up together.

It would be wrong, however, to allow the suspicion attaching to the "snowball" method to undermine our faith in the value of seriation generally. I would therefore advocate that other procedures for discovering the spectrum be tried out.

I have myself looked into the performance of a powerful (albeit not wholly elegant) method, due to R.S.Kuzara et al.1, over the four samples studied in NTG2. For three of these, this method gives an unequivocal result, which I compare below with that of Griffith3:

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2. I am most grateful to Mr. Griffith for sight of his data.

3. To obtain the "Kuzara" spectra, I used Griffith's data in its original state, i.e. without making special provision to include any unique readings of D. The "Griffith" spectra, on the other hand, are those established in NTG, and are derived from the data as modified by the inclusion of some of those unique readings.
In Luke 14:1-15:17, however, where Griffith offers:

S W E Y N A Θ fam 1 fam 13 L N B ρ75 D

the method of Kuzara et al. hesitates between three equally "well-fitting" spectra (strain=147):

- D fam 1 fam 13 A W E S Y Π Θ L N B ρ75
- D Θ fam 1 A N W E S Y fam 13 L N B ρ75
- D Θ fam 1 A W E S Y Π fam 13 L N B ρ75

That is, the order of ten of the witnesses is agreed (DAWESYLN ρ75), but it is not clear where the four other witnesses (Π and Θ, fam 1 and fam 13) fit in. I leave it to those who are better qualified than myself to judge the value of these spectra.

1. The number in brackets is the sum of the "negative differences"; the smaller this "strain", the better the spectrum fits the data.
The spectra can be interpreted up to a point. It is usual\(^1\) to regard these witnesses as falling into four main groups, viz

(i) Alexandrian: \(\text{K \& \(p\)}\); \(\text{BL}^\psi\text{p}\); \(\text{M}^\psi\); \(\text{W}\) in John\(^2\).

(ii) Western: \(\text{D}\).

(iii) Caesarean: \(\Theta\), fam 1, fam 13.

(iv) Byzantine: \(\text{AE}\text{S}\text{BY}\); \(\text{W}\) in the Luke samples\(^3\).

In the first Kuz. spectrum, the four groups are distinct, and appear in the order: West., Byz., Caes., Alex. In the others, however, the Byzantine and Caesarean witnesses coalesce to form a larger group, within which the Caesarean mss are scattered in various ways. This may possibly be because (1) both the Caesarean and the Byzantine texts are essentially mixtures, albeit of differing complexions, formed from the Alexandrian and Western texts, and (2) the Caesarean text is the most mixed and the least homogeneous of the four. The three groups that have now emerged remain distinct in all the spectra, but (1) the order of the witnesses within each group varies, and (2) so does the order of the three groups themselves (with Alex. sometimes in the middle, sometimes West., sometimes Byz./Caes.).

Although I think that (1) may prove important, I doubt whether (2) is as significant as it might at first appear. For each of the three classes is substantially different from both the others, yet the fact that we are arranging them in a spectrum means that one of them will perforce have to stand between the other two; it does not seem to matter a great deal which of the three happens to be forced into the position of bogus "mediator", or whether different "mediators" emerge over different samples. Altogether, my impression is that the spectrum, which is formulated in only one dimension, is not really adequate to represent this particular material.

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1. Here I follow B.M. Metzger, "The Text of the New Testament" (Oxford 1964), Ch. VIII.

2. and in a small part of the first Luke sample (8:5b-12)

3. except for a small part of the first.
Let us now turn from the question of mechanics to the wider issues concerning the usefulness of the spectrum. My feeling is that the value of the spectrum has to be determined anew for each textual tradition to which the method is applied. In general, it is likely to provide an objective basis for the classification of the mss; beyond that, we can expect the spectrum to be meaningful somehow, but we cannot specify in advance in precisely what way. It does not claim to be a sketched history of the text; still less does it yield ready-made canons for choosing between rival readings. Its usefulness in any particular case will depend on at least three factors: (1) the extent to which it "fits" the data, (ii) the interpretation that can be attached to it, (iii) the logical basis that can be advanced for such an interpretation. Let us enlarge on these three points:

(1) We cannot take it for granted that every array of specimens is capable of being satisfactorily represented in the form of a spectrum. It may turn out that, no matter how we arrange the mss, we find an unacceptably high number of instances of a mss being placed between two others without being in any way a mediator or "half-way house" between them. True, the Juvenal spectra are impressive; but Cowgill has had experience of arrays of archaeological specimens that simply "do not seriate well" (p. 374), and the textual critic too must be prepared for such a contingency. To put it another way: a spectrum is by definition one-dimensional, and if the divergences that exist between our mss are due to many
independent factors, then several factors may be competing for expression in the one available dimension, and the result could well be an uninterpretable mess\(^1\).

(i) Granted that we have arranged the mss into a satisfactory "near-neighbour" sequence, we cannot tell in advance how that sequence can be interpreted, nor how useful such an interpretation would prove when we came to choose between rival readings. The case of Juvenal was particularly favourable, in that the spectrum lent itself to being viewed as a scale of sincerity, with obvious implications for the relative worth of variant readings. But no such straightforward interpretation has been offered for the NTG spectra, and in general we must expect the interpretability of the spectrum to vary considerably from one tradition to another.

(iii) Granted further that we can offer a satisfactory interpretation, we cannot be confident of our conclusions unless we can point to a rationale explaining why that interpretation should hold. This is no mere quibble. Thus, in the field of archaeology, Robinson was at pains to shew why a spectrum based primarily on relative similarity should be interpretable in terms of chronology; he offered a background theory based on the view that the principal factor responsible for the differences between the different members of an array of archaeological deposits, was the rise

\(^1\) That one dimension sufficed for the Juvenal data is perhaps due to the possibility that one particular factor was overwhelmingly predominant, viz the extent of interpolation.
and fall, effected in the course of time, in the popularity of different forms of artefact. However, this rationale - and, by implication, his conclusions - has been disputed by J.B. Kruskal: "the dissimilarities [between different deposits] may reflect other variables in addition to time - for example, social class, wealth, climate and so forth". In our field too, a rationale must be explicitly stated, so that it can be discussed, and if necessary, disputed.

If the spectrum can satisfy us on these three counts, then it will have the important advantage over most other methods, that the amount of computation it involves is modest. In particular, it should prove feasible to divide a text into many sections, and, by obtaining a spectrum for each, detect changes of affiliation.

In conclusion, although one cannot predict with what success an attempt to analyse the mss in the form of a spectrum will meet, the results achieved so far are without doubt impressive. We may expect seriation to prove itself of great value in textual investigation.

1. MAHS, p. 120.
PART C

PRELIMINARY NOTE

This final part is concerned with two mathematical methods which have been widely utilised in other fields (e.g. archaeology, biology) but not in any work as yet published on textual criticism. The two methods are hierarchical clustering (or, cluster analysis) and principal components analysis (which is a form of factor analysis).

What follows does not claim to be in any way exhaustive. I have not tried to explain in my own words the workings of these methods; the reader to whom they are unfamiliar may use the account given by Cowgill, and the works cited at the end of his article, as a starting-point. The two methods here described, although they are particularly well-known, do not by any means exhaust the field of statistical pattern recognition; but they are the only techniques for which I have results relating to textual criticism. Nor have I investigated these two methods themselves any more like as thoroughly as MDS. I do nevertheless feel that the results which I have to hand are worth reporting and can help us to assess the potential value of these approaches for the textual critic.
References

Cowgill's discussion of this method (pp. 369 f.) offers a good account of what hierarchical clustering achieves. A number of different criteria have been proposed for forming clusters; several studies are listed in Cowgill's bibliography. In his article, he describes the "average-linkage" approach; my own preference however has been for the techniques expounded by S.C. Johnson, "Hierarchical Clustering Schemes", Psychometrika (1967) pp. 241-254.

The method with which we are here concerned sets out to fit a given array of objects into a hierarchical clustering scheme, which may be conveniently represented in the form of a tree. In straightforward cases, the analysis can be performed by hand; when the number of specimens is large (say 15 or more), computer assistance will probably be called for, but the computer time required will be only a small fraction of that which an MDS analysis would demand.

The basic information required is a table of measures of similarity (or dissimilarity) between each pair of objects. For the task of deriving a hierarchical clustering scheme from these figures, a number of alternative techniques exist. The above article by Johnson advocates two such techniques: the clusters formed by one are optimally "connected" and those formed by the other optimally "compact"; these terms are defined explicitly within his article.

In our own field, the statistics of similarity required for such an analysis may be based either on the
number of times a pair of ms agree (over a given field of variants) or on the number of times they agree in error. As we have stated above (pp. 3:4 ff.), the former criterion has the advantage of being wholly objective, but in the present context the latter has certain points in its favour, as we shall presently learn. I have tried out both of Johnson's techniques with both types of data.

Let us first consider results based on the criterion of simple agreement. The textual traditions chosen for this investigation will already be familiar to the reader: Dom Quentin's model text, Cyprian: De Unitate, Aeschylus: Persae (lines 1-746), and the Peshitta Psalter. The results are presented in a tree form in fig. B.11.28.

These results are of obvious interest, but I wonder to what extent they are truly enlightening. A hierarchical clustering scheme, although it can be represented by a tree, is intended as a classification, not as a sketch of the history; indeed, should anyone be tempted to suggest that these clusterings could serve as stemmata, it will be sufficient to note how little resemblance there is between the clusterings obtained for Quentin's model text and the diagram showing the true history (p. 5:3). As classifications, the diagrams presented in fig. B.11.28 are satisfactory in some respects, but hardly in all. True, many of the groupings they establish agree with the results of scholars who used traditional methods. It turns out

1. e.g. the identification of the groups P k, W Y, h H T, H T, m p, for Cyprian; N Ed P V (in the 'connected' clustering only), K Q, O Y, B C A H, for Aeschylus; C G H Q S ("early Jacobite") and K L m N O Ua Uc ("Nestorian") for the Peshitta Psalter.
however that most of these "correct" groupings (1) are found in the lower regions of the tree, and therefore concern relatively small groups, and (2) are common to both the "connected" and the "compact" clusterings. The larger groupings, on the other hand, seem to become progressively less useful. Firstly, the "connected" trees show a disturbing number of instances of a sizeable cluster which divides into a single ms on one side and all its other constituent mss on the other. Secondly, the two techniques work out these higher links quite differently. This disagreement is not of course the fault of the techniques themselves, which are based on two differing criteria, but it can only confuse the textual critic who is studying a complicated textual tradition and turns to cluster analysis in the hope of clarification. Thirdly, there are some alarming discrepancies between these results and those established by traditional means: for example, in the "compact" clustering for Aeschylus, P is associated with A I M, and two clusters of quite different textual character (K Q, C Y) intervene between P and the other members of the group to which it in fact undoubtedly belongs (N Nd F V). In sum, I would endorse the view of Cowgill (p. 370) that "there is a relatively large amount of information in the smaller branches, and progressively less as one moves to larger and larger branches; and as the smallest groupings will probably be known to the investigator already, I find it difficult to see in what way these classifications can be relied on to tell us something which is new and of real value.

We now turn to results based on agreement in error. The special interest attaching to this criterion will be appreciated if one supposes a hypothetical tradition wherein the six assumptions of Ch. 1 hold good, together with the further assumption that no two scribes commit errors at the same point in the text. These assumptions amount to virtually the same as Proger's conditions of "généalogie normale" (Thes., pp 1:42 f). Then the errors common to two given mss will be precisely those which were present in their latest common ancestor¹, so that the number of agreements in error between two mss will be the number of errors contained in that common ancestor. Now fig. B.11.29 illustrates for a particular example a proposition of whose general validity one may readily convince oneself, namely that if any of the more popular clustering techniques² is applied to a table of agreements in error which has been drawn up for such a tradition, then a tree will be obtained which recovers (with one reservation³) the historical relationships and can serve as a stemma. This comes as something of a surprise, in that the tree given by cluster analysis does not start out with any stemmatic pretensions.

1. As before, the set of ancestors of a ms is regarded as including that ms itself.

2. such as "average-linkage", or either of Johnson's techniques.

3. namely that if one extant ms is an ancestor of another, it will appear instead to be a collateral thereof.
Explanation. Suppose a tradition of 11 mss, A..K; A is the original, and the mss are related to one another according to the stemma (i). There are fourteen passages which are transmitted incorrectly in one or more of the mss; let these passages be denoted by the letters a..n. It will be assumed that every ms subsequent to A reproduces all the errors of its exemplar, and adds one or two errors of its own. The places where these errors are introduced are listed (ii); thus B has an error in passage a, C reproduces that error and has two more in passages b and j, and so on. The table (iii) shows where each ms has the original reading (denoted /) and where it has an error (X). Suppose now that ABEJ are lost, and that a stemma is to be drawn up for the mss that remain. The true stemma, which we must presume to be as yet unknown, is (iv). We may draw up a table of agreements in error (v), and apply cluster analysis thereto*; the result is a tree (vi) which is identical with (iv) in every respect except that C appears to be a collateral instead of an ancestor of D.

* Note that after we have joined C and D (with similarity index 3), we find three pairs with similarity index 2 which clamour simultaneously to be joined together, namely PG, PH, GH. The obvious solution, which I have adopted, is to create a junction with three branches. Normally, of course, cluster analysis will yield a wholly binary tree; but the circumstance that three of the entries in the table are absolutely equal is unusual, and calls for unusual measures.

FIG. B. 11. 29.
The idea now suggests itself of applying cluster analysis to the tradition of a real text; for though "généalogie normale" can in real life be no more than a rough approximation to the truth, and though our table may be somewhat inaccurate because we sometimes err in deciding which of the available readings is original, we may nevertheless hope to reach thereby a "best-fitting" or "first-approximation" stemma.

I have drawn up such trees for the Aeschylus data.

1. For details of the readings deemed to be original, see p. 6:
Of the trees shown in fig. B.11.30, (a) is based on "connectedness" and (b) on "compactness". They agree with each other far more than the trees of fig. B.11.28 do; in fact, if we neglect A 0 Y Ya, the sub-graphs containing the twelve remaining mss are identical (c). Both diagrams moreover agree in many respects with the conclusion of scholars who used 'literary' methods alone:

I. If we extract from the stemma of A.Turyn\(^1\) (p.115) fifteen of the sixteen mss here considered\(^2\), we find that he too proposed a two-branched stemma, with M as the sole representative of one of the two great families.

II: The twelve mss A B C \(\Delta H\) N Nd O P V Y Ya, and none of the other four, were considered by Turyn to go back to an early common ancestor \(\Phi\).

III: Particular families are formed by:

\[\begin{align*}
(1) & \quad B C \Delta H - Turyn's Class \(f\) (pp. 53 ff.); \\
& \quad Dawe\(^3\), pp. 23 ff. \\
(11) & \quad N Nd P V - Dawe, pp. 33 f. \\
(111) & \quad O Y - Dawe, pp. 31 f. (who groups together OYYa, but states that OY "cohere very closely") \\
(1v) & \quad k Q - Turyn, pp. 76 f. \\
& \quad ("Thoman mss"), Dawe pp. 35 f. \\
\end{align*}\]


2. The remaining one is Cod. I, which has to be left on one side because Turyn had no opportunity of investigating it.

Thus if no stemma of the mss of Aeschylus had been attempted, and we had ourselves sought to formulate one by this method, the results would have been of some value. The real point, however, is whether any stemma can give an adequate picture of the Aeschylus tradition; and Dawe has proved conclusively that it cannot. Thus the trees of fig. B.11.30 would have been suggestive, but they are very far from telling the whole story.

The possibility therefore remains open that it be worth while to ascertain whether textual traditions exist wherein the malady of contamination has progressed far enough to cause Froger's method to break down, but not far enough to rule out the compilation by this method of a meaningful "approximating" stemma.

Fundamentally, however, I feel that whichever type of similarity measure is utilised, cluster analysis cannot do justice to the complexity of most textual traditions. Its basic limitation, as Cowgill states, is that it "does not get away from a fundamentally classificatory or 'pigeonholing' approach toward data. At any stage, a link is made on the basis of joining the units or clusters which best meet the linking criterion, without regard for whether some alternative possible link meets the criterion almost as well or whether all possible alternatives are much poorer" (p. 370). Though cluster analysis is rendered attractive by its computational ease, and though it may prove useful on occasion, my impression is that many text-critical
problems call for a method capable of greater subtlety and sensitivity.
References

Cowgill, pp. 370 ff., under the heading "Factor Analysis", will give the non-mathematician some idea of what PCA sets out to do; however I feel that PCA rests on too many of the abstract mathematical concepts of linear algebra to be rendered satisfactorily comprehensible to one who is innumerate. For the more mathematically inclined a number of accounts are available, such as K. Fukunuga, "Introduction to Statistical Pattern Recognition" (New York and London, 1972), Ch.8. The mathematical background can be found in a college algebra textbook such as G. Birkhoff and S. MacLane, "A Survey of Modern Algebra" (revised ed.; New York 1953), pp. 192 ff. This section has been written with the mathematician primarily in mind.

Suppose that we are given the readings of a set of extant mss over a substantial collection of variant passages. It is possible to formulate the patterns of agreement and divergence among the mss by assigning to each ms a vector in a large number of dimensions. These vectors can then serve as a basis for a PCA treatment.

The reduction of the data to vector form can be achieved in more than one way. What is probably the simplest is to restrict ourselves to passages that exhibit a two-way split, and to allocate one dimension to each. One ms may be chosen arbitrarily as the "basic" ms. This done, a ms will be assigned in a given passage the value 1 in the appropriate dimension if it is in agreement with the "basic" ms, and the value 0 if it is not. An alternative method, which allows us to include cases wherein more than two readings are attested, is to allot to every k-way split k dimensions (k $\geq 2$), each of which is associated with one
particular reading. According as a ms does or does not attest a given reading, it is assigned the value 1 or 0 in the appropriate dimension. The accidents of time, and the efforts of correctors, will probably have resulted in a number of instances wherein the reading (sc. of the first hand) of a ms has been lost; these may be dealt with either by omitting such passages altogether or (w the problem is more serious) by assuming the value 0.5 where we do not have the information needed to choose between 0 and 1. By some such process, then, every ms is associated with a vector in a large number (perhaps a hundred or more) of dimensions, and we then seek to apply PCA in order to obtain vectors in a far smaller number of dimensions (ideally two or three, if a visual presentation is contemplated) which will serve to characterise each ms adequately.

I have not carried out any such experiments myself, but Mr. Griffith has generously acquainted me with some of his own results, which he reached in collaboration with Dr. C. Rogers of Jesus College Oxford. Full details of his investigation are to appear in his forthcoming book: "Numerical Taxonomy and Textual Criticism". These results seem to me suggestive and valuable in many respects, and I look forward to Griffith's own discussion. For the moment I must point out, however, that they indicate that the textual critic who would utilise PCA faces two rather serious drawbacks.

One is that no means have been devised of interpreting the respective vectors which are eventually assigned to the
mss after the necessary transformations have been executed. Until some principle of interpretation has been laid down, the textual critic is not likely to find that these results offer real guidance in the tasks which confront him (e.g. the evaluation of rival readings), as opposed to being politely called interesting and stimulating, or to appearing somehow reasonable on an intuitive level.

The second difficulty is of a purely mathematical nature. To restrict oneself eventually to a small number of dimensions is not satisfactory unless the sum of the eigenvalues associated with the dimensions we adopt, forms by far the greater part of the sum of all the eigenvalues. It turns out, however, that over the 23 analyses conducted by Griffith, the rate at which the eigenvalues decrease is discouragingly slow - which is, incidentally, an interesting result in itself. The one possible exception is Horace: Ars Poetica, for which the three greatest eigenvalues together make up 71% of the total; in the 22 other experiments, the proportion is less than 60%, and it falls short of 50% in 11 of these. The results are given in greater detail in Table B.11.5. Two possibilities are then open to us. Either we work with more than three dimensions; a visual representation is then ruled out, and our conclusions will have to be stated in terms of numbers alone. Such numbers, to which no interpretation can as yet be attached, would do little, I fear, to enlighten most textual critics - though one must admit that if a theory of interpretation were established, PCA could well
<table>
<thead>
<tr>
<th>Literary work analysed</th>
<th>No. of samples</th>
<th>No. of mss</th>
<th>No. of variants per sample (approx.)</th>
<th>Number of cases where $r^*$ falls within range:</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>40-45</td>
</tr>
<tr>
<td>Aeschylus: various extracts</td>
<td>6</td>
<td>14-16</td>
<td>60-110</td>
<td>1</td>
</tr>
<tr>
<td>Horace: Ars Poetica (entire)</td>
<td>1</td>
<td>10</td>
<td>70</td>
<td>-</td>
</tr>
<tr>
<td>Juvenal: the sixteen Satires</td>
<td>11</td>
<td>18-21</td>
<td>70-130</td>
<td>-</td>
</tr>
<tr>
<td>Gospel of Mark</td>
<td>1</td>
<td>24</td>
<td>100</td>
<td>-</td>
</tr>
<tr>
<td>1 Corinthians</td>
<td>1</td>
<td>25</td>
<td>80</td>
<td>1</td>
</tr>
<tr>
<td>Rule of St. Benedict</td>
<td>3</td>
<td>26</td>
<td>120</td>
<td>1</td>
</tr>
<tr>
<td><strong>TOTAL</strong></td>
<td><strong>23</strong></td>
<td>-</td>
<td>-</td>
<td><strong>3</strong></td>
</tr>
</tbody>
</table>

\[ * r = \frac{\text{sum of first three eigenvalues}}{\text{sum of all the eigenvalues}} \times 100 \]
prove itself especially powerful precisely because it brings in a great number of dimensions, each of which would then be interpretable. Or we confine ourselves to three (or, more conveniently, two) dimensions, in which case we shall have to acknowledge that a considerable proportion of the information available to us is thereby neglected. One may hope, however, that progress will be made towards easing both these difficulties.

It would be indeed rash to arrive now at anything like a final conclusion on the potential value of PCA in the field of textual criticism. Although we have pointed out certain problems, the great usefulness of PCA within other disciplines certainly suggests that it is well worth following up here too.

1. One could retort that in MDS the proportion of information discarded is even higher (p. 4:23). The fact remains that the results of our MDS experiments (with "stress" lying in all cases below 17%) would be considered pretty well satisfactory by exponents of that method, whereas the sort of results yielded by PCA (with the first three eigenvalues - which, if a visual representation is to be possible, we should have liked to adopt to the exclusion of the others - accounting for no more than 50% of the total) seem rather less encouraging in relation to other applications of PCA.
The work which follows was done towards the beginning of my research studies, before my interest had turned to the textual criticism of mss as such. I decided to examine the P' rendering of the Foreign Oracles in Jeremiah, partly because Jeremiah is one of the few O.T. books for which there is available no detailed study of the Peshitta version, and partly because these chapters, although their general meaning is clear, must have presented the translator with considerable difficulties, and may be expected to provide for us a favourable opportunity of observing his methods.

It should be made clear immediately that this section is on a rather different footing from the many detailed "Einzeluntersuchungen" that have been undertaken on portions of the Syriac O.T. It does not consist, then, of a thorough investigation of P' in these chapters; instead, I have tried to follow up certain questions which suggested themselves in the course of my study of Jer 46-51, questions to which most

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1. A summary of the work of this section was presented to the Institute of Jewish Studies, London, in a paper read on 24 Feb. 1971.

2. A valuable investigation is F. P. Frankl's "Studien über die Septuaginta und Peschito zu Jeremia", in Monatschrift für Gesch. und Wissenschaft des Judenthums" (Oct-Dec 1872), pp. 444-456, 497-509, 545-557. It does not, however, claim to be exhaustive; still less does the present study, as the next paragraph will explain.

3. On these, see Thes., p. 1:5, n.1.
studies of P' cannot devote much space. Thus what is offered here starts out, in the first instance, from the stated passage, but I hope that it will prove to be of some interest to students of the Peshitta of the O.T. in general.

As there is not yet available a critical edition of the Syriac Jeremiah, I have not been content to use Lee alone. I have generally consulted the Ambrosian codex (7a 1) in Ceriani's facsimile edition, and on occasion employed various mss in the British Museum.

The topics considered may be conveniently grouped under four headings, and this Section is divided up accordingly.

§I. A POSSIBLE APPROACH FOR DISTINGUISHING DIFFERENT (SCHOOLS OF ?) TRANSLATORS OF P'

Our earliest extant information on the authorship of P' comes from Theodore of Mopsuestia, who states, in an oft-quoted passage 1: ήρμηνευται δε ταύτα είς μὲν τὴν Σβρων παρ’ άτον δήποτε (οὐδὲ γὰρ ἡγνωσται μέχρι τῆς τιμῆρον δοσίς ποτὲ οὕτως έστιν). This would suggest that one translator, albeit unknown, was responsible. However, Barnes 2 was convinced that the Peshitta, like the Septuagint,

was heterogeneous. In particular, "it is difficult to believe that the same school of translators rendered into Syriac both the Law and the Psalter". Thence Barnes goes on to deduce quite plausibly that "if there were as many as two schools, there may well have been more". However, there remain a great range of possibilities. Perhaps we shall find that a whole group of books (e.g. the Pentateuch) were the work of a single school (or indeed of a single translator); perhaps we shall find that different hands can be detected in the course of a single book.

Thus, the discovery that more than one school is represented, is only the beginning. We must go on to discover how many schools are responsible, which school is responsible for what, and indeed whether we ought to be speaking of schools or of individual translators. These questions are of great importance when we come to use P in establishing the Hebrew text. To be specific, one often reads an argument such as the following: "In books A, B, etc., the Hebrew word H is rendered by the Syriac S; in a certain passage in book X, the Peshitta also has S; therefore in that passage too there is a substantial possibility that the translator's Hebrew Vorlage had H." The cogency of this argument will depend to a great extent on whether X was the work of the same translator as A, B, etc., but to such questions, hardly

1. The possibility was entertained by Baethgen (op. cit, of Thes. p. 7:38) "dass die syrische Uebersetzung der Psalmen nicht aus einer Hand hervorgegangen ist" (p. 446).
any substantiated answers – as opposed to speculation – have been offered. Perhaps the following suggestions will provide a lead.

We continually find that MT has a Hebrew word which possesses in Syriac a cognate of like meaning. It is not surprising that P' generally employs that cognate to render MT, not only because it was the most obvious equivalent but also – we may surmise – because of a wish to carry over as much as possible of the form, as well as the meaning, of the Hebrew exemplar. A comparison of P' with MT in almost any passage of the O.T. will illustrate this. We often find whole phrases that have been rendered by the substitution of cognates, e.g.

Jer 46:10

Jeremy חרב נשבעתocratesמקים
שְׁבַּר עַל הַבָּשָׁם וְכִשָּׁם

Jer 49:32

רְוֹחֲמֵי לֵבַל-רָדִית קֶצֶרֶנְיָה פַּחַת
ַחַת הַנּוֹתַר הָלַחְתָּא

Jer 50:6

נַגְּיוֹן אֶכְלוֹת בֶּן, עָמָלִים נַחֲצָאש

Jer 51:38

נַעֲרָא מְכַנְּיֵי אֵרִין

1. These references have been verified in 5a 14 and 7a1. The former agrees with Lee; but 7a1 has לְמַשׁ in 49:32 and לַעֲבָרָה in 51:38.
This use of the cognate is not invariable: sometimes Syriac possesses another word in much commoner use, which the translator prefers. Thus to סירא there corresponds a cognate Syriac סירא; but סירא is normally rendered by the far more usual סירא. This is, in itself, not unexpected. What can be, however, of particular interest is a situation wherein a Hebrew word possesses a Syriac cognate which is consistently used to render it within some books and just as consistently avoided in others. This will call for an explanation; and the possibility suggests itself that that word was part of the working vocabulary of some Syriac speakers but was not employed to any considerable extent — although it may have been recognised — by others. This may be due to differences of dialect or merely to the vagaries of individual taste; in either case, there will be a suggestion that those books which repeatedly use that Syriac cognate, on the one hand, and those which fail to do so, on the other, are not the work of the same translator.

Consider, for example, the Hebrew סירא "go into exile",1 with its derivative nouns סירא and סירא. Cognates exist in Syriac, viz the verb סירא with similar meaning2 and the noun סירא. I was therefore surprised

1. By using the notation סירא, I do not mean to imply that here we have a root which is historically distinct from סירא "reveal": this is merely a device to distinguish a particular well-defined meaning of the root סירא, which here exhibits polysemy.

2. Payne-Smith (col. 717) groups this word together with סירא "reveal". When this is the meaning, the form סירא is there said to be commoner.
to find that, in all the four passages where חלוה appears within Jer. 46-51, it was rendered not by חלוה but by חלוה. Thus in Jer 46:19, חלוה is rendered חלוה. Moreover, in all the 15 places in Jeremiah where חלוה occurs we find it rendered by חלוה, and in all 20 Jeremiah passages containing חלוה P' has חלוה - the Ethpeel usually corresponding to the Hebrew Qal and the Peal to the Hiphil. Thus the opportunity of rendering חלוה by its cognate is totally passed over in Jeremiah.

A similar reluctance to use these cognates is met with in Ezekiel: חלוה are always (15 times) rendered by חלוה or חלוה, and חלוה (in all 3 passages) by חלוה. P's treatment of one of these passages is particularly instructive. In Ez 12:3, the prophet is commanded to warn his contemporaries of Jerusalem's impending downfall by leaving his home as if he were going into exile:

which P' renders:

Here the imperative Ethpeel: "be thou captured....." produces an awkward effect which could have been avoided.

1. This has been checked in mss 7a1 and 6h 15.
if the translator had been willing to use $\sqrt{\text{םלע}}$.

In the other prophetic books, $\sqrt{\text{םלע}}$ and $\sqrt{\text{םלע}}$ are not so frequent, but in a total of 22 occurrences, we find in P' the rendering $\sqrt{\text{םלע}}$ 18 times, $\sqrt{\text{םלע}}$ thrice¹, and $\sqrt{\text{םלע}}$ only twice²; for details see Table C.1, which presents the P' renderings of $\sqrt{\text{םלע}}$ throughout the O.T. Again, the translator of Ezra renders $\sqrt{\text{םלע}}$ by $\sqrt{\text{םלע}}$ 10 times, and by $\sqrt{\text{םלע}}$ only once (viz at 8:35, where MT has $\sqrt{\text{םלע}}$ and thus $\sqrt{\text{םלע}}$ had already been used by the translator immediately before).

When we turn, however, to 2 Kings, we find the translator quite willing to use $\sqrt{\text{םלע}}$, which he does in 17 passages, to the exclusion of $\sqrt{\text{םלע}}$. We note that 2 Kings 25 is a parallel narrative to Jeremiah 52; but in translating these two accounts, in very similar language, of the same events, P' in 2 Kings will use only $\sqrt{\text{םלע}}$, and P' in Jeremiah only $\sqrt{\text{םלע}}$. Other books in which $\sqrt{\text{םלע}}$ is employed - though less frequently - and recourse is not had to $\sqrt{\text{םלע}}$, are Lamentations and Esther; Chronicles uses $\sqrt{\text{םלע}}$ five times and $\sqrt{\text{םלע}}$ only once (in a paraphrastic rendering).

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1. The subject is in each case an abstract noun which could hardly have been coupled with $\sqrt{\text{םלע}}$ (Is 24:11, 38:12; Hos 10:5).

2. In Is 49:21 (MT $\sqrt{\text{םלע}}$) and Is 57:8 (MT $\sqrt{\text{םלע}}$).
<table>
<thead>
<tr>
<th>Source</th>
<th>Sentence</th>
<th>Rendering</th>
<th>Otherwise</th>
</tr>
</thead>
<tbody>
<tr>
<td>2 Sam 15:19</td>
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<td></td>
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<tr>
<td>2 Kings 15:29; 16:9; 17:6; 23:26</td>
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<td></td>
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<tr>
<td>17:27; 28:33; 18:12; 24:14; 14.15</td>
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<tr>
<td>24:15; 16:25; 11:21; 27</td>
<td></td>
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<td></td>
</tr>
<tr>
<td>Is 49:1-6</td>
<td>a ad</td>
<td></td>
<td></td>
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<tr>
<td>(Job 20:28 ae)</td>
<td></td>
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<tr>
<td>Lam 1:24;22</td>
<td>a a</td>
<td></td>
<td></td>
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<tr>
<td>Est 6:6; 6.6</td>
<td>a b a a</td>
<td></td>
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</tr>
<tr>
<td>Ezra 8:35</td>
<td>b</td>
<td></td>
<td></td>
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<tr>
<td>Neh 7:6</td>
<td>b a</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1 Chron 5:26;41;6:9</td>
<td>a a a a</td>
<td></td>
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<tr>
<td>Jud 18:30</td>
<td>a a c</td>
<td></td>
<td></td>
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<tr>
<td>Is 5:13; 45:13</td>
<td>a c</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Jer 13:13; 19:20; 22:12</td>
<td>a c b b b b b</td>
<td></td>
<td></td>
</tr>
<tr>
<td>19:20; 22:22; 31:19</td>
<td>a b b b</td>
<td></td>
<td></td>
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<tr>
<td>Ezek 1:23; 11:15; 11:15</td>
<td>a c</td>
<td></td>
<td></td>
</tr>
<tr>
<td>11:25; 42:23; 3:4; 7:11; 25:3</td>
<td>b b b b b b b</td>
<td></td>
<td></td>
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<tr>
<td>33:21; 23:28; 40:1</td>
<td>c a c</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Amos 5:6; 9:15; 5:27</td>
<td>a a c b b a a</td>
<td></td>
<td></td>
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<tr>
<td>6:7; 11; 11:17; 17</td>
<td>a a a a</td>
<td></td>
<td></td>
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<tr>
<td>Obad 20:20</td>
<td>a c</td>
<td></td>
<td></td>
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<tr>
<td>Mic 1:16</td>
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<tr>
<td>Neh 3:10</td>
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<tr>
<td>Zac 6:10</td>
<td>b</td>
<td></td>
<td></td>
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<tr>
<td>11:1; 1;16; 19; 20:21</td>
<td>b b b</td>
<td></td>
<td></td>
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<tr>
<td>9:10; 16; b b b</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2 Chron 36:20</td>
<td>ah</td>
<td></td>
<td></td>
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</tbody>
</table>

N.B: These references have all been verified in Cerini's facsimile of 7a1. Beneath each reference will be found a letter or letters corresponding to the following notes:

- a: MT has a verbal form of the root. b: MT: הָיָה c: MT הָיָה
- d: This is the sense apparently discerned by P', but most authorities would disagree (MT הָיָה).
- e: P' here takes הָיָה in the sense "reveal".
- f: הָיָה is omitted in most Hebrew ms', but it is found in some and was apparently read by P'.
- g: MT הָיָה, but P' must have read הָיָה so G' הָיָה וּמַלּוֹוָּס וָּסָע.
- h: P' expands, rendering מַלּוֹה.
- i: The second occurrence of הָיָה in the verse is omitted in P'; on the first, see col.2.
- j: Here הָיָה denotes the community which had returned from captivity.
- k: This has co-e in from earlier in the verse, where P' apparently read הָיָה (but MT has הָיָה).
What conclusions may we draw? This feature, the use or non-use of יָדַע, seems capable of separative force: it constitutes presumptive evidence that books wherein יָדַע is used freely were not produced by the same translator as those in which the root is avoided and יָדַע used instead. Thus we may form two lists of books, such that we may tentatively assume that no two books divided between the two lists go back to the same translator:

<table>
<thead>
<tr>
<th>Books preferring יָדַע</th>
<th>Books preferring יָדַע</th>
</tr>
</thead>
<tbody>
<tr>
<td>Kings</td>
<td>Jeremiah</td>
</tr>
<tr>
<td>Lamentations?</td>
<td>Ezekiel</td>
</tr>
<tr>
<td>Esther?</td>
<td>Dodekapropheton</td>
</tr>
<tr>
<td>Chronicles</td>
<td>Ezra</td>
</tr>
</tbody>
</table>

We may ask whether the use or non-use of a cognate could also be regarded as a linking feature: that is, does the evidence tend to prove that all books belonging to the same list have a common origin? Only exceptionally would we be justified in drawing any such inference from community in willingness to use the cognate, because that is the behaviour which we may expect any number of Syriac translators to exhibit independently. A case could be made out, however, for regarding as a linking feature the avoidance of the cognate in favour of a particular alternative,

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1. We can only speculate why certain speakers of Syriac could have avoided יָדַע "go into exile". Perhaps the reason lies in its homonymy with יָדַע "reveal"; we can easily imagine a speaker finding יָדַע ambiguous and preferring יָדַע. In the same way, I have encountered English speakers who avoid using the adjective "funny", so that the hearer need not ask the follow-up question: "Funny peculiar or funny ha-ha?".
under certain circumstances. However, we must remember that two different translators could easily decide not to employ the cognate - e.g. if that Syriac word were ambiguous or somewhat recherché and not likely to be immediately intelligible to the intended users of the translation - and could easily hit on the same substitute independently. Thus it seems advisable to draw no linking inferences from the treatment of a single word; but if consistent patterns were to emerge from investigations of several words, a division of the O.T. books in P' according to authorship could be attempted.

The next step, then, should be to carry out a score, say, of similar investigations of Hebrew words possessing a Syriac cognate. Unfortunately, however, the identification of appropriate Hebrew words is far easier said than done. שִׁמְעֵה II was particularly suitable, for several reasons. It occurs fairly frequently, in several O.T. books. It does not undergo, in different contexts, any appreciable variations of meaning, which might have invited the translator to vary intentionally his choice of Syriac equivalent. Without the help of a concordance, such words are exceedingly difficult to identify, and so I have had to leave the task unfinished. Only one other word study seems worth reporting, on the rendering of the Hebrew לֹא , to which corresponds a Syriac cognate לֹא . The renderings in P' of this word are1:

1. These references have been verified in 7a1 (in facsimile)
<table>
<thead>
<tr>
<th>Verb</th>
<th>Reference</th>
<th>Notes</th>
</tr>
</thead>
<tbody>
<tr>
<td>לְפָהֵל</td>
<td>Job 7:16, 9:29, 21:34; Qoh 1:7 et passim</td>
<td></td>
</tr>
<tr>
<td>לְפָהֵל</td>
<td>2K 17:15; Jer 25:10-15, 16:19, 51:18</td>
<td></td>
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<tr>
<td>לְפָהֵל</td>
<td>Ps 39:6-7, 12, 62:10a, 94:11, 144:4</td>
<td></td>
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<tr>
<td>לְפָהֵל</td>
<td>Is 30:7, 49:4?, Zach 10:2, Ps 78:33</td>
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<tr>
<td>לְפָהֵל</td>
<td>Ps 62:10b, Prov 31:30</td>
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<tr>
<td>לְפָהֵל</td>
<td>Job 27:12, 35:16, Lam 4:17</td>
<td></td>
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<tr>
<td>לְפָהֵל</td>
<td>Prov 21:6</td>
<td></td>
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<tr>
<td>לְפָהֵל</td>
<td>Prov 13:11</td>
<td></td>
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<tr>
<td>לְפָהֵל</td>
<td>Is 57:13</td>
<td></td>
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<tr>
<td>לְפָהֵל</td>
<td>Dt 32:21, 1K 16:13, 26, Jer 8:19, 10:8, 14:22, Jonah 2:9, Ps 31:7</td>
<td></td>
</tr>
</tbody>
</table>

1. This is possibly the rendering in Is 49:4, where becomes simply לְפָהֵל.

2. This is the equivalent of the plural of לְפָהֵל, whenever it means "false gods." Otherwise is used only in Qoheleth (4 times, where it is rendered לְפָהֵל or לְפָהֵל).
Only four books are here represented fully enough for us to speak with confidence of the policy exhibited therein for rendering this word, viz

(a) Jeremiah, where נֶעְלָה occurs 5 x not used
(b) Psalms, where נֶעְלָה appears 6 x (and מִפְּלָה once) used
(c) Job, where we have מִפְּלָה 3 x but מִפְּלָה is preferred as an adverb used
(d) Qoheleth, where מִפְּלָה is consistently employed

Thus there is a prima facie case for supposing that Jeremiah and Psalms in P' are the work of different translators, neither of whom is identical with the translator(s) of Job and Qoheleth. (To deduce that the two latter books go back to a single translator would of course be quite unwarranted.)

I have examined the renderings of several other Hebrew words having Syriac cognates, but I have not found any others to give meaningful results, mainly because the translators seem to have felt that these words varied in meaning between one context and another, so that it is not clear to what extent differences throughout the O.T. in the choice of Syriac equivalent are due to different translators, and to what extent they are conditioned by shifts of meaning according to context. To identify and to research the

1. Among these unsuccessful attempts were נָא, מִפְּלָה, רֹזֵר, לֶאָשָׁת, רֹזֵר, אָלֵלָה, אָלֵלָה

These I list so that any future worker need not duplicate my effort.
treatment of a number of suitable words sufficient to yield reliable results on the authorship of P' will require— at least in our present state of knowledge—a great investment of effort. We look forward to the completion of a Peshitta concordance, by means of which, studies on these lines may come to shed light on the long-unsolved problems of the origin of the Peshitta.

§ II. REMARKS ON THE PSYCHOLOGY OF TRANSLATION, AS EVINCED IN P'

The purpose of this section is to consider certain ways in which an element of MT, and its rendering in P', may be related. The most usual way of rendering a Hebrew word into Syriac is, of course, to attempt to reproduce its meaning, within the limits set by the resources of the language and the competence of the translator. There seem, however, to be many instances of renderings which cannot be regarded strictly as attempts to a proximate the Hebrew original, and which tend to bear out Frankl's observation (p. 500): "Ueberhaupt sind unsere Versionen sehr häufig mit dem Maßstabe psychologischer Gesetze zu messen. Die subjektive Ideenverbindung überwiegt über die objektiv richtige Auffassung." It is with renderings of this sort that we are concerned here. But before speculating further about the causes underlying these renderings, let us consider the evidence itself.
We sometimes find that P' renders a Hebrew word by a Syriac word which has a similar sound but a different meaning. This was noted by Frankl, who termed it "Syromanie" (p. 501); investigators of other P' books have also observed it. In Jeremiah we find the following examples:

2:20

23:19

Note that in the parallel verse 30:23, (ה) becomes נלע.

46:18

On this passage, see Frankl, p. 502.

47:2

This too was noted by Frankl (p. 546).

49:29

50:21

1. e.g. Albrektson (cf. p. 9:52), p. 211: "he [the P' translator] sometimes renders a Hebrew word by a phonetically similar Syriac word with a different meaning." See also the remarks of Gerleman quoted below (pp. C:15f).

2. These readings have been verified in 6h 14 and 7a1. They have also been checked in 7h 8 where that ms is extant (23:19, [30:23]).
How are we to account for such translations?

It has long been acknowledged that G' occasionally renders a Hebrew word by a phonetically similar word in Greek (e.g. רְכִּית→δρέπανον in 1 Sam 13:21); a list of these was drawn up by Prof. S. R. Driver.¹

But in P' - at least within the book of Jeremiah, and presumably in other books too² - this phenomenon seems considerably more frequent. An attractive explanation was proposed by G. Gerleman, who believed the close affinity between Hebrew and Syriac to be conducive of mistranslations of this sort. Gerleman's remarks deserve to be quoted at length³:

³A translator working with two


2. One example from P' is given by Prof. J. Barr, "Comparative Philology and the Text of the Cld Testament", Oxford 1968, in the section (pp. 262-5) entitled "Uncertainty about the Meaning of the Version". On p. 265 he notes, at 1 Sam 20:13, MT מָמַלְתִּי אַלְּ-אָבִי תַּחַנֶּה יְלִיְדִי P' מִי יִתְּבַע הַלַּאִבִי נָחַת יְלִיְדִי (from Heb. "ask", hence "ascertain"?)

But the main point with which Barr is there concerned is a rather different one: Suppose that T' renders a Hebrew word by a phonetically similar Aramaic word which is not found, or is very rare, in Aramaic outside T' itself. (An analogous problem can of course be posed for P'.) Then we have to decide whether (a) T' has coined an Aramaic word based on the Hebrew (Barr uses the term "calque"), which can have no independent value for determining the meaning of the Hebrew, or (b) the T' word is a genuine cognate, which can be legitimately invoked for that purpose.

3. "Zephanja, textkritisch und literarisch untersucht," Lund 1942, p. 89. As this quotation is rather long, I have translated it.
closely related languages runs the risk of confusing a word in the original which he has only superficially understood, with a phonetically or graphically similar word in the other language. The Syriac translator has sometimes given up the attempt to carry over the meaning of the word and, for a given sequence of letters in the Hebrew, has merely substituted a similar one in Syriac. The translator had both heard and read the word correctly, but its exact sense was not grasped by his conscious mind, and instead of actually translating it he dressed it up in Syriac guise. These renderings, then, constitute a sort of folk etymology. The cognation between the two languages naturally made it very easy for the translator to attach to a Hebrew word any of the possible associations evoked by Syriac words that were comparable in sound. Gerleman, then, saw the explanation in terms of the psychology of a translator who is working with an original written in a language cognate with his own. Let us now go on to see whether there are other types of rendering which may be accounted for on similar lines.

A second class of translations may be considered here: those wherein a difficult Hebrew phrase has been rendered in such a way as to yield not a false sense but no tolerable sense. Each word is translated, but the phrase as a whole is scarcely intelligible. This is not quite the same as

1. It is not of course uncommon that a translator produces virtual nonsense. Thus M. Flashar, "Exegetische Studien zum Septuagintapsalter", in ZAW (1912), pp.81-116, 161-169, 241-268, states (p.94) that the G' translator "da, wo er seine Vorlage nicht verstand oder nicht verstehen wollte, einfach mechanisch Wort für Wort übersetzt und es dem Leser überlassen, einen Sinn aus den Worten heraus zu finden". What interests us is to see just where these unintelligible renderings appear.
being slavish, for although in such passages the lexical items are rendered literally, grammatical features are not always respected; thus in the passage from Jer 50:5 quoted below, the word order has been considerably changed. In many cases, but not in all, the Syriac equivalents found in these atomistic translations are phonetically similar, or even truly cognate, to the words of the Hebrew original. For each of the examples which follow I have tried to bring home the obscurity of P' by translating it into English. In order to obtain some idea of how these renderings were explained, I have consulted: (1) the commentary attributed to Ephraim in the Roman edition of his works; (2) the Aušar Rāzē of Barhebraeus, from the ms Bl. Add. 21580; (iii) the Latin translation offered in the London Polyglot (ed. Walton).

3:5

"Will it be kept for ever? or will it be kept for ever?"

Both Ephraim and Walton supply a subject, viz הָיֶשֶׁת and pertinacia tua respectively.

1. These have been confirmed in 6h14 and 7a1, and where possible (6:11 only) in 7h8.

2. But it cannot be regarded as an undoubtedly genuine work; see Thes., p. 829.
6:11

... and thou hast been filled with the wrath of the Lord, and thou art weary. Measure!

and pour out on children in the streets....

Walton improves the sense somewhat by rendering by *exhauri*, a meaning which of course suits but does not seem to be attested elsewhere.

38:11f. Here, Zedekiah's servants are trying to pull Jeremiah up from a miry pit, and they attach cords to some rags which he is told to put under his armpits. These rags are, in MT, 

*גַּלְוָ(א)י* P renders *אֶחְשַׁמְוּ* which echoes the sound of the Hebrew but can hardly have been very informative to the Syriac reader.

Barhebraeus comments:

*גַּלְוָ(א)י* (the end of the last word is doubtful in the ms). Walton offers *pannis quibus terguntur iumenta* in V.11 and *peniculó tarsorícos* in V.12.

---

1. On this passage see Frankl, who was however concerned only with the fact that P uses the 2nd. pers. sing. fem.
they have lifted up their voice; from Zoar unto Horonaim, and unto the town of Eles (?), a three-year-old heifer" Walton is content to translate P' word by word. "... et usque ad urbem Eles vitulam trimam".

"on the roads of Zion they will ask with their faces"

The Ephraim commentary seeks to make sense of P' by expanding:

Walton too expands. De viis Sionis interrogaabant in facies suas provoluti

"we healed Babylon, and she was not healed".

1. It is intriguing to find this fleeting allusion to a town called Eles (I follow the vocalisation of Payne Smith, col. 211). In the Thesaurus there are a few other references to the n. pr. loc. Eles, but we may doubt whether they all represent the same place. This name also appears in the Syrohexaplar (ed. Ceriani), which has here ἔλες γενέσθαι; this must represent a Greek text similar to that of many Lucianic witnesses, viz ἀγγελιάς εἰς εἰς ἀγγελιαν, which Ziegler (in the Göttingen Septuagint, vol. xv, 1957) regards as a corruption of Ἀγέλα Ἀλασία. Thus the reference in P' seems to go back to a G' text which exhibited that corruption. It is uncertain whether an interpolator was responsible, or the translator himself consulted G' in a desperate attempt to make sense of the text; I am myself inclined to favour the latter possibility.
the conclusive force of the Piel in MT being lost in P'. Walton renders: Curavimus Bäbelem, et non est sanata. This translation makes better sense than the original, because *curo* — unlike *טנא* — may mean not only "heal" but also "attend to".

Thus we have to recognise that P' is sometimes quite obscure. One is often given the impression that although the sense of MT is not always clear, once we turn to the versions we do at least know what they mean. Thus the Vulgate is, in general, intelligible and meaningful Latin; but P' does not deserve to be characterised throughout as intelligible and meaningful Syriac. Despite the name *רַבְרַבָּכָה*, P' sometimes offers a text which itself requires exegesis; and we may ask how it came about that the translator, whose aim surely was to make the biblical text intelligible to a Syriac audience, allowed such renderings to stand in the translation he offered.

There is another phenomenon to be considered, wherein the P' translator seems to commit what logicians term the Fallacy of the Ambiguous Middle. Let the reader consider the following example. We read in Jer. 20 that the priest Pashhur afflicted the prophet, who then told him that his name would no longer be מְגַנֵר מְסַכְּבַר מָשְׁאָבָר "terror on every side" (V.3). For this, P' has גְּרוֹזָה גֶּהֲנָה (6h 4, 7a1, 7h8) "a sojourner and a beggar". That מִגְנָא was derived by P' from תָּרָא "sojourn" is obvious enough; but how do we
explain  בָּנַח ? It seems that מָזָּה was associated with בָּנַח "surround"; and although the development shown in בָּנַח "one who wanders about, a beggar" is not paralleled by the Hebrew מָזָּה, this word - in a sense covered by the Syriac rendering but not by the original Hebrew word - came to stand in P'. The process may be represented thus:

\[
\begin{align*}
\text{מָזָּה} & \rightarrow \text{בָּנַח} \\
\text{בָּנַח} & \rightarrow \text{בָּנַח} \\
\text{בָּנַח} & \rightarrow \text{בָּנַח}
\end{align*}
\]

The ambiguous middle term then is מָזָּה.

Another instance is Jer 23:27, where we have

MT יְָּשַׁבֶּהָ מִלְּתֵּנִי מִלְּתֵּנִי אֵלֶּה, אֱ-עִם, יַעֲקֹב

P' יְָּשַׁבֶּהָ מִלְּתֵּנִי מִלְּתֵּנִי אֶלֶּה לָכֶם

"who seek to mislead My people in My name"

The ambiguity here lies in מָזָּה, which can mean both "forget" (as in MT) and "go astray". Thus the process whereby P' reached this translation is

\[
\begin{align*}
\text{יְָּשַׁבֶּהָ} & \rightarrow \text{יְָּשַׁבֶּהָ} \\
\text{יְָּשַׁבֶּהָ} & \rightarrow \text{יְָּשַׁבֶּהָ} \\
\text{יְָּשַׁבֶּהָ} & \rightarrow \text{יְָּשַׁבֶּהָ}
\end{align*}
\]

1. Compare the later Hebrew לָכֶם.

2. A different explanation is suggested by W. Rudolph in BHK, namely that P' misread לָכֶם, which is however not an obvious misreading for לָכֶם.
In Jer 49:10, the meaning of MT is reversed by such a P' rendering:

MT:

יָגַנְתָּהּ לָא יָבֵכֵל

P':

יָגַנְתָּהּ L'כַּל hבֵל (6h14, 7a1; hiat 7h8)

In MT, Edom - against whom the oracle is directed - cannot hide; according to P', he is hidden and cannot be found. Once again P' has used a Syriac word (אֲזַרְצָא) whose meaning overlaps with that of the Hebrew word (לִהְבָּל), in a sense which is not carried by the latter. That is,

be able to find

In the above passages it can hardly be doubted that, of the two meanings of which the ambiguous Syriac word was capable, the one which the translator intended was not that of MT. We may compare two other passages wherein P' has a rendering which may be accounted for in terms of this ambiguity but can be otherwise explained.

At the end of Jer 13:27, MT offers

The last three words are difficult but not completely unintelligible (AV: When shall it once be?) They are rendered in P' יָבֵל אֲזַרְצָא 1: "How much

1. This is the text (with Yodh at the end) of Lee and of all the mss I have consulted (6h14, 7a1, 7h8, 11d1).
longer? Repent!". Now יִזְכְּרוּ נֶאֶרֶךְ יָדוֹ is often translated in P1 by the adverb יְבַיֵּשׁ "again", hence we could suppose that the ambiguity of יְבַיֵּשׁ brought about this translation:

\[
\text{again} \quad \text{again} \quad \text{repent}
\]

But it is perhaps more likely that P1 translated word for word, originally reading יְבַיֵּשׁ, and that the obscurity of P1 ("How much longer again") caused יְבַיֵּשׁ to be corrupted, very early on, to יְבַיֵּשׁ{superscript}12.

The other case is at Jer 48:35. There MT has יְבַיֵּשׁ he that offers a sacrifice at a hill-shrine: P1 יִזְכְּרוּ נֶאֶרֶךְ יָדוֹ (so 5614, 701). Now יְבַיֵּשׁ has two meanings: (a) alter-reasonably close to יְבַיֵּשׁ; (b) sacrifice. Since in this passage it is not preceded by a preposition, most readers would naturally have thought of (b). So Walton (St abeolebo.) offersentem victimas. If (b) was intended by the translator himself, then we have once more a situation analogous to that in Jer 2015 etc. However it seems more likely that the translation in his mind was (a), and that he followed the Hebrew in not introducing any preposition before יְבַיֵּשׁ, so that his translation was tolerably accurate but liable to be understood in a manner which he had not envisaged.

A noteworthy instance of this ambiguity is furnished by Jerome’s treatment of Is 2:22. There MT reads יִזְכְּרוּ נֶאֶרֶךְ יָדוֹ and AV translates: "Cease ye from man... for wherein is he to be accounted of?" In certain Jewish quarters, however, [YAH] was taken to refer to Christ, as Jerome testifies and as appears from Kimhi’s comment ad loc. Jerome countered this not by denying the reference to Christ but by vocalising יְבַיֵּשׁ as יְבַיֵּשׁ. This word means of course "hill-shrine", but the usual Latin term is "excelsum", which Jerome felt justified in stretching to "excoelsum". Hence we find the verse rendered in P1:

\[
\text{excoelsum} \quad \text{hill-shrine} \quad \text{excelsum} \quad \text{hill-shrine} \quad \text{excoelsum}
\]

which Jerome explains (P.L., loc cit.) to be a warning against offending Him "qui secundum virum quiem homo sacrum spiritus in narisibus eius quia excelsum reputatus est ipse".

This case differs however from those considered in connection with P1, in that the translator was well aware of the obvious meaning of the Hebrew6, but chose deliberately to translate otherwise. 

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1. It may be remarked that יְבַיֵּשׁ יִזְכְּרוּ נֶאֶרֶךְ יָדוֹ is actually the reading found in SyroHex.

(Cod. Ambros).

2. P.Volz, "Studien zum Text des Jeremia" [Beiträge zu Wiss. vom A.T., I.25], Leipzig 1920, pp. 121 f., appeals to P1 in support of the emendation יְבַיֵּשׁ for MT in Jer 13:5, 14:1. He is supported here by W.Rudolph in BHK (ed.,). However, P1’s rendering here can hardly be called admissible evidence.

3. See the opening words of the passage quoted in n.5.

4. "Verbum Hebraicum BAMA, vel Οὐκομά, dicitur, id est, "excelsum", quod et in Regnorum libris et in Eschileae legimus: vel certe "in quo", et elaeem litteris scribitur BETH, MEM, RE: so pro locorum qualitate, et voluminus legere, "in quo", dictum est BAMA; sin autem, "excelsum" vel "excelsum", legimus BAMA." See his Commentary on Isaiah ad loc. (Migne, F.L. xxiv, col. 55). For the continuation of this passage, see n.5.

5. It is interesting that Jerome himself uses this very term: "Intelligentes ergo Judaei prophetiam esse de Christo, verbum ambitum in deteriorem partem interpretati sunt, ut evidentur non laudare Christum, sed nihil pendere".

6. For example, he acknowledges that it was so translated by Aquila.
All the renderings considered so far (with the possible exception of the last) have one thing in common: they indicate that P often translated words rather than ideas, and did not always keep track of the meaning of the original, or of his translation, or of both. In this respect his work is deficient by the standards set up by modern scholars. As Prof. Leonard Forster remarks: ¹ "Before we can convert symbols from one language into another the original symbol must be understood. An act of comprehension must come first, and that is not by any means always the case in one's own language, let alone a foreign one". Ideally, the aim of a translator must be to appreciate fully the meaning of the original, and then to re-create the effect thereof in the "target" language; translation then becomes an activity which occupies quite amply the intellectual powers of the translator. But the P translator evidently did not always work in that way; at times he seems to have translated without really thinking out what he was doing. Yet he had a considerable knowledge of Hebrew, Frankl calls him, deservedly, "nüchtern" and "besonnen" (p. 501); and it could well be urged that he was perfectly capable of translating correctly many of the passages wherein what he in fact produced is false or unintelligible. This can be deduced from an examination of parallel passages. Thus 23:19 is

is virtually identical to 30:23 in MT; in the former, P' mistranslates (תל) by יִֽלָּֽה, but in the latter he has דָּלֵל (see p. C:14). Again, in 20:3 the word מַעְרֵד כְּפֵנִים, in the phrase מערד כפינים, is wrongly translated מדכע; but in V.10, the same Hebrew phrase occurs again, and there P' has the satisfactory rendering זַע נַע. Why do we find P' sometimes departing from his usual competence to yield translations which bring to mind the schoolboy "howler"?

We have, then, to account for three classes of renderings (pp. C:14, 16, 20). We have seen that the first was attributed by Gerleman to the psychological effects of working on translation between two cognate languages; and I would suggest that the other two can be explained on the same lines. Let us consider the matter in greater detail. The respective structures of Hebrew and Syriac are so similar that it is possible, for much of the time, to obtain an adequate translated by mechanically replacing each Hebrew word by a Syriac one, frequently a cognate; examples have already been offered (p. C:4). Thus the translator's work became to some extent automatic; and the dangers inherent in executing one's work mechanically may be appreciated if we consider the psychology of a copyist, whose task is of an even more mechanical nature. On this subject, Froger\(^1\) observes: "Le travail de copie est monotone; il engendre une sorte

---

de torpeur. Devenu mécanique par la répétition des mêmes actes, il n'absorbe pas entièrement l'attention du copiste et laisse une partie de son esprit s'abandonner à la rêverie". The scribe comes to work in a "demi-sommeil" (p. 13), all too often without real sing it and making allowances for it. The effect will be, of course, that he commits errors of transcription. It seems that the P' translator too fell into something of a "demi-sommeil". In his case, the result was that his critical powers were temporarily deadened, so that he sometimes lost sight of the meaning, and, without really thinking, rendered a Hebrew word by a Syriac word which is more or less obviously associated with it but does not satisfactorily translate it in its particular context. That he allowed these renderings to stand in his translation is due to this induced lethargy. This is, essentially, what underlies all the three classes considered above; and for the most part, I submit, these renderings were not truly intentional.

Once we recognise that some of the translator's work stole out, as it were, under the threshold of his consciousness, then some interesting implications follow. One which we shall examine here concerns Syriac lexicography.

It is well-known that the Hebrew יָֽדֶּשׁ "to return" may be used with two very different meanings: (1) to repent, (11) to apostatize. The noun יָֽדֶּשׁ is used in the second sense only, as the context in each case makes clear. It is
interesting therefore to find, as Th. Sprey pointed out\footnote{1}, four passages wherein $P'$ translates it by יִֽשָּׁן, which is of course very frequent with the meaning "repentance". The four places are:

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<th>Verse</th>
<th>Hebrew Text</th>
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<tr>
<td>Jer 2:19</td>
<td>נַשְׁנֶהֽ</td>
<td>(6h14,7a1)</td>
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<tr>
<td>Jer 3:22</td>
<td>אֲרַךְ מַשְׁנֶהֽ</td>
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<td>Jer 8:5</td>
<td>מִשְׁנֶה נְבָעַת</td>
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<tr>
<td>Hos 14:5</td>
<td>רָאשָׁה מַשְׁנֶהֽ</td>
<td>(7a1)</td>
</tr>
</tbody>
</table>

As Sprey observes, these passages in $P'$ are hardly intelligible if we take יִֽשָּׁן in its usual sense. He therefore praises those Syriac lexica\footnote{2} which admit, on the basis of these passages, a negative meaning for יִֽשָּׁן; and he advocates that this meaning be added to other dictionaries.\footnote{3}

It would seem then that Sprey is justified in calling for recognition of a negative meaning of יִֽשָּׁן. Although his note dealt only with the noun יִֽשָּׁן, a similar problem arises with the adjectives בֹּקֶשׁ and בֹּקֶשׁ "wayward", which $P'$ renders יִֽשָּׁן in Jer 3:14,22; 31:22.\footnote{4} In general: יִֽשָּׁן

\footnote{1}{"shall—sinned", in Vetus Testamentum (1957), pp. 408-410.}
\footnote{2}{viz R. Payne Smith's Thesaurus, and J. Payne Smith's Compendious Syriac Dictionary.}
\footnote{3}{such as J. Brun (Beirut 1911), C. Brockelmann (Halle 1922, 2nd ed.). Sprey also mentions the Syriac-Arabic lexica of bar 'alil and bar bahlul.}
\footnote{4}{So 6h14,7a1. Elsewhere we find יִֽשָּׁן (Is 57:17 - a guess?) and יִֽשָּׁן (Jer 49:4. NT לְשָׁן but $P'$ יִֽשָּׁן so 6h14,7a1, Lee. Did $P'$ read יִֽשָּׁן? - an adjective not found in the Bible but common in later Hebrew? Cf V' falla delicata. Or is יִֽשָּׁן a corruption from יִֽשָּׁן badly written as something like יִֽשָּׁן ?). In Mic 2:4, בֹּקֶשׁ is taken neutrally (= "give back").}
means "penitent", but the Thesaurus Syriacus gives the meaning "apostatans" for these three passages (and for no others); the usual sense of מלח is hardly appropriate, at least in Jer 3:14,22. Again, if we take the matter to its logical conclusion, the verb מלח itself may be assigned a possible negative sense, because the Hebrew תושב is sometimes used unfavourably and rendered in P' by מלח. Thus in Jer 8:4, MT offers

ומלא תושבו España יושב רלא תושב

It is generally held that the prophet is playing on the polysemy of תושב, the first תושב being meant unfavourably and the second favourably. Thus G' הזע"ק ש דברים ouk ἐκπορεύεσθαι AV "shall he turn away, and not return?" Otherwise it is difficult to make sense of MT. Now P' renders

ניאם ותל השמים איה תושב (so 6h14, 7a1, 7h8)

If we consider P' to be truly meaningful, we shall act inconsistently if we do not suppose that תושב here exhibits polysemy precisely analogous to that of תושב; we shall then translate P': "will they fall and not arise? and if they turn back, will they not repent?"

1. There is no difficulty in understanding the text as a question; no interrogative particle is required (Nöldeke §331 A). Walton, however, takes P' quite differently, as we shall see below.
These extensions of meaning of derivatives of שָׁלַש do solve problems, in that they render פ' more intelligible; but they create problems of their own. The first is the fact that the commentaries of Ephraim and Barhebraeus never - to my knowledge - make it clear that they have taken these words negatively; on the contrary, sometimes their comments suggest that they knew only the positive meaning. Thus on Hos 14:5, where פ' has בְּֽאָשֶׁר בֵּין, Ephraim notes:

"..." this seems to indicate that Ephraim explained פ' by construing בְּֽאָשֶׁר as a collective noun. Similarly, Barhebraeus comments on Jer 2:19 (פ' בְּֽאָשֶׁר בֵּין) as follows: "..." which starts out from a favourable interpretation of בְּֽאָשֶׁר. The fact that these native commentators were apparently unaware of a negative meaning counts against - though it does not disprove - the supposition that בְּֽאָשֶׁר was capable of being used unfavourably and was employed by the translator to that end.

A second problem lies in the fact that homonymy can impede communication. If a word is capable of two meanings, then the hearer may not always find it easy to ascertain which one is intended by the speaker; to what extent this fact will be a real source of confusion, will depend on the nature of the homonyms. 1 This must serve to discourage us - though not

1. Thus, to take an example from modern English, the homonyms "bat" (cricket) and "bat" (animal) can coexist because one can hardly imagine a context in which they might be confused. But homonymy between two technical terms relating to theology, such as "apostasy" and "repentance", words that are liable to occur in similar contexts, would have presented a grave obstacle to communication.
necessarily to prevent us in any given case - from postulating the existence of homonymy. An illustration is provided by the P' text of Jer 8:4 f; if we believe in the proposed negative meanings of מַכְבָּּּשָּׁא etc., then the reader of P' must be supposed to have accomplished no mean feat of mental acrobatics:

MT

"And thou shalt say unto them: Thus saith the Lord. Will they fall and not arise? and if they turn back (neg.), will they not repent (pos.)? (But Walton translates: Cadent et non resurgent; et licet poenitent (pos.) eos, non

P'

1. This point is discussed by Prof. J. Barr (op. cit, Ch. VI) in connection with the creation of homonyms in philological treatments of the Hebrew of the O.T.

2. So read with 6h14, 7a1, 7h8, rather than רָאָיָא (Lee).
tamen poenitet (pos.) illos'). Why has this people returned (neut.) to Jerusalem? (It is) insolent apostasv (neg.). They hold fast to deceit, and they refuse to repent (pos.)

These repeated changes of meaning are indeed problematic; but there is, I believe, an acceptable alternative. The reason for which a negative meaning for $\chi\alpha \omega \eta$ etc., was first proposed, is that P' would not otherwise make sense. But we have seen that it is by no means exceptional for P' not to make sense, especially when the translation is virtually word for word. The translator who produced the renderings listed on pp. C:114 was surely capable of putting $\chi\alpha \omega \eta$ for $\nu\beta\mu\alpha\nu\alpha$, simply because he knew the words to be cognate; whether they corresponded in meaning, and whether his translation of the verse as a whole was intelligible, were secondary questions. It follows that, in general, if the case in favour of identifying a new meaning for a familiar Syriac word consists in essence only of the plea that P' would otherwise be unintelligible, then we do not

1. This is presumably meant as an oxymoron: "If they repent, they do not (truly) repent"; compare the Hebrew of Jer 48:33 $\znu\nu\nu \zeta \zeta \zeta$ "the shouting is no shouting". But this interpretation seems far less fitting for a verb ($\nu\omega \chi\eta\lambda$) than for a noun (such as $\zeta\zeta\zeta$), and in this context it is indeed forced.
yet have sufficient grounds to justify the introduction of this inferred meaning into our lexica. 1

1. It is interesting to compare the word ידועה used in T' (Jer 5:6, 14:7, 31:22) to render הקורב (or: הקורב ). This word is well attested with the meanings "answer" and "repentance", but neither of these meanings is appropriate:

5:6 MT

הִיָּה הָעָישִׁים יִשְׂמָעֵהשָׁם"הָעָישִׁים יִשְׂמָעֵהשָׁם"

T' אָזֶר מִשְּמִירֵי הַקּוֹרֵב הַיוֹרְבִים

14:7 MT

כִּי רָבָּהּ מַשָּׁנֶיהָרָבָּהּ מַשָּׁנֶיהָרָבָּהּ

T' סְפִּיאָה הַיוֹרְבִים קֶרֶם הַנּוֹם

31:22 MT

עֲרַמְתָּא חָסַפְתָאָה הַנּוֹם יִדְעָה

T' עֲרַמְתָּא חָסַפְתָאָה לָמֵתָא יִדְעָה

Hence Levy assigns to the word, in these three passages alone, a third sense: "Wiederkkehr auf den sündhaften Weg, Entartung". This identification of a negative meaning would at first seem open to analogous objections; but I believe it to be sound. For, in 31:22, MT has an adjective, and the fact that T' has employed the noun ידועה shows that the word was chosen deliberately, not merely written down mechanically. Moreover, the resulting homonymy between "repentance" and "apostasy" would not impede communication, because the former is found only in the singular (which is natural enough for an abstract noun) whereas in all the three passages where Levy proposes a negative meaning, ישיבת is evidently plural. In English too we find that the singular and plural forms of an abstract noun may bear different meanings without risk of confusion; thus "manner, manners", "attention, attentions".
A similar problem arises in connection with the Hebrew \( \sqrt{\text{יָרָה}} \), which signifies excitement, either in terror or in rage. \( \sqrt{\text{יָרָה}} \) often translates by using \( \sqrt{\text{יָרָה}} \). The Syriac root means "to be angry"; the only translation offered in Payne Smith’s Thesaurus for the Peal is "iratus fuit" (plus a few synonyms); similarly the Aphele \( \sqrt{\text{יָרָה}} \) means only "ad iram provocavit", and the noun \( \sqrt{\text{יָרָה}} \) is given as "ira", which has occasionally developed to "plaga". In particular, there is no evidence that \( \sqrt{\text{יָרָה}} \) can denote terror – except for the following passages, where MT has \( \sqrt{\text{יָרָה}} \) and the meaning "be angry" gives no satisfactory sense.¹

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¹. These references have been verified in 7a1.
It would be easy enough to make sense of \( \sqrt{\text{fear}} \) by supposing that is capable of a second meaning "fear". But I can find no evidence to corroborate this, outside the problematic passages themselves, and so we would be overstepping the mark if we were to add such a sense to our Syriac dictionaries.

Thus we see that psychological factors have to be borne in mind when we study \( P' \), either for its own sake or as a tool in the textual investigation of the Hebrew.

§ III "DRUDGE" WORDS

There were many passages, as we have already seen, where the translator was not sure of the meaning of a Hebrew word. In many of these, he employed a "drudge" word - a Syriac word that is used quite frequently in the translation, to render rather a large number of different Hebrew words, many of which are obscure. The drudge word does not provide an accurate translation
of all the Hebrew words which it repeatedly enters; it seems rather that this word was repeatedly pressed into service when the translator met a Hebrew word which he did not know and for which that drudgery word seemed an appropriate translation, yielding a reasonable sense.¹

An example in the Syriac Jeremiah is הָלָה, which is assigned two senses in Payne Smith: 1) agitatus est, 2) elanguit. P' appears to use this word to cover a wide range of experiences which he vaguely felt to be unpleasant. Thus we find:

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¹ Such a phenomenon has been observed in other versions. Flashar shewed (pp. 169 ff.) that certain derivatives of מּוֹר (mōr) were each found to correspond to a number of different Hebrew words, within the Psalter; thus מַגְּלוּת renders 11 Hebrew words, and מַמְּלוֹכָה six. Here a powerful theological factor has come into play, as Flashar points out, viz. the central importance, to the translator, of the concept of the Law (מּוֹר). Of more general import is Barr's short discussion (op. cit., pp. 251-3) entitled "The Use of Favourite Words", where it is shewn that a certain word may appeal to a translator, usually without any obvious reason, and that he will then tend to use it wherever he thinks fit, "without concern for the way in which it obscures the difference between the Hebrew meanings in the verses translated". Barr offers a number of telling examples from G' and T' but does not deal with P'.
We may consult the glossaries that exist for the Peshitta to Judges, Kings and Psalms in order to ascertain whether ellation is used as a drudge word in other books too. In Psalms, we find that it is; Techen's glossary shows

1. These have been verified in 7a1, 6h14, and, where possible (9:4; 14:18; 25:16, 27), in 7h8.

2. See p. 10:38, n. 3.
that is found for and also (42:5), (90:10) and (78:63). Yet in Pings, does not appear at all, according to Rosenwasser; and Lazarus on Judges mentions it only once (4:21, MT ). This shews that some translators found a useful "multi-purpose" rendering, whereas others got by without employing it at all.

A concordance of the whole Peshitta O.T. would of course be of enormous advantage in the identification and study of drudge words. Techen's work on the Psalter allows us to pick out certain drudge words, e.g.

- to render 18 different Hebrew roots (see Techen, p. 303)

- to render 12 different Hebrew roots (see Techen, p. 321)

An interesting drudge word - of a particular sort - is the verb . This is used as a vague rendering six times - for six different Hebrew words - within the limited area of Ps 35-39:

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1. For an interesting consequence of the use of and of the verb , as a drudge word, see p. 8:51, n.1.
Outside the compass of these five Psalms, מַפְרָצָה occurs twice only.¹ It might be inferred that these Psalms in P' are the work of a translator who had a particular fondness for מַפְרָצָה and that the rest of the Peshitta Psalter was not translated by him. A likelier explanation, however, is that the translator suddenly and unpredictably formed a liking for מַפְרָצָה, which he just as suddenly abandoned. Thus מַפְרָצָה may be viewed as a temporary drudge word.

The investigator of the Hebrew text of the O.T. will be saved from various pitfalls if he is aware of the identity

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1. viz 107:29 (MT שֵׁלֶש) and 125:3 (MT שָׁלֹשׁ).
of drudge words. For example, if we were to contemplate emending ריב in Ps 35:25 to ריב, we could hardly invoke the reading ריב in P as evidence that ריב stood in P's Hebrew exemplar. Again, Barr has demonstrated that the use of drudge words must be borne in mind when one attempts a philological treatment; thus in Hab. 3:6, the G rendering סאלאסונ for ריב has been taken as evidence for a Hebrew ריב cognate with Arabic סאלאסונ 'be violently agitated', but Barr objects (p. 252) that סאלאסונ is a favourite word in G, and that its use here need not indicate special linguistic knowledge.

IV. EMENDATIONS

The following emendations have suggested themselves during my study of Jeremiah 46-51:

46:6 MT

Here בה is not translated, and seems a gratuitous addition. Let us therefore read instead "in the north".

1. This is not, of course, put forward seriously as an emendation of MT; it is simply an example made up to demonstrate a methodological point.

2. These readings have been confirmed in 6h14, 7a1 and 11d1.
The adjective שָׁמוּר "adorned" is somewhat incongruous (Payne Smith translates the phrase as 'vaccia picta'); moreover it corresponds neither to מִשְׁמַר nor to anything else in MT. It seems to me to be a corruption of הַנַּחַל. Perhaps P' originally kept the Hebrew word order and rendered הַנַּחַל. Because it was placed after its predicate — which is somewhat uncommon in Syriac — may have become corrupted; and then, in order to supply a subject, a scribe may have added הַנַּחַל, this time in the usual position.

P' may be explained if we suppose that the translator took מֹנֵק as a proper name and rendered מֹנֵק מָסִים. This will then have been corrupted, in the direction of greater familiarity, to מֹנֵק מָסִים.

The Pael מֹנֵק means "to render subject", but it is not at all common; Payne Smith notes only one occurrence in Syriac literature (as opposed to lexica). Moreover, it does not correspond at all closely to MT. Perhaps, therefore, P' originally read מֹנֵק מָסִים "make him drunk". The previous word is מֹנֵק, and a corruption
from אָבָא to אָבָא
could easily have occurred. Note especially the possibility of haplography of the repeated Aleph (cf. p. 2:12).

Three further emendations, each concerning the interchange of Dalath and Resh, are proposed by Frankl (p. 555).

§ V. CONCLUSION

It is fair to say that research into P' has for a long time been concentrated on textual criticism; in particular, the application of P' to the textual and philological investigation of the Hebrew has attracted the lion's share of scholarly effort. Meanwhile the study of the meaning of P' - in the mind of the translator and of his readers - and other internal problems, have remained in the background. The reason may lie in a belief that until we have a definitive critical edition of P', it is too early to proceed to matters which delve any further into the text. However, I hope that the work of Section C has gone some way towards dispelling that prejudice. Such "internal" investigations ought not, I submit, to be relegated to second place in favour of the textual criticism of P', either by itself or in relation to MT and to the other versions. On the contrary, anything that casts light on the former will ultimately be found to advance our understanding of the latter.