Hebrew Biblical Manuscripts

Their History and Their Place in the HUBP Edition [1]

M. H. Goshen-Gottstein – Jerusalem

To Chaim Rabin
Friend and Colleague
On his Fiftieth Birthday

1. The renewed activity in textual studies of the Bible has led of necessity to a revival of interest in Hebrew biblical manuscripts. It stands to reason that in the present state of a general re-evaluation

[1] This study bears the imprint of the oral presentations on which it is based. This may explain the unwieldy subtitle which hints at the subjects treated together — the history of the Hebrew Bible text, the *Problemgeschichte* and the practice in the new edition of the Hebrew University Bible Project (HUBP). Keeping to the form of the original papers serves also, as usual, as an excuse for the large number of footnotes. Some of the points made here are touched upon in the Introduction to my recent 'Sample Edition' of Isaiah, especially §§ 16-18, 73-77, and should be read in conjunction with it. Slight changes of stress and formulation are intentional. This study is to a large extent a provisional summary of my work on the text of medieval Hebrew MSS, started soon after the first discovery of Qumran Scrolls, but is not meant to submit the detailed evidence on which my view is based; that would mean writing a special volume. Apart from the common abbreviations, the following *sigla* for my recent publications should be noted: *TL* = *Text and Language in Bible and Qumran* (1960); *BMU* = "Biblical Manuscripts in the United States", *Textus* 2 (1962); *RTBT* = "The Rise of the Tiberian Bible Text", *Biblical and Other Studies*, ed. A. Altmann (1963); *TPTC* = "Theory and Practice of Textual Criticism", *Textus* 3 (1963); *ISE* = *The Book of Isaiah: Sample Edition with Introduction* (1965); *PSCT* = "The Psalms Scroll — A Problem of Canon and Text", *Textus* 5 (1966). In my work on the text of Hebrew MSS I have been ably assisted by Dr. A. Hurwitz and by Mr. M. Bar-Asher of the HUBP, to whom I should like to express my thanks. The responsibility for the views put forward is solely mine.

we cannot yet fully appreciate the change in our overall picture of the development of the Hebrew Bible text. On the one hand, we have already grown accustomed to the idea that no other type of evidence for the Bible text is known to us for such a length of time as that of Hebrew MSS, and this seems almost too natural to need further comment. On the other hand, attitudes to, and evaluations of, Hebrew MSS which have developed during a period of about two centuries still linger on, and perforce influence our judgment. It is, therefore, a matter of course that we do not, as yet, possess any theory which tries to integrate all the known facts into one historical framework. The present study will deal mainly with the problem of Hebrew 'medieval' [1] MSS as part of an attempt at such a theory. The remarks about earlier periods are thus intended, not as an investigation in its own right, but as a sketch of the background against which medieval manuscripts should be seen and as an outline of the pre-medieval history.

2. Our evidence at present [2] suggests a division of the history of Hebrew biblical MSS [3] into three major periods, with an evident cleavage between the second period and the third [4]. Our witnesses from the first two periods are of great importance both for our knowledge of the development of the Bible text and for our understanding of the text itself. In other words: their readings can materially change our understanding of the text [5]. Although these witnesses have only recently become available to scholarship — and are still partly

[1] For the term cf. below, § 7.

[2] But for the basic problem discussed in *PSCT*, one might feel more confident that our results with regard to the details of the second period are there to stay. We are too much in the middle of new discoveries to claim finality for the picture we attempt to paint. The borders between the periods may therefore stand in need of future revision, apart from the inevitable borderline cases.

[3] This discussion deals only with the periods of MS evidence, not with the earlier transmission of the text.

[4] Cf. below, § 6.

[5] According to the position set forth in *ISE* § 3 f., I am not talking in terms of preferability for the reconstruction of the *Urtext*. Those scholars whose primary aim is such a reconstruction would adopt the formulation that those witnesses can sometimes get us nearer the *Urtext*.

unpublished — nobody would contest their place (1) in a 'critical' edition of the Bible text (2).

3. The first period starts around 300 B.C.E.; to what degree the picture emerging for that period holds true for the preceding 'Persian' period in the history of the Second Jewish Commonwealth is anybody's guess (3). This period is characterized by a diversity of textual traditions undreamt of two decades ago. It may already be said today that at the stage of *examinatio* the 'Massoretic Text' (4), in comparison with these traditions, often yields one out of two or more hyparchetypal variants which for the time being seem irreducible (5). Whether we shall be able to subdivide these traditions into more or less fixed 'recensions' and whether it is useful to add at this juncture identifying labels according to the habitats of Jewish communities, is a matter

(1) Whether they will occupy pride of place (cf. *ISE* § 8) obviously depends on future finds.

(2) One is almost afraid to use this term. I just refer to an edition of the text with an apparatus adducing the readings from various types of witnesses.

(3) Cf. *ISE* § 6. It is most tempting to push the limits upwards, beyond the earliest fragments from Qumran. But the outcome of past speculation beyond the limits of the evidence should teach us a lesson.

(4) Especially SPERBER has repeatedly suggested dropping the definite article from '*the* Massoretic Text'. Cf., e.g., A. SPERBER, *A Grammar of Massoretic Hebrew*, 1958, § 36. Cf. below, n. 3, p. 270. It seems inevitable that the different senses in which the term was used have become confused. In the light of our results in this study (cf. especially below, § 24) we might as well keep the article, at least for the purpose of text-critical discussions (as opposed to the problems of the Tiberian *textus receptus*. Cf. below, n. 6, p. 272, *ISE* § 74 and *RTBT*, Ch. VIII. Just now the re-issue of C. D. GINSBURG, *Introduction to the Massoretico-Critical Edition of the Bible* (New York 1966) has come into my hands. In the 'Prolegomenon' to this edition H. M. ORLINSKY wages war, amongst other things, against all the editors of Bible editions and translations who dared use the term 'massoretic' on their title-page. (The last editor who has committed this 'sin' is, I think, ORLINSKY himself, although his note 34 does not clarify his responsibility). I am not convinced that his pronouncements against '*the* massoretic text' (p. IX ff.) have lessened the general confusion. I hope that the present paper will clarify the facts; but I may have to return to an evaluation of ORLINSKY's statements on another occasion.

(5) Cf. *TL* p. 161. The present discussion deals with traditions in Hebrew only.

which needs further discussion (1). This diversity in Hebrew manuscript tradition is paralleled by extra-Hebrew witnesses, and the combined evidence must be taken into consideration. It is this period that offers the greatest challenge to the student of the Bible text (2), who, quite naturally, is mainly interested not in textual dynamics *per se* but in 'material' insights. In contradistinction to what will be said later (§ 22), it should be stressed that the types of readings and the textual development which previously could only be illustrated from extra-Hebrew sources, can now be more or less paralleled from Hebrew MSS (3). But it should be stressed no less that such a statement is to be understood typologically. We are in need of many more material finds before the theoretical importance of the insights gained during the past years will be equalled by factual gains in the sphere of variant readings (4).

4. The second period may be said to centre in the first century C.E. (5) and seems to have come to a close in the first half of the second

(1) Cf. *ISE*, Ch. I, n. 15.

(2) If evidence were needed, the first two apparatuses of *ISE* may be taken as an illustration.

(3) Differently put, these MSS contain both ever-fresh variations such as caused by the 'law of scribes' (cf. *TL*, p. 159) and 'real variants' (*ibidem*, p. XIII). All this is stressed in contradistinction § 22 to below.

(4) Cf. n. 5, p. 244, above and § 30, below.

(5) I am not sure that our paleographical knowledge is precise enough to fix very narrow limits (cf. *PSCT*, n. 15). A date in the first century B.C.E. for the beginning of this period seems acceptable, even without pressing too much the argument from halachic activity in the generation of Hillel and the statement of JOSEPHUS, *Contra Apionem* I, 42: τοσούτου γὰρ αἰῶνος ἤδη παρῳχηκότος οὔτε προσθεῖναί τις οὐδὲν οὔτε ἀφελεῖν αὐτῶν οὔτε μεταθεῖναι τετόλμηκεν. Even though it is based on Deut 4,2 and meant to impress foreigners, we have no right to discard this explicit testimony. The turn of the era must be presupposed also because of the activity of 'book correctors'. For this problem, which will not be discussed here, cf., e.g. S. LIEBERMANN, *Hellenism in Jewish Palestine*, 1950, p. 20 f. See also S. TALMON, *Textus* 2, p. 14 f. and now M. BEER, *Bar Ilan Annual* II, 1964, p. 136 f., and the references mentioned there.

Most recently F. M. CROSS has put forward strong arguments for an extremely low dating of the final rift between Jews and Samaritans (cf. *Harvard Theological Review* 59 [1966] 201 f.). I should like to suggest that it may be no coincidence if his date for the final branching off of the Samaritan Pentateuch turns out to be the first century B.C.E. (*ib.* p. 211). While it would not be unusual that in such a situation of religious tension

century (1). Since our material evidence comes to an abrupt end at that point, it stands to reason that our picture would emerge somewhat less schematic if later evidence were available. But in the light of the Rabbinic activity around 100 C.E., this picture — which in a slightly different shape is of long standing (2) — is probably more or less correct. The first century C.E. (3) is emerging more and more as the decisive phase in the stabilization and growing predominance of what I have termed elsewhere the 'Massoretic type' (4). By the end of the second period that type had become absolutely dominant — deviating details notwithstanding. Again, whereas previous descriptions of the history of the Bible text had to be pieced together without the benefit of Hebrew evidence, the picture which now emerges fits the combined evidence of Hebrew MSS, Versions and Rabbinic literature (5). To put it differently, the ratio between 'real variants' (6) and text diminishes during that period, but the readings can still be

---

textual differences tend to become part of a general interplay of cause and effect, the final breach may have been one of the factors which resulted in further unification of the 'Jewish' text-tradition. Hence the gradual emergence of that century as the turning point in the history of the Hebrew text and as the beginning of the second period.

(1) For a discussion it may be useful to agree on convenient, if somewhat arbitrary, terms. Chronologically 'pre- and post-Christian' may not be too far off the mark, but the term would be badly chosen. Since we have to reckon with Tannaitic activity as a major influence in the process of standardization (see below, § 31) 'pre-Tannaitic' may, perhaps, be suggested for the first period and 'Tannaitic' for the second.

(2) Cf. the sources mentioned below: n. 2, p. 258; n. 4, p. 263; n. 1, p. 270; and see § 31.

(3) It remains to be seen to what extent a more exact limitation is borne out by the facts. In *ISE* § 13 the 'period of the Destruction of the Temple' — the last third of the first century and the first third of the second century — has been suggested. But cf. the problems mentioned above, n. 2 (p. 244), and n. 5 (p. 246).

(4) Cf. *ISE* § 14.

(5) The results of the study of the Peshitta and the Aramaic Versions will have to be integrated, and it is to be hoped that recent activity in these fields will lead to a study of the aspects affecting our problem. The results of my own unpublished attempts in this field corroborate the picture. Cf. *ISE*, Ch. I, n. 25; cf. *ibidem*, Ch. IV as regards Rabbinic Literature. See also "Prolegomena to a Critical Edition of the Peshitta", *TL*, p. 175 f.

(6) As opposed to variations caused by the 'law of scribes'; cf. above, n. 3, p. 246.

of importance for our understanding of the Bible text (1). To be sure, we must be careful not to press our present evidence for the trend towards a more or less stabilized text or to make *ex silentio* arguments bear too much weight. But attention should be paid to the fact that from the 'destruction period' (2) no Bible texts of the 'non-Massoretic type' have so far come to light (3).

5. The end of the first two periods marks the decisive turning point in the history of the Hebrew Bible text. To put it, perhaps, somewhat boldly: the two periods we have tried to describe lead up to the very point which was the reconstructed starting-point of theories about the history of Hebrew biblical manuscripts current until two decades ago. Working back from medieval Hebrew MSS, and connecting the result with what could be inferred from the history of the Bible Versions and Rabbinic Literature, our predecessors were able to reconstruct a point of departure more or less identical with the end of the second period (4). This identity is neither coincidental nor self-evident, and this question will need some further discussion.

6. The turning-point reached is remarkable for an additional reason. For the first two periods Hebrew and non-Hebrew evidence runs on roughly parallel lines and all the evidence can be fitted into one picture. As from the end of the second period this 'parallelism' is disturbed: the third period of non-Hebrew evidence, down to the days of Jerome (5), as yet lacks any comparable Hebrew evidence; the third period of Hebrew MSS starts centuries later (6). It is here

(1) Cf. *ISE* § 13 f. and above, n. 5, p. 245.

(2) The problem of the Nash Papyrus need not be gone into on this occasion. If 11QPs[a] should turn out to be datable to this period, this statement may stand in need of revision. Cf. above, n. 2, p. 244.

(3) The much quoted Torah-scrolls with variant readings (from the כנשתא דסוירוס etc.) are not of the 'non-Massoretic' type. Cf. *ISE*, Ch. I, n. 26. For the facts cf., e.g., M. H. SEGAL, *Məvo ha-Miqra IV*, 1950, p. 880 f.

(4) For details see below, n. 1, p. 270.

(5) For our discussion it is immaterial whether any independent use of the Hebrew text by a Christian source later than Jerome can be detected. The anonymous Greek version of Habakkuk III is probably earlier; cf., e.g. GOOD, "The Barberini Greek Version of Habakkuk III", *Vetus Testamentum* 4 (1954) 29.

(6) The facts can be described by counting the periods differently, e.g., by leaving a blank in the 'Hebrew' column for the period 3rd–9th

that we are most likely to misinterpret what seems to be our evidence. We have just begun to understand that the history of about four decisive centuries — about 300 B.C.E. to 100 C.E. — was telescoped until this generation for lack of evidence. It seems improbable that new evidence of similar impressiveness will ever come forward (1), from the span of about six to seven centuries, until the emergence of the earliest MSS of the 'Massoretic period' (2); but the gap is too large to be made light of. On the other hand, it must be admitted that the state of the text reached at the end of the second period does not require any further intermediary stage for the 'Massoretic' MSS to have developed from (3).

7. We have thus reached the third period (4). The term 'Massoretic' may be used in this connection to indicate that none of the Hebrew biblical MSS known today which were written after ca 150 C.E. antedates the activity of the Massoretes (5); for the moment ' medieval' (6) is an equally acceptable term for that period. In quantity these by far outnumber all other Hebrew MSS, and until our generation this was the only Hebrew evidence. To be sure, these witnesses provide

---

century, and assigning the medieval Hebrew MSS to period IV. The variants found in the Arabic Version of R. Saadia Gaon hardly go typologically beyond what is found in early medieval Hebrew biblical MSS.

(1) I am on record (*Ha'arez* 15.1.1965) as having said that I would be more than surprised should we ever come across a find of Hebrew MSS where a whole lot (as opposed to one or two MSS) written after the beginning of the second century would turn out to be of a non-Massoretic type. In other words, it is more than improbable that there will ever be a discovery of a hoard of non-Massoretic type MSS, similar to that of Qumran Cave IV, written in that period.

(2) For the difference of terms cf. *ISE* § 74.

(3) It should be noted that this is an attempt to differentiate between main stages. For the possible developments after the second period, cf. below, §§ 20, 31.

(4) It may not be without interest to compare the 'periodization' suggested from quite a different point of view by B. KENNICOTT *Dissertatio Generalis* § 14 f. (Unless otherwise stated, I quote from the augmented reissue by BRUNS, 1783).

(5) I am not aware of any witness certainly datable before the ninth century. Earlier datings are still in need of proof.

(6) In order to avoid terminological difficulties (cf. *TL* p. 160, *ISE* § 74), witnesses from the first and second periods are termed here 'pre-medieval'.

an excellent illustration of textual dynamics, and they deepen our knowledge of the development of the Bible text in the technical sense. But ever since these were first studied in larger numbers, about two centuries ago, it was obvious that their contribution to the understanding of the text itself is extremely doubtful (1). We must therefore ask ourselves whether the retention of this type of evidence in the apparatus is due to more than mere habit and inertia. Perhaps in the light of the type of evidence available today, the time has come to disregard the readings of medieval MSS completely and to free the apparatus from meaningless ballast? If it is true that these MSS contribute practically nothing to an attempt to reach back to an earlier textual stage, perhaps the consequences must be drawn? Those who wish to study textual dynamics in their own right or to investigate the changes introduced by medieval scribes will turn to the original collections; other scholars may safely ignore that material. This problem cannot be brushed aside, because it has both theoretical and practical implications. We must, therefore, turn to the history of this question as it took shape in recent generations (2).

8. All views on the text of medieval Hebrew biblical MSS held by scholars in the past and at present are based primarily on the collations published in the end of the eighteenth century by Kennicott and de Rossi (3). In spite of assertions to the contrary (4), and of some material additions in the form of further readings, mainly due to the

(1) For the contrast cf. below § 22. The criteria for judging MSS in the time of Kennicott and in this generation are different, but the judgments are not as unconnected as appears at first sight. If we bear in mind that scholars in those days tended to accept the very identity of readings as proof of relationship, the widespread disappointment even in those days is telling. Cf. below, § 10.

(2) The following paragraphs had to be written in some detail because I have been unable to find a description of the *Problemgeschichte* which I can regard as absolutely correct and to which the reader could be referred. Much of the material for a full-length study is referred to in the notes to these paragraphs and it is hoped that our discussion will be sufficient within the present framework.

(3) B. KENNICOTT, *Vetus Testamentum Hebraicum cum Variis Lectionibus*, 1776-80; J. B. DE ROSSI, *Variae Lectiones Veteris Testamenti*, 1784-88; J. B. DE ROSSI, *Scholia Critica*, 1798.

(4) Cf. especially below, § 20.

diligence of Ginsburg (1) nothing typologically new was discovered after their time (2). It is therefore not really surprising that after a relatively short period of violent discussion in the nineteenth century most scholars in our time hold a view substantially identical with what had been stated back in the seventeen-nineties.

9. Kennicott's collations (3) were not undertaken as a purely philological work. They were one of several enterprises (4) resulting from the onslaught of the 'Critica Sacra', and were intended to be of theological significance. Both text-critical and theological positions were at stake ever since the implications of the *Sola Scriptura* and *Theopneustia* ideologies had developed to their logical end. The Reformers had placed the Hebrew text on a pedestal and it was this text on which severe doubts had been cast. Whether it was *Iudaeorum malitia* or *incuria librariorum* (5) was for many a question of secondary importance. Only the extensive study of Hebrew MSS could give an answer to the issue itself.

10. Against this theological background, the judgments and views expressed by scholars at that time (6) and their strong accusations and

(1) The material adduced by STRACK is hardly worth mentioning in this context. Cf. below, n. 3, p. 266.

(2) Of course, anything printed before the work of KENNICOTT must be regarded as superseded, although not all the variants published in the edition of the Bible by J. H. MICHAELIS (1720) were included by KENNICOTT. Practically all modern studies which use Hebrew variants are based on the collations of KENNICOTT and DE ROSSI.

(3) Unless specification is necessary, KENNICOTT's name alone will be mentioned, because most of the discussion turned on his work and because its arrangement made it more influential, although from the scholarly point of view DE ROSSI's work was more exact. Cf. below: n. 1, p. 252; and n. 2, p. 255. As *sigla* K–R will be used.

(4) Cf. the editions and collations from MILL until BENGEL and until WETTSTEIN and, in other fields, those of SABATIER and HOLMES-PARSONS. The study of Hebrew MSS had its special aim, but it was part of a larger movement. The term 'Critica Sacra' is used here in its general sense, not as the title of a collection.

(5) See L. DIESTEL, *Geschichte des Alten Testamentes*, 1869, p. 326 f., 345 f.

(6) The theological overtones remind us that but for the theological implications the work would never have been carried out. Perhaps even our generation is less free from theological predilection than we like to admit. Cf. for the field of N.T. studies the latest Presidential Address to

counter-accusations (1) must be understood. From the point of view of the theory of one fixed immutable Hebrew text the sheer mass of readings was overwhelming (2) and some scholars even felt the need to explain it away (3). Many readings previously known from the Greek seemed all of a sudden paralleled by Hebrew variants (4). These

the SBL of K. W. CLARK, "The Theological Relevance of Textual Variations in Current Criticism of the Greek New Testament", *Journal of Biblical Literature* 85 (1966) 1 f. and his statement (p. 5): "The only objective and justification of textual criticism is that it's emended text should give access to a clearer insight and a deeper faith." Cf. *TPTC* p. 137. We should also remember the remark of H. S. NYBERG, *Zeitschrift für die alttestamentliche Wissenschaft* 52 (1934) 244 repeated *Studien zum Hoseabuche*, 1935, p. 14 "Diese Geringschaetzung ist nur als Antithese gegen das alte kirchliche Dogma von der Verbalinspiration zu verstehen. Die ganze Psychose kann nur durch nüchterne philologische Betrachtung überwunden werden".

(1) From a perusal of the literature of the second half of the 18th century it seems that various scholars occupied different positions on a 'scale' of attitudes towards the received Hebrew text, and consequently had to defend their position on both sides. Even KENNICOTT — who certainly was interested in overstating the importance of the readings (cf. also below, n. 4) — started out by turning against denigrations of the Hebrew text; cf. B. KENNICOTT, *The State of the Printed Hebrew Text of the Old Testament Considered*, 1753, p. 556 f. J. H. MICHAELIS turned against KENNICOTT's claims, but he himself was considered by others to exaggerate the importance of the Hebrew variants (cf. ref. below, n. 4, p. 253). At the other end of the scale we find the extreme position of O. G. TYCHSEN (cf. n. 3). The rather sloppy work of some of KENNICOTT's assistants helped to make things easier for his opponents.

(2) Cf. e.g., the evaluation of J. C. DOEDERLEIN, *Auserlesene Theologische Bibliothek* II, 7, 1782. p. 483 f.

(3) The most radical attempt was made by O. G. TYCHSEN, *Tentamen de Variis Codicum*, 1772; *Befreyetes Tentamen*, 1774. Whereas this work is better known for the 'Transkriptionstheorie', taken up in our time by F. X. WUTZ, its major aim was to defend the received Hebrew text and to explain the deviations as coming from 'inferior' sources (written by Christians, women, etc.). Cf. also below n. 2, p. 270, and n. 3, p. 275. In any case, his extreme position did some good in making scholars more critical towards the 'Varientenrummel'.

(4) To be sure, many scholars were dimly aware that most of these 'readings' were secondary harmonization, simplificationss, etc. But we must not forget that at least one variation of the יָדְ / יָדֶ type (cf. below n. 3, p. 281) was discussed on many occasions. I am referring, of course, to the famous חסידך problem in Ps 16,10, which was judged to be of the utmost theological importance. KENNICOTT himself was very out-

variants seemed thus of the greatest importance and their use in any exegetical-critical work was imperative. Textual critics were at liberty to plough through the collations and to dig up what they thought useful for their purpose (1). They have done so ever since (2).

11. On the other hand some scholars were not slow to recognize that very little could be actually gained and that the huge mass of readings consisted mostly of secondary scribal changes, parallelisms, normalizations, harmonizations or free associations (3). In fact, for them the old theory of the one 'Massoretic Text' seemed vindicated *de facto*, only the picture (4) which had emerged was somewhat more

spoken in his claim and his influence is felt in all the subsequent discussion. Cf. his *Dissertatio Generalis*, § 49: "Quanquam vero manuscriptorum vetustissimi non superant aetatem annorum 800 vel 900, tamen desumti fuerunt hi ex manuscriptis qui ipsis antiquiores erant, idque fortasse per quaedam secula: auctoritas vero eorum, dum saepius confirmant versiones antiquas, assurgit ad aliquot secula A.C." Cf. also the more sophisticated discussion of DOEDERLEIN, *loc. cit.*, (n. 2, p. 252), p. 523.

(1) The summary of DOEDERLEIN, *loc. cit.*, II, 9, 1783, p. 675 is quite typical. According to his summary we should be content " dass hie und da eine Lesart verborgen steckt, die der Ausleger sucht und als Rest der alten Recension betrachten kann, dass Untersuchung, Prüfung und Wahl der Materialien, welche hier ausgelegt sind, dazu dienen kann, dass wir in der biblischen Kritik doch einige Schritte vorwärts kommen".

(2) It seems superfluous to voice again objections to the procedure of R. KITTEL'S *BH*. Cf. below n. 5, p. 264. It is somewhat surprising, however, to find out that in our generation a scholar should come forward with an appeal for readings from Hebrew MSS to be used more frequently in everyday exegetical work — an appeal which does not betray much knowledge of the problems involved and illustrated by rather unsuitable examples. See F. S. NORTH *Jewish Quarterly Review* 47 (1956) 77 f. Cf. below, n. 3, p. 284.

(3) Cf. *TL* 45 f., 57 f. DOEDERLEIN, *loc. cit.*, 653 f. gives a striking example of how, on closer inspection, practically none of the readings from the apparently deviating MS K 1 stands up to scrutiny. See also below, § 25 f.

(4) In spite of certain differences in stressing details, leading scholars of that time, like DOEDERLEIN, EICHHORN and MICHAELIS came to similar results. Thus, J. G. EICHHORN, *Einleitung* II, 1781, p. 239 summed up immediately after the first volume of KENNICOTT appeared: " Schreibfehler in Menge und brauchbare Lesarten äusserst wenige" (and this according to the criteria of that generation). This statement remained in EICHHORN'S standard work throughout the editions. Cf. 4th ed. II,

complicated than before. It was thus quite natural that a scholar at that time could sum up his impressions in the form of a 'one-recension theory' which was destined to get mixed up hopelessly, almost a century later, with Lagarde's ' archetype theory '. Rosenmueller, one of the half-forgotten foremost critics of that time was the first to formulate the results of his study of the huge collections in philological terms (1): "Dieser ganze mit so viel Aufwand von Zeit und Kosten zusammengefuehrte Variantenwust giebt übrigens das einfache Resultat dass alle noch vorhandenen Codices im Verhaeltnis zu den Originalen sehr jung sind... dass sie saemmtlich zusammen *eine* Rezension (2) darstellen, aus einer Quelle geflossen sind, und dass folglich aus ihnen für die etwa verdorbenen Stellen des hebräischen Textes wenig oder

p. 700, and *ib.* p. 707: "die Geschichte des Hebr. Textes lehrt, dass seine Hauptfehler älter sind als alle unsere noch vorhandenen kritischen Hülfsmittel..." Cf. MICHAELIS, *Oriental. und Exeget. Bibliothek* 11 (1876), p. 72 f. esp. p. 95 f., as well as the appendix containing the dispute between him and KENNICOTT.

(1) J. G. ROSENMUELLER, *Handbuch der bibl. Kritik und Exegese* I, 1797, p. 244. This publications is not available to me and I cannot verify the exact title and the quotation. I quote after E. PREUSCHEN *ZAW* 9 (1889) 303. Since PREUSCHEN had pointed to the formulation of 1797, it is not clear why scholars went on quoting ROSENMUELLER as if his view was formulated in 1834 only. Cf. below n. 3, p. 261, and n. 1, p. 264). (I have not searched for other possible statements of ROSENMUELLER on the subject. He does not mention it in his preface to the reprint of J. SIMONIS' edition of the Bible [1828]). By the way, there was no real need to rediscover ROSENMUELLER'S view, since it was quoted in DIESTEL'S standard work in 1869 (*loc. cit.* [n. 5, p. 251], p. 594, quoted as *Handbuch d.... Literatur*). DIESTEL took the 'one-recension' formulation as representative of the view taken by scholars who summed up KENNICOTT'S work. He does not yet as much as mention LAGARDE! Cf. below, n. 4, p. 263.

(2) The term 'Rezension' in ROSENMUELLER'S writings has to be understood in the sense in which it was current since it was introduced by J. S. SEMLER (1765) into New Testament studies. For the history of our problem it may not be uninteresting to point out that SEMLER'S term *recensiones* superseded J. A. BENGEL'S term *nationes*. The two terms thus foreshadowed the later 'local recension' from K. LACHMANN'S time down to our own (cf. below, n. 2, p. 257 and *ISE*, Ch. I, n. 15). Cf. now for N.T. studies B. M. METZGER, *The Text of the New Testament*, 1964, pp. 112 f., 119 f. The term 'Rezension' in those days was not yet understood as implying almost complete 'official' regulation. Hence it is accepted throughout this study and the term "one-recension' theory is used. But cf. below, § 31.

gar keine Hülfe zu erwarten ist" (1). The 'one-recension' theory was the result of a level-headed analysis of the collations of Kennicott (2); it was never seriously challenged or disproved (3), and its substance remains true to this day (4).

(1) This remains the most important criterion; cf. above n. 1, p. 250; n. 1 and n. 4, p. 253. In the words of EICHHORN, *loc. cit.*, II, 706: "Die suesse Hoffnung muss man aufgeben, dass er [the Hebrew text] selbst bei einem moeglichst vollstaendigen kritischen Apparat zu seiner voelligen urspruenglichen Reinigkeit wieder gelangen werde".

(2) The attitude to the text was shaped by the analysis of KENNICOTT's collection and in spite of its special features DE ROSSI's work was never allowed the same influence. Cf. above, n. 3, p. 251. It should be remembered that KENNICOTT did not note variants in vocalization; hence the impression of a unified text was even greater. Cf. below, n. 1, p. 282. Of course, KENNICOTT advanced good reasons for leaving out the vocalization — the amount of work involved, the different types of MSS, etc. — but the omission should also be viewed in the light of the 'vowel-war', which had not yet abated at that time. Cf. DOEDERLEIN *loc. cit.*, p. 508, and see the telling remark of MICHAELIS, *Orient. etc. Bibliothek*, 23, p. 104: "bey allem meinem Unglauben an das Alter der Puncte waeren mir diese doch sehr wichtig gewesen". Cf. TYCHSEN, *Tentamen*, p. 182 f., and *Befreyetes Tentamen*, p. 144: "Ein grosser Fehler ist es, dass Hr. D. K. sich nicht um die Vergleichung der Punkten (*sic*) bekümmert hat. Denn da er durch seine Varianten-Samlung den masorettischen Text zu verbessern zur Absicht hatte, die Punkte aber ein wesentliches Stück desselben ausmachen, so hätte er sie auch vergleichen lassen sollen... Sagt man, dass die Punkte oder die Aussprache eine neuere Erfindung der Juden sind und ein jeder Ausleger das Recht habe sie nach Belieben zu ändern, und dass daher die Consonanten blos authentisch sind, so sezt man unausgemachte Sachen als ausgemacht voraus, etc. etc.".

(3) Statements in the literature to the contrary rest on the misunderstanding discussed below, § 15 f.

(4) I am not concerned here with the felicitousness of this formulation or with the term itself. Cf. below, § 31. Some statements in the literature are worded to convey the impression that the 'one-recension' theory was already suggested by SPINOZA. It should be made clear, therefore, that not only is there the difference in the textual theory, indicated by the term 'Rezension', but SPINOZA's statement is simply a paraphrase of the well-known story about 'the three Scrolls of the Law that were found in the Temple Court'. This can be seen if one cares to check the discussion in *Tractatus Theologico-Politicus*, end of Ch. 9. The statement itself reads: "... *credo scribas pauca admodum exemplaria reperisse, forte non plura quam duo vel tria*". For the story of those scrolls cf. most recently S. TALMON, *Textus* 2 (1962), p. 14 f.

12. In spite of certain formulations in the beginning of the second half of the nineteenth century (1), matters might have rested there, but for Lagarde. Students of the history of a problem have got used to the fact that posterity tends to connect a certain theory with a certain man, regardless of his predecessors. Whatever our feelings about Lagarde as a person, his stature as a master of textual criticism was unrivalled. In spite of his waverings between theology and philology, he did more than anyone else to lay the foundations of textual criticism of the Bible as a philological discipline (2). Together with this went his penchant — his strength or weakness, as one prefers to see it — to formulate very outspoken and extreme theses which tended to acquire an axiomatic character. It was his clear-cut and attractive formulation which became the basis for subsequent discussion of our subject, and earlier formulations were soon erroneously equated with his view. A spurious dilemma began to bedevil all later statements, and a perusal of many studies and handbooks reveals that hardly anyone has managed to state the exact positions ever since (3).

13. The quotation of the famous 'archetype theory', first formulated just over a century ago, is in order (4): "es ergiebt sich also, dass unsere hebräischen handschriften des alten testaments auf ein einziges exemplar zurückgehn, dem sie sogar die korrektur seiner schreibfehler als korrektur treu nachgeahmt und dessen zufällige unvollkommenheiten sie herübergenommen haben. Über diesen archetypus des masoretischen textes würden wir nur durch conjectur hin-

(1) Cf. below, esp. n. 3, p. 261.

(2) The present writer is probably not the only one who often wonders wistfully whether the place of P. DE LAGARDE as a combination of all-round textual critic and Semitist has ever been filled. Precisely because the man and his views were often objectionable (see also below, n. 4, p. 258), his merits should not be belittled. LAGARDE did not reach his goal, and the specialization in the generations since then has carried us further and further away from it. Cf. *TL*, p. XIV, and my *Hebrew and Semitic Languages*, 1965, p. 26.

(3) I can only hope that I have got the facts right and that no further misunderstandings will occur; but, unfortunately, misunderstandings copied from book to book are almost ineradicable. Cf. also below n. 6, p. 272.

(4) LAGARDE, *Anmerkungen zur griechischen Übersetzung der Proverbien*, 1863, p. 2. I have tried, in general, to reproduce LAGARDE's orthography.

ausgelangen können, wenn uns nicht die griechische version des alten testaments die möglichkeit verschaffte, wenigstens eine schlechte übersetzung eines einer andren familie angehörenden manuscripts zu benutzen". According to this picture, the Hebrew and the Greek texts were likened to two witnesses representing two textual families. The conclusion to be drawn from all the existing Hebrew MSS was thus that they all went back to one archetype, one single manuscript(1).

14. No sooner had this theory been formulated than the misunderstandings began. Lagarde had not bothered to base his view on any analysis of readings in Kennicott and de Rossi. He rested his case at this stage exclusively on what could be regarded as a 'stemmatic proof' based on common characteristics — a type of proof acceptable according to the rules of textual transmission in cases in which texts were developing without outward influence or regulation (2). The

(1) It has not always been emphasized that LAGARDE assumed a parallelism in the development of the Hebrew and the Greek texts, that is, he maintained that in both cases we may attempt to reach back to the respective archetypes. There remained, however, an important difference. The aim for the Greek was the reconstruction of 'hyparchetypes' — the famous 'local recensions' — which would then enable scholars to work their way back to the original Septuagint. For the Hebrew there was no more than that one assumed 'archetype'. For the temptation to put forward similar theories to account for the facts in related fields of textual inquiry, cf. below, n. 1, p. 272, and n. 3, p. 281. For the somewhat belated introduction of such a theory into the study of the Vulgate, especially by H. QUENTIN, and the present state of the issue in that field cf. now R. A. KRAFT, *Gnomon* 37 (1965) 777 f.

(2) For a long time I have been wondering how the misunderstanding about LAGARDE's 'proof' (see below) could arise, and how most of his contemporaries were apparently unable to take his argument at its face value. The following seems to be a possible explanation: The scholars in the sixties of the last century to whom LAGARDE's formulation was addressed were Semitists and theologians, however thorough their training in classical philology. LAGARDE's way of argument, as well as the very term 'archetype', were taken over from K. LACHMANN. But in 1863 only thirteen years had passed since LACHMANN had finalized his textual theory in the introduction to his edition of Lucretius (1850). The theory and the way of argument were quite novel, and LAGARDE's fellow-Semitists and theologians could not really be expected to appreciate that LAGARDE rested his case exclusively on the slender evidence of 'common mistakes' etc., completely ignoring the whole issue of variant readings which in previous generations had stood in the centre of the argument. One might

common exterior signs, *puncta extraordinaria* and *literae suspensae*, which appeared at the same places in the Hebrew MSS were deemed sufficient proof (1) for the assumption of one common ancestor text. Nothing more was needed to prove the case. This completely theoretical construction was soon to be buttressed by two further pillars (2). First came a late Arabic story about a Scroll of the Law saved from Bittir from which other scrolls were said to have been copied — a story which was taken as historical proof (3), fixing the time of the archetype in the reign of Hadrian. Second — an unbelievable argument which was later on deliberately glossed over by others because of its unpleasant affinities to a certain type of literature — was the 'theological' proof, i.e. a certain chronological difference between MT and LXX was alleged to prove the wilful tampering of the Rabbis with the text from anti-Christian motives. Such tampering could only have succeeded, in Lagarde's contention, because all later manuscripts derived from the selfsame copy. (4). In other words: the arche-

object that theologians should have been familiar with LACHMANN's method from his work on the N.T., but before 1850 he had not yet developed his system. We have the word of U. v. WILAMOWITZ-MOELLENDORFF, *Geschichte der Philologie*, 1921, p. 59 that the edition of Lucretius was the decisive step. Cf. also G. PASQUALI, *Storia della Tradizione e Critica del Testo*, 1952, p. 3. The influence of LACHMANN on LAGARDE is, of course, obvious in his treatment of the 'local recensions' of the LXX. Cf. above, n. 2, p. 254, and below, n. 1, p. 264.

(1) LAGARDE stressed that he considered his statement in 1863 as proof; see, e.g., LAGARDE, *Symmicta* I, 1877, p. 50; II, 1880, p. 120.

(2) LAGARDE, *Materialien zur Geschichte und Kritik des Pentateuchs* I, 1867, p. XII and the references there.

(3) *Loc. cit.*: "... eine mir damals unbekannte notiz, die als ein historisches zeugniss für jenen damals nur durch combination gefundenen satz gelten darf".

(4) "... um die mit huelfe der LXX angestellten berechnungen der christen zu widerlegen, nach denen der Messias im jahre 5500 der welt erschienen war. Solche fälschungen (welche die kirchenväter so oft den juden vorwarfen) sind nur denkbar, wenn sie an Einem exemplare vorgenommen werden konnten, aus dem alle übrigen abschriften des textes zu entnehmen waren. Aquila ... hat seine übersetzung offenbar nur angefertigt, damit das im Interesse des grimmigsten christenhasses gedokterte hebräische alte testament ja den auständigen 'Gebildeten' nicht unbekannt bliebe...." Can one really do more than add exclamation marks with A. GEIGER, *Jüdische Zeitschrift* 7 (1869) 312? This incredible argumentation was quickly disposed of in a masterly study of A. KUENEN, *De Stamboom van den Masoretischen Tekst des O.T.*, 1873 (I have used

type theory had been formulated in 1863 on purely theoretical grounds, and it was bolstered up and given a chronological setting in 1867. It should be emphasized that these and only these points were Lagarde's 'proofs', and the problem of variants was never mentioned. These points were repeated over and over again (1), but no attempt was made by him to answer any of the queries raised (2). If Lagarde was forced to come back to the issue it was only in order to guard his priority rights (3).

---

mainly the German translation in BUDDE's edition of KUENEN, *Gesammelte Abhandlungen*, 1894, p. 82 f.). Cf. below, n. 4, p. 263. In the light of the history of this problem it is rather unfortunate that the author of a recent well-known handbook on the Bible text found it necessary to say in this context — I am sure, in ignorance of LAGARDE's argument — that in the "movement for fixing an authoritative text of the Hebrew Bible... the name most prominently associated with it is that of Rabbi Aqiba, probably the most renowned Rabbi of the early Christian era, and a notorious anti-Christian". See B. J. ROBERTS, *The Old Testament Text and Versions*, 1951, p. 25.

(1) Cf. *Materialien, Symmicta* I, *loc. cit.*, and especially his discussion of his priority rights, *Symmicta* II, 1880, p. 120 f. and *Mittheilungen* I, 1884, p. 22 f.

(2) *Mittheilungen, loc. cit.*, does not disprove anything. The arguments against LAGARDE are summarized below, § 17 f. Cf. also n. 4, p. 263.

(3) Cf. above, n. 1. An elaborate description of the theory in its historical context was given in 1870, reprinted *Symmicta* I *loc. cit.*: " Im jahre 1863 habe ich in den anmerkungen zur griechischen übersetzung der proverbien bewiesen (ich betone dies zeitwort), dass alle bisher bekannten handschriften des jüdischen kanons aus Einem archetypus stammen. am nächsten läge es, diesen archetypus für das exemplar der sammler jenes kanons zu halten, das natürlich officiell war. auffallen würde dann die stellenweise ausserordentlich grosse fehlerhaftigkeit des überlieferten textes. man könnte zu deren entschuldigung freilich sagen, dass der unter Esdras amtierende ausschuss die widersprüche des deuteronomikers etwa gegen den elohisten übersehen hat, also auch wohl fehler in den worten nicht bemerkt haben kann... indessen abgesehen von anderen erwägungen, haben wir ein, allerdings durch die mündliche überlieferung eines für geschichtsschreibung und geschichtsauffassung absolut unbegabten volkes hindurchgegangenes, also mit mehr oder weniger unzuverlässigem stoffe vermengtes ausdrückliches zeugnis dafür, dass jenes urexemplar unseres textes der zeit Hadrians angehoert... glaublich erscheint jene nachricht insoferne, als eine in den schweren zeiten unter Hadrian sich sammelnde gemeinde frommer juden leicht in der lage sein konnte, sich mit einem einzigen exemplare ihrer heiligen schriften begnügen zu müssen, und als das ansehen der männer, welche sich eines solchen exemplares bedient hatten, unschwer diesem buche eine grössere bedeutung verschaffte, als sein innerer wert philologischer kritik gegenüber ihm verliehen haben würde... was aber jene überlieferung als in der grundanschauung richtig erweist, ist der umstand, dass unser text des jüdischen kanons mindestens an Einem punkte gegen das christentum gerichtete correcturen enthält, mithin sein archetypus aus der christlichen zeit stammen muss...".

15. It is not easy to understand today how a theory built on so insecure a foundation was so warmly welcomed (1). I would go so far as to suggest that the main reason was that very misunderstanding against which Lagarde tried unsuccessfully to guard himself (2). His contemporaries took it for granted that his theory was only a refinement of the view expressed on the basis of the K–R collations, with slight variations, ever since the days of Rosenmueller — especially in Lagarde's own time by Olshausen. They talked of the theory of Olshausen and Lagarde (3) — and Lagarde's formulation appealed to

---

(1) The first scholar of note who accepted the theory was none other than Th. NOELDEKE, *Die alttestamentliche Literatur*, 1868, p. 241. But according to the popular character of that book he did not state expressly that this was LAGARDE's view. In scholarly discussion he reiterated his acceptance of the view five and ten years later (see HILGENFELD's *Zeitschrift für wissenschaftliche Theologie*, 1873, p. 445; *Zeitschrift der Deutschen Morgenländischen Gesellschaft* 32 [1878] 591). There is therefore no justification for LAGARDE's complaint (*Mittheilungen, loc. cit.*, pp. 22; 210) that his theory met with the 'Hohn der Zunft'. Since later literature states that the theory was accepted by J. WELLHAUSEN in the fourth edition (1878, p. 620 f.) of F. BLEEK, *Einleitung in das A.T.*, it should be noted that A. H. KAMPHAUSEN had already done that in the third edition (1870, p. 733). This is apparently the first textbook which mentions the theory in the name of J. OLSHAUSEN next to LAGARDE. KAMPHAUSEN's formulation shows that he did not appreciate the difference between LAGARDE's formulation and the older view: "... besitzen wir denselben wesentlich nur in Einer Recension, d.h. alle unsere Hebr. Handschriften gehen auf ein einziges Exemplar zurueck, dass man... zur Musterhandschrift erklaerte, und dann mit ganz sklavischer Treue abschrieb". Cf. *ib.*, p. 802. But it was probably WELLAUSEN's acceptance that was decisive. Cf. n. 3, p. 269.

(2) There is no evidence that scholars tended to welcome the theory because of the allegedly similar state of textual transmission, claimed at that time for the Vedas by M. MUELLER. But the parallel was suggested by A. GEIGER, *Jüdische Zeitschrift* 3 (1864) 78 f. and was taken up in BLEEK–WELLHAUSEN, *loc. cit.*,

(3) WELLHAUSEN, *loc. cit.*, described the theory as that of OLSHAUSEN and added (p. 621): "Um die weitere Verbreitung dieser sehr plausibeln Hypothese OLSHAUSENS's hat sich LAGARDE verdient gemacht" and ex-

them because he had added further 'proofs' (1). Soon the 'one recension' theory and the 'archetype' theory had become fused into one. Rosenmüller was quoted as having said more or less the same thing as Lagarde (2) — and it would seem that some scholars who were annoyed by Lagarde's unsavoury habit of repeatedly starting some 'priority quarrel' enjoyed pointing out that in this case it was Lagarde who had forgotten his predecessors (3). It was a somewhat tragico-

---

pressly stressed that "LAGARDE überschaetzt hier... seine Originalität". In the same vein, although not so outspoken, were the remarks of NOELDEKE, *Hilgenfeld's Ztschr.* and *ZDMG*, *loc. cit.* (n. 1, p. 260). Such remarks forced LAGARDE to stress his priority rights; cf. below, n. 1 f., p. 263.

(1) The widespread acceptance in the eighties of the last century can be seen from various statements in the literature. W. ROBERTSON SMITH, *The O.T. in the Jewish Church*, 1881, p. 70 f., 398 — through whose influence the theory was accepted in England — stated "I know of no attempt to refute the argument". Slightly different was B. STADE, *ZAW* 4 (1884) 302: "Es ist bekanntlich eine noch nicht alte Erkenntniss, dass der massoretische Texte des A.T. auf eine einzige Handschrift zurueckgeht. Und wiewohl die Variantensammlung zu jedem beliebigen Capitel des A.T. diese Annahme für jeden philologisch Geschulten als nothwendig erweisen müsste, vernimmt man zuweilen dagegen noch Widerspruch". Note that STADE connects the theory with the result of collations of variants. F. BUHL, *Kanon und Text des A.T.*, 1891, p. 259 summed up: "Insofern ist die verhaeltnissmaessig neue, aber schon sehr verbreitete Annahme, dass alle vorhandenen Handschriften auf einen Archetypus zurückweisen, entschieden richtig".

(2) Since handbooks are mainly responsible for perpetuating the formulations of summaries, I single out the wording of BUHL, *loc. cit.*, p. 261: "Die Abstammung aller Handschriften von einem Archetypus ist behauptet worden von ROSENMUELLER... OLSHAUSEN... LAGARDE... NOELDEKE." And he adds: "Eine ganz eigentuemliche Formulierung hat LAGARDE dieser Hypothese gegeben". This is a fair example of the way the two theories had become fused and BUHL's formulation was copied up to our own generation. Cf. R. GORDIS, *The Biblical Text in the Making*, 1937, p. 45. I. L. SEELIGMANN, *Tarbiz* 25 (1956) 118 also fell victim to the statements of his predecessors, in spite of some attempt to differentiate between views. See also below, n. 4, p. 263.

(3) OLSHAUSEN was named as the originator of the theory as soon as discussion started; cf. above, n. 1, p. 260 STADE, *loc. cit.* (above, n. 1), pointed to ROSENMUELLER's statement in the preface to the Bible edition of 1834, and PREUSCHEN, *ZAW* 9 (1889) 303 rediscovered the statement from 1797 (cf. above, n. 1, p. 254). Finally C. H. CORNILL, *ZAW* 12 (1892) 309 rediscovered the position of J. G. SOMMER, *Biblische Abhandlungen*, 1846, p. 79. ROSENMUELLER had talked about a 'recension' but SOMMER was the

mic situation that had developed: scholars had enthusiastically embraced Lagarde's formulation, but named his alleged predecessors into the bargain. Lagarde fought tooth and nail to make it clear that

---

first to suggest an exact description — of course without using the term 'archetype': "... da alle Handschriften und Ausgaben des hebr. Textes nicht nur im Allgemeinen Einer Recension angehören, sondern ursprünglich sammt und sonders aus Einem Exemplar der bereits vollendeten Schriftsammlung — vielleicht war es ein zu besonderem Ansehen gelangter Tempelcodex — herstammen...". It should be emphasized that SOMMER was not only the first to formulate the 'archetype' theory but also the first to differentiate it clearly from the 'one recension'. Cf. below, n. 2, p. 270. OLSHAUSEN, *Die Psalmen*, 1853, p. 18 wrote in a very similar vein, without any mention of SOMMER. According to him, an official 'recension' — very similar to the later 'massoretic' one — already existed in early Christian times. Since this 'recension' contains obvious mistakes which were not corrected, we must assume that it was based on one single MS for each part of the Bible, which was often damaged but was followed "mit sklavischer Treue". The recension itself had probably been arranged at the end of the first century B.C.E.

Even LAGARDE's 'proof' had practically been mentioned already by OLSHAUSEN, two years before the *Anmerkungen* appeared. In his *Lehrbuch der hebräischen Sprache*, 1861 § 31a OLSHAUSEN stated: "Wenn in einigen Stellen des Alten Testamentes die Finalform eines Buchstaben in der Mitte des Worts oder umgekehrt die gewöhnliche Form am Ende gefunden wird, so ist der Grund davon darin zu suchen, dass in dem Exemplare der heiligen Schriften, welches die diplomatische Grundlage der heutigen Textesrecension bildete, der Schreiber entweder mit Absicht oder durch Versehen eine andre Wortabteilung statuirte..." There can be little doubt that in fact LAGARDE only added some refinement in his formulation of the 'proof' as well as the very term 'archetype' to this theory. WELLHAUSEN's judgment of the actual contribution of LAGARDE (cf. above, n. 3, p. 260) therefore seems justified.

It remains an unsolved problem whether LAGARDE — who was quick to censure others for ignorance of the literature — was really completely unaware of his predecessors. As shown above, (n. 1, p. 260), KAMPHAUSEN knew about OLSHAUSEN in 1870, but NOELDEKE adduced OLSHAUSEN's view in 1873 (see n. 3, p. 260) as mentioned to him in a private talk, and LAGARDE maintains (*Mitth.*, I, p. 23) that OLSHAUSEN himself "von mir seiner zeit ausdrücklich befragt, erinnerte sich nicht, sich über den hier behandelten gegenstand einmal öffentlich geäussert zu haben". There remains the possibility that LAGARDE propounded his view independently, and once the facts were pointed out to him tried to defend his priority rights by stressing the slight differences. OLSHAUSEN himself appears to have remained completely quiet and did not claim his rights, perhaps because of his official position or else because he himself had omitted to credit SOMMER.

his theory was different from any other (1), that he did not accept Olshausen's position, that nobody had ever put forward any of his, Lagarde's, proofs, and that his proofs were different *toto caelo* (2). Hence it was his theory only — a theory different from their's (3). But he did not succeed (4), and scholars went on talking about 'archetype' and 'recension' as if they were synonyms and quoting Rosenmüller etc. as the first to have put forward Lagarde's theory (5).

(1) For details cf. *Symmicta* II, 1880, 120 f. and *Mitth.*, *loc. cit.*

(2) Almost the whole discussion centered around OLSHAUSEN's priority. Only in the '*Nachtraege*', *Mitth.* I, p. 381, did LAGARDE hear of the note about ROSENMUELLER, to be published that year (1884) in the *ZAW*, and he identified ROSENMUELLER's view with that of OLSHAUSEN, as opposed to his own. As far as I can see in the literature available to me, LAGARDE returned to the issue finally in 1886, in his review of CORNILL (*Mitth.* II, p. 49 f.) On that occasion he mentions for the first time that the question of variant readings may also be of importance for proving the existence of the archetype. Here he seems less confident about the dating of the archtype in the time of Hadrian, and even finds it worth mentioning that the archetype "erheblich über 750 hinaufreicht" (the date is based on a wrong premise in that context and need not concern us here). Cf. below, n. 2, p. 265, and n. 1, p. 270.

(3) *Mitth* I., p. 24: "Da ich Wert darauf lege, das oben wiederabgedruckte zuerst erkannt zu haben...".

(4) Right at the beginning of the discussion the particular position of LAGARDE had been stressed by GEIGER, *Jüdische Zeitschrift* 7 (1869) 312 f. in his review of LAGARDE's *Materialien* (1867). For GEIGER the commonly accepted position of his day was that there was practically one text as from the second century C.E. In other words, he bears witness that the 'one recension' theory was the reigning one. (cf. above n. 1, p. 254): "Im Gegensatze zu diesem wohlbezeugten Resultate stellt Herr LAGARDE in dem Vorworte zu den "Materialien" eine Ansicht auf, die wir als eine Probe seltsam launenhafter Kritik nicht vorenthalten wollen..." Thus, LAGARDE's first opponent stressed the difference. LAGARDE took notice of the review only by some personal sneers (*Mitth.* I, p. 22). Nor did he ever trouble to refute KUENEN, *Stamboom* (cf. above, n. 4, p. 258), who was the only one at the time who had attempted to undermine LAGARDE's structure by reasoned argument and who had formulated afresh a 'one-recension' formula ("In één woord: al onze codices vormen te zamen éene familie"). The most correct assessment of the differences of position given right after LAGARDE's death is the summary by E. KOENIG, *Einleitung in das Alte Testament*, 1893, p. 88, but he, too, missed a number of points. As for BUHL cf. above n. 2, p. 261.

(5) Thus, e.g., B. STADE, *Lehrbuch der Hebräischen Grammatik*, 1879 p. 20, mentions LAGARDE as having put forward a view about one recension ("eine einzige Recension"). In *ZAW* 4 (1884) 302 he mentions correctly, "eine einzige Handschrift" — without realizing the difference — and then goes on to equate this with ROSENMUELLER's view.

16. It seems a reasonable assumption that the lack of differentiation between the two formulations (1) had a share in preventing scholars from drawing the necessary consequences for their theoretical positions — and here we return for a moment to the text-critical practice. As long as the Hebrew manuscripts were viewed as belonging to one recension (2), there was at least a theoretical justification for their use in the critical apparatus and for connecting their readings with those of the ancient versions. A 'recension' is not watertight (3). But an 'archetype' is, by definition, only one manuscript. *Ex hypothesi* the belief in an archetype should have meant that all medieval readings were secondary changes in the transmission and hence could only be used for the reconstruction of that archetype. They could never be simply used side by side with readings from the versions (4), and such statements as 'read with G 4 MSS' or the like should have been disallowed on theoretical grounds. One might even have envisaged a difference of procedure between 'exegetical practitioners' according to their adherence to one theory or the other, with the adherents of the 'archetype theory' abstaining from the use of Hebrew MSS.

17. Nothing of the kind happened (5). Scholars were quick to accept the 'archetype theory' (or what they thought it to be), although they differed in their opinions as to how that archetype had come into

(1) In the eighteen eighties 'Archetypus' was already a well-established term. But it may be that the term 'Rezension' was not longer free any more from ambiguity. Cf. above, n. 2, p. 254; n. 2, p. 257.

(2) That is, until the seventies of the last century, because only by then had LAGARDE succeeded in making an issue of the question.

(3) Cf. below, § 21 f.

(4) I am not concerned at present with the actual correctness of retroversions (cf. *TPTC*), but with the practice of adducing readings in the apparatus, which ran counter to what theory should have taught. The consequences which should have been drawn from the 'archetype' theory seem to me self-evident. Looking for a discussion of such a problem in general literature I found this application of the theory affirmed by H. KANTOROWICZ, *Einfuehrung in die Textkritik*, 1921, p. 24. For my suggestion on handling the evidence, cf. also *ISE*, § 23.

(5) Quite typically, R. KITTEL, *Über die Notwendigkeit und Möglichkeit einer neuen Ausgabe der Hebräischen Bibel*, 1902, p. 5, having mentioned the differences of opinion about the 'archetype' theory, adds: "Für unsern

being (1). In practice nothing changed and Hebrew readings were quoted as before (2). But perhaps the time was too short for the exegetical and text-critical practice to be influenced by the new theory.

Zweck verschlägt der Streit wenig". It is largely thanks to his *Biblia Hebraica* that two generations of Bible scholars were reared on this way of looking at the evidence. Cf. also, n. 2, p. 253, and below, n. 2.

(1) OLSHAUSEN, *Psalmen*, p. 18 had assumed a "pharisäische Redaction" whereas Lagarde tried to pin down the historic situation in which there was literally just one MS left (cf. above, n. 3, p. 259). (KUENEN, *Ges. Abhandl.*, p. 118 turned also against this detail). NOELDEKE, *Die alttest. Literatur*, p. 241 thought that one MS had been chosen more or less by chance, "... dass man ziemlich planlos nach einer Handschrift griff, weit mehr besorgt darum, einen einzigen als einen guten Text zu bekommen". Cf. also BLEEK–KAMPHAUSEN, *Einleitung*, p. 802. A few years later the idea of 'deliberate suppression' of other MSS was added to the description (NOELDEKE, *Zeitschrift f. Wiss. Theol.* 1873, p. 444 f. Cf. below, n. 4, p. 288. It was probably because of NOELDEKE's preoccupation with the history of the Qoran that he saw the acceptance of the Hebrew archetype in the light of the action of OTHMAN, in utter disregard of the different historical situations. (This is not intended to deny that the comparison of the histories of Qoran and Bible texts may yield instructive illustrations, if cautiously applied; cf. also GORDIS, *Tarbiz* 27 [1958] 454 f.).

This picture of the Rabbis ruthlessly ordering all other texts to be suppressed (or destroyed) was only too readily accepted as explaining the later actions of the Massoretes, too. Cf. KAHLE, *Cairo Geniza*², 1959, p. 141 f., and my criticism, *RTBT*, Ch. IV. That picture mainly grew out of the misunderstanding of the institution of *'Geniza'*, which was mentioned in connection with the dearth of MSS again and again, at least since the days of WALTON (*Prolegomena* IV, 8): "Ratio etiam probabilis reddi potest cur non habemus codices Hebraeos... quia scil. post Masoretharum criticam et punctationem ab omnibus receptam Judaeorum magistri omnes codices his non conformes ut profanos et illegitimos damnarunt..."; cf. also H. L. STRACK, *Prolegomena Critica*, 1873, § 6.

For a summary of opinions by the end of the last century as to the choice of the archetype, cf. BUHL, *Kanon*, p. 259 f. More or less the same opinions were expressed as regards the origin of the recension, a number of scholars claiming that the MSS chosen were actually the best; cp. e.g. the position of KAHLE, *Masoreten des Ostens*, 1913, p. XVIII, against GRESSMANN. For a summary of the Rabbinic sources with regard to the question of the possible 'archetype' and the different types of MSS cf. LIEBERMANN, *Hellenism in Jewish Palestine*, 1950, p. 20 f.

(2) The first scholar of that generation who seriously attempted to carry out the demand that the exegesis of a book should start off with the use of the ancient witnesses and should be preceded by a systematic

For no sooner had the 'archetype theory' become accepted than the gradual rejection of Lagarde's extreme position started (1). Just as scholars had not really bothered to examine Lagarde's reasoning and his position was accepted under a partly false impression, so they did not bother to disprove Lagarde's proof formally by unmasking the fallacy of his stemmatic argumentation and by rejecting his theory on those grounds. Thirty years after 1863 it began to be realized somehow that the particular history of massoretic MSS made them defy the usual stemmatic procedures — and hence Lagarde's proof was no proof at all (2). But this was not the decisive argument. To be sure, the weight of the work done by those of his younger contemporaries who had worked on Hebrew MSS, especially that of Strack (3), was thrown against Lagarde, although their actual

---

evaluation of those witnesses was C. H. CORNILL, *Ezechiel*, 1886 (cf. also the review of LAGARDE, *Mitth.* II, p. 49 f.) CORNILL accepted LAGARDE's thesis enthusiastically and even tried to strengthen it by reintroducing the problem of variants (cf. n. 2, p. 263, and n. 2, p. 272). But he then went on to quote Hebrew readings without so much as realizing the dilemma. Nor did LAGARDE comment on it in his review.

(1) The reasons given in the literature for the rejection of the 'archetype' theory are many and varied, showing a correlation with the subject each scholar happened to be dealing with; cf. also n. 1, p. 269, and n. 1, p. 274. An interesting example is F. ZIMMERMAN, *Jewish Quarterly Review* 34 (1944) 459, dealing with the problem of double readings, who claims as first reason for LAGARDE's fall: "In the first place, scholars have noticed that in quite a number of instances the massoretic context contains obvious variants placed side by side." Cf. below, n. 1, p. 285.

(2) During LAGARDE's lifetime KUENEN (cf. above, n. 4, p. 258, and n. 4, p. 263) remained the only one to have written a proper refutation, but also he was not aware of the basic fallacy of LAGARDE's argument based on the *puncta extraordinaria*, etc. Immediately after LAGARDE's death E. KOENIG, *Einleitung* etc., 1893, p. 88 f., and H. L. STRACK *Einleitung*, etc., ⁴1895, p. 172 questioned the validity of that argument, followed by V. APTOWITZER, *Das Schriftwort in der Rabbinischen Literatur: Prolegomena*, Sitzungsberichte Akad. Wien, Phil.-Hist. Kl. Bd. 153, 1906, p. 5. A remark on the subject from D. H. MUELLER's classroom, quoted by APTOWITZER, clearly exposes the weakness of LAGARDE's argument. The main issue, however, was the return to the evaluation of variants, and LAGARDE's real argumentation was pushed aside. Cf. esp. below n. 5, p. 275; n. 2, p. 278.

(3) STRACK was the only one in the decade before APTOWITZER who repeatedly rejected LAGARDE's formulation even though he did not add further evidence. Cf. preceding note and his paper in *Semitic Studies*

findings did not really change the overall picture as known from the collections of Kennicott and de Rossi. But nothing of what they said actually countered Lagarde's basic argument about the *puncta extraordinaria*, etc. Trying to reconstruct the history of our problem, one is tempted to conclude that since Lagarde's theory had never really been accepted just on its own merits, but rather as a continuation and logical conclusion from the positions of his predecessors, which were based on the study of variants, it sufficed to point out forcefully that the overall evidence of variants now available precluded the assumption of an archetype. It was first and foremost a return to the evaluation of variant readings which dethroned the Lagardian formulation (1).

18. Once the Lagardian structure had begun to collapse, further attacks were readily interpreted as having dealt it the final blow. Scholars were now only too eager to accept the contention of Aptowitzer (2) that the variants in Rabbinic literature suffice to nullify Lagarde's formulation (3). Since the type of material on which Aptowitzer based his argument was relatively unknown to most theologians and had hardly been used seriously in the previous discussion of the

in Memory of A. Kohut, 1897, p. 571 — basing his opposition especially on the large number of marginal notes to codices — and *Dictionary of the Bible*, IV, 1902, p. 28.

(1) Cf. E. KOENIG, *loc. cit.*, C. STEUERNAGEL, *Lehrbuch der Einleitung in das A.T.*, 1912, p. 20 f. See the list of B. J. ROBERTS, *O.T. Text and Versions*, 1951, p. 27. But cf. below, n. 3, p. 283. It must, of course, be remembered that not all the scholars whose views are mentioned in such lists were in a position to express a view based on first-hand study.

(2) *loc. cit.*, p. 3 f.

(3) "Dieses Argument fällt mit dem Nachweis solcher Varianten einfach weg". It is characteristic of the atmosphere in those days that nobody paused to ask whether the argument based on variations in *Rabbinic* sources had any necessary bearing on the theory of an archetype assumed for *Bible* MSS. In any case, I have not seen that anyone suggested this rather obvious counter-argument. All later discussions revolved around the question to what degree one should assume 'quotation by heart'. (For a similar problem in the study of the Peshitta cf. *TL*, p. 196 f.) An interesting argument recently put forward from 'orthodox' quarters is that because of the strict regulations as to the conditions of writing down Scripture, the Rabbis intentionally refrained from exact quotations. Cf. R. MARGULIES, *Bible and Massora* (in Hebrew), 1964, p. 47 f. It is not clear how this explanation accounts for the great majority of quotations which agree with MT.

subject (1), its impact was considerable and made many scholars decide finally against the archetype formulation (2). It thus began to become customary for scholars to propagate the view that Lagarde's theory was still basically true, but had to be slightly modified. His position was almost right — with the stress on 'almost'; his formulation was too extreme and should be tuned down; one must allow for more flexibility, etc. (3). It is not without reason that we attempted previously to analyze the development through which the 'one-recension' theory got mixed up with the Lagardian position. Since most scholars around the turn of the century were convinced that Lagarde's predecessors had intended to put forward his theory and that there was only one theory accepted by most leading scholars during the latter half of the nineteenth century, none of Lagarde's opponents was able to state in so many words that the seemingly new, 'almost Lagardian' position was in fact nothing else but a return to the

(1) Even so, it had come up sporadically, especially since the days of KENNICOTT, and STRACK had stressed it in his dissertation. A list of his predecessors is given by APTOWITZER, p. 8.

(2) The statements of KAHLE in his young days show clearly that his anti-Lagardian position was influenced by APTOWITZER. This is foreshadowed in a long footnote, *Masoreten des Ostens*, 1913, p. XVIII f. and developed in his "Untersuchungen zur Geschichte des Pentateuchtextes", 1915 = *Opera Minora*, 1956, p. 31. APTOWITZER's work is not even mentioned in *Cairo Geniza*!

(3) A typical formulation, e.g., STEUERNAGEL, *loc. cit.*, p. 22: "... dass jene These in abgemilderter Form doch ein gewisses Recht hat". Similarly, KAHLE, *Opera Minora*, p. 33: "Seit ungefähr 100 n. Chr. ist in den offiziellen jüdischen Kreisen unser masoretischer Konsonantentext als *textus receptus* betrachtet worden — so könnte man vielleicht LAGARDE's Hypothese modifizieren". The formulation, some decades later, of O. EISSFELDT is not much different, but he allows for more flexibility; cf. *Einleitung*², 1956, p. 838: "die... These... ist in dieser Form nicht mehr haltbar..."; cf. 3rd ed., p. 929 f. See below, n. 2, p. 270, and n. 3, p. 283. It should be noted that EISSFELDT also writes as if his view is directly opposed to LAGARDE's thesis, and the reader is not really made aware that LAGARDE's formulation had been rejected by practically all scholars since the beginning of this century. One may compare the slightly varying formulations of J. REIDER, *Prolegomena... to Aquila*, 1916, p. 81 f.; A. BENTZEN, *Introduction to the O.T.*, I, 1948, p. 51 f.; D. Winton THOMAS in H. H. ROWLEY (ed.), *The Old Testament and Modern Study*, 1951, p. 244 f. B. J. ROBERTS, *loc. cit.*, p. 25; M. NOTH, *Die Welt des A.T.*, 1953, p. 245, etc. My own formulation in *TL*, p. XI, reflects the common state of misinformation; the formulation in *ISE* § 75 is correct, as far as it goes.

view formulated by the father of the 'one recension' theory, back in 1797 (1).

19. At no stage, however, did the pendulum swing back towards an even more 'positive' evaluation of Hebrew readings, such as was suggested by some scholars in Kennicott's times. If there was not sufficient proof for assuming an archetype, there was not more than what could be encompassed within one recension (2). To be sure, back in the eighteen-eighties matters had looked different for a short moment, when Hebrew MSS with completely different readings were said to exist. But the forgeries were soon detected (3), and the result of the disappointment may, in a way, have indirectly even strengthened the general opinion that no readings of 'importance' could ever be found in genuine MSS.

20. Thus, by the first quarter of this century, practically all specialists were agreed on what amounted in fact to a 'one recension' theory (4). The main difference as compared with the position of

(1) It may be added that since scholars have not been aware of the differences of position, the outline of the *Problemgeschichte* suggested here should be judged only in the light of the primary sources concerned.

(2) This may explain why the issue of studying variants from biblical MSS was practically reopened only in the thirties of this century. Cf. P. VOLZ, *ZAW* 54 (1936) 104: "Mir scheint dass die Annahme eines Archetypus daran gehindert hat in der Untersuchung der hebr. Handschriften auf Wert usw. vorwärts zu kommen". But (p. 106): "Der Ertrag der Variantensammlung ist, wie ich aus eigner eingehender Beobachtung weiss, ganz abgesehen von der Frage des Archetypus, sehr gering".

(3) As for FIRKOWICH, cf. my recent remarks in *Tarbiz* 33 (1964) 149 f. It is most unfortunate that J. M. ALLEGRO has attempted to warm up the SHAPIRA affair in a popular book, connecting it afresh with the Qumran discoveries and insinuating gross errors committed by the leading Bible scholars and archeologists in those days. (*The Shapira Affair*, 1965). ALLEGRO has not adduced a shred of new evidence or so much as attempted to refute the arguments brought forward in recent discussions. Any attempt to clear the Shapira forgeries will first have to get rid of the evidence I submitted in *Journal of Jewish Studies* 7 (1956) 187 f. See the London *Jewish Chronicle*, Nov. 12, 1965 and cf. also O. RABINOWICZ, "The Shapira Scroll: A Nineteenth-Century Forgery", *JQR* 56 (1965) 1 f.

(4) Sometimes a formulation was copied which reminds one of J. G. SOMMER's statement (cf. above n. 3, p. 261), but seems to have been some kind of 'compromise'. As far as I can see, it occurs first in S. R. DRIVER, *Notes... on... Samuel*, 1890, p. XXXVII: "All MSS belong to the same recension, and are descended from the same imperfect archetype." This was taken over, e.g., by J. REIDER, *loc. cit.* (cf. above, n. 3, p. 268).

Rosenmüller was that it became now usual to quote a date for the recension, mostly by simply relying on the date suggested by Lagarde for the 'archetype', but sometimes by pushing it slightly up or down (1). Apart from minor fluctuations (2), the problem could be regarded as settled, and remained so up to the time of the recent discoveries. In this context one cannot but point out that the work of Kahle and his followers on the vocalization systems dealt, in a way, only an additional blow to the Lagardian theory (3). Statements in the literature

The only one in our generation who built his own thesis on the 'archetype' theory is R. GORDIS, *The Biblical Text in the Making*, 1937, p. 45 f. He believes in one single MS on the margins of which variant readings had been noted. His picture is somewhat like that of NOELDEKE in *ZDMG* 32, p. 591. In private talk he stressed that he did not think of the archetype in the Lagardian sense, that no other MS was in existence at the time, but rather as *the* MS, out of a larger number, which became the basis for all the later ones. Cf. now his slightly modified exposition in *Tarbiz* 27 (1958) 444 f., where he stresses that the large number of variants in Hebrew MSS need not influence the verdict on the issue of the archetype.

(1) In a way, the suggestion to recognize a 'second period' (cf. above, §4) is little more than an attempt to get into one framework the various dates suggested for the 'archetype' on the basis of ancient Rabbinical and historical literature, since the days of SOMMER and OLSHAUSEN (cf. above, n. 3, p. 261). For the problem of evaluation of Rabbinical sources cf. also the references above, n. 5, p. 246 and in GORDIS's discussion (cf. preceding note and his reply to ALBRIGHT in *JBL* 37 [1938] 330). The connection with the early Christian period has been claimed at least since R. Simon; cf. BUHL, *Kanon*, p. 261. More scholars accepted the suggested date in the first century C.E. than is usually realized; cf. especially Th. NOELDEKE, *Zeitschr. f. wiss. Theol.*, 1873, p. 446 and LAGARDE's slight withdrawal (above n. 2, p. 263).

(2) The formulation of O. EISSFELDT, *Einleitung*², p. 838 is typical of a position which assumes a rather considerable degree of fluctuation (cf. above, n. 3, p. 268), probably influenced by KAHLE (see below). But nobody went as far as to suggest a typological differentiation among Hebrew MSS. In the light of § 23 f. below, it should be added that the last one to maintain that against the uniformity of the rest of the recension there are a few MSS which are seriously deviating was SOMMER (cf. above n. 3, p. 261). But from his discussion it would seem that his remark was rather in the nature of claiming Christian etc. origin for deviating MSS, after the fashion of TYCHSEN (cf. above, n. 3, p. 252): "Allerdings giebt es nebenher auch etliche hebräische Codices die einer andern Reihe von Abschreibern, zum Theil sicher Judenchristen, angehören...; aber auch diese Gattung gehört mit den aecht jüdischen Codices derselben Recension an".

(3) Cf. e.g., the introduction to R. KITTEL, *Biblia Hebraica*³, p. XIII. KAHLE's remarks on problems of textual variants remained incidental

which may create the impression that it was mainly Kahle's work that overthrew the 'archetype' theory are not borne out by the facts (1). Kahle published the first major summary of his views (2) when the anti-Lagardian movement had won the day and he never added new evidence against the 'archetype theory' (3). His main contribution

---

and he never attempted to study the Geniza material in this respect systematically; cf. *BMU*, Ch. II. The provocative statement of A. SPERBER (cf. above n. 4, p. 245): "There never existed The Massoretic Text and consequently never will be" has little to do with the subject under discussion here. But such statements easily lend themselves to misinterpretation.

(1) Thus, e.g., it is difficult not to understand A. BENTZEN, *Introduction*, I, p. 56, as if it is because of KAHLE that "we have to abandon the theory of the archetype". Similarly E. WÜRTHWEIN, *Der Text des A.T.*², 1963, p. 26. More correctly R. H. PFEIFFER, *Introduction to the O.T.*, 1948, p. 79. Cf. below, n. 3. Precisely because of H. M. ORLINSKY's many just strictures against KAHLE one would have wished that his description of the position in *Journal of the American Oriental Society* 61 (1941) p. 84 f. had been more exact, the more so since younger scholars have copied the details on his authority. As far as I can make out, according to ORLINSKY, too, it was KAHLE who attempted to refute LAGARDE. But LAGARDE, according to him believed in the 'one-recension' (not the 'archetype'!), whereas KAHLE rejects the theory of a one-text tradition. Since the idea of 'one-recension' is right, ORLINSKY upholds LAGARDE. ("In the second third of the nineteenth century, ROSENMUELLER, OLSHAUSEN and LAGARDE, especially the last-named, advanced the view, which is held by practically all competent scholars today, that all preserved MSS of the Hebrew text of the O.T. go back to the one recension which came to domination in the first-second century A.D. at the latest. In this I concur".) I can only add that I wonder whether ORLINSKY would have identified his position with that of LAGARDE, had he been aware of the exact facts. As his words stand, they are part of a total position for LAGARDE against KAHLE, as regards both the Hebrew and the Greek textual traditions. Furthermore, in spite of the strong words of ORLINSKY against GORDIS (cf. *JAOS* 60 [1940] 30 f.), both these pupils of M. L. MARGOLIS happen to be the only ones who maintained, around 1940, that LAGARDE was right. Since I tend to assume at least a partial misunderstanding, I agree with ROBERTS, *JJS* 1 (1949) 147 that the apparently considerable differences between the positions of KAHLE and ORLINSKY are exaggerated.

(2) Published in 1915; cf. above n. 2, p. 268.

(3) The few variants from MSS with Palestinian vocalization published in *Masoreten des Westens* II, 1930, p. 22* f. did not change the picture at all. Cf. below, n. 3, p. 272. I should think it fair to state that KAHLE's main point was that in the light of the different systems of traditions of vocalization, "es scheint dass erst die exakte Arbeit die die

was an argument *ex analogia* (1) from his own theory about the work of the Massoretes (2). It was thus no specific evidence (3), but rather the overall weight of Kahle's position, which made some scholars adopt a view in favour of a certain textual fluctuation even after the second century C.E. (4) It would, therefore, be correct to sum up that Kahle's (5) writings made it finally clear that Lagarde's formulation was erroneous and that Kahle went further than his predecessors in assuming fluctuations within the 'one recension' (6). On the theo-

---

Punktation des Konsonantentextes erforderte, einen wirklich einheitlichen Konsonantentext — in Palästina wie in Babylonien — durchsetzte" (*Opera Minora*, p. 32). Perhaps one may say that whereas KAHLE's predecessors had rejected LAGARDE's formula, but had thought of the 'one-recension' as being almost of an archetypal nature, KAHLE saw larger divergences within the recension which became unified through later gradual convergence. Cf. below, n. 2, p. 273. This may explain why some authors of handbooks credited KAHLE with overthrowing LAGARDE's thesis (cf. above, n. 1, p. 271). It should be stressed that what KAHLE put forward was a somewhat refined view, but not based on any new and relevant evidence. In a way, he refined the picture drawn by his anti-Lagardian predecessors, just as LAGARDE had done with the statements of OLSHAUSEN, etc.

(1) Cf. especially *Opera Minora*, p. 26. It cannot be shown here in detail how the argument *ex analogia* played a large part in KAHLE's theories in general. Cf. also *RTBT*, n. 42, and *TL*, p. 176.

(2) This did not prevent him mixing up facts about the text and about vocalization, and claiming that CORNILL in his comparisons of the Petrograd Codex of 916 with the printed text "in all the details of punctuation of such a long and difficult text he was able to find only about a dozen slight variations" (cf. *Cairo Geniza*², p. 63). Because of the interest of CORNILL's position for the history of our subject (cf. above, n. 2, p. 263; n. 2, p. 265) I cannot but note that KAHLE got the facts wrong.

(3) ROBERTS, *JJS* 1 (1949) 152 sums up, rightly, that the variants collected by KAHLE have little bearing on textual questions. Cf. above, n. 3, p. 271.

(4) Even some of those who favoured KAHLE's modification of the theory felt he pressed his point too much, Cf. ROBERTS, *loc. cit.*

(5) For KAHLE's lack of interest in Tiberian Geniza material cf. *BMU* p. 35 f. See above, n. 3, p. 270.

(6) KAHLE's opening sentences on the two senses in which 'textus receptus' is used (*Opera Minora*, p. 26) can be wholeheartedly endorsed. His summary (*ib.*, p. 33) is in line with what had been said by his predecessors. ROBERTS, *O.T. Text*, went even beyond KAHLE when he stated that "the theory collapses when it is argued that the archetype belongs to the second century A.D., or some such early date. The MSS on which

retical side, the victory of the 'one recension' theory — regardless of the terms or words used — was thus well established by the end of the first third of this century, and it has remained victorious throughout the second third. What seemed to Kahle and his opponents a major clash between their positions was no more than a minor difference of views which did not really affect the basic common theory (1). It was a difference as regards the degree of internal unity that scholars were ready to allow the recension at *ca* 150 C.E., with Kahle opting for the picture of a broader basis and a partial later ironing-out of variations (2).

21. In the context of our present study it should be stressed that at no point of the discussion did the theoretical position have any repercussion on the everyday practice of the use of Hebrew variants by 'practitioners' of textual criticism. Hebrew variants were continued to be quoted as before, and their use was not felt to constitute any problem (3). On the other hand, the end of the first third (4) of

---

it is based belong in the main to the twelfth century A.D. and later" (p. 24) and "... until final emergence of the standard archetype text of the ben Asher Massoretes, which received official status by the decree of Maimonides in the twelfth century A.D." (p. 29). Bearing in mind the history of the term 'archetype', this is either an unfortunate use of the term or a mistaken presentation. My objections to ROBERTS' formulation on another occasion (cf. *RTBT*, n. 126; see also above, n. 4, p. 258) have been attacked by him in *Journal of Theological Studies* 15 (1964) 253 f. (but see *ib.*, p. 331). I shall try to reduce differences of opinions between us to a minimum in a future paper.

(1) Cf. also above, n. 1, p. 271. Possibly some writers tended to overstress the differences between positions precisely because there are too few facts.

(2) In a way, the differences and possible combinations between the 'family-tree theory' and the 'wave theory' in linguistic theory may be compared.

(3) Cf. above, § 16. Of course, had anyone been asked about the theoretical basis, he might have answered that since the 'archetype' formulation had been rejected, there was no bar to using Hebrew variants. It should be noted that the scholars who took part in the theoretical discussion or wrote the handbooks were usually not those who carried out the actual text-critical work. Cf. below, n. 7, p. 274.

(4) Cf. below, § 23. HEMPEL was the first scholar who approached the problem methodically, and was aware of the necessary connection between the Lagardian theory and text-critical practice. His study in

this century marks a renewed interest in the old problem of the 'value' of those variants and their sources (1). This problem was to appear in a new perpective about the middle of the century (2). It has been mentioned above (3) that the K–R readings have remained the basis for any view expressed on the subject of Hebrew variants and that none of the additional material published since has altered the overall picture (4). Therefore, any judgment expressed in our generation is, by necessity, hardly more than a restatement of the opinions put forward in the second half of the eighteenth century, whether pro or con (5). Our main advantage over our predecessors lies, therefore, in our ability to view medieval Hebrew MSS against the background of pre-medieval ones (6), and to attempt to create some interaction between theoretical insights and text-critical–exegetical practice (7).

22. The results of our investigations force upon us the conclusion that pre-medieval Hebrew biblical MSS are *typologically* different from medieval ones. Only now that different types of readings can be adduced from pre-medieval MSS can it be fully appreciated that what sets medieval MSS apart is the fact that they contain practically exclusively variations of the types which can arise again and again

---

ZAW 48 (1930) 187 f. is a step forward. Had I been aware of that article a dozen years ago, when I first turned to this subject (*Biblica* 35 [1954] 429 f.), I could have saved myself some remarks.

(1) Whereas by the end of the 19th century the disappointment with K–R's collections is given as the reason for the discontinued interest in Hebrew MSS (cf., e.g., STRACK, *Semitic Studies... Kohut*, 1897, p. 562), other reasons were also put forward now; cf. n. 1, p. 266, and n. 2, p. 269.

(2) Cf. *Biblica, loc. cit.* = *TL*, p. 51 f.

(3) Cf. above, § 8.

(4) If anything, the basic textual uniformity has only been underlined by the publication of medieval MSS which belong to other traditions than the 'Tiberian receptus' one.

(5) It seems not impossible that the very fact that scholars of our generation were not always educated to work for themselves from the primary collections but rather got acquainted with Hebrew readings through sporadic and selective quotations, has created among many a wrong evaluation of the material and fostered unjustified expectations. Cf. above, n. 5, p. 264.

(6) Cf. above, § 7 and n. 6, p. 249.

(7) For attempts to combine the two aspects cf. the introductions to *TPTC* and *ISE*. Cf. above, n. 3, p. 273.

through scribal activity (1), Among tens of thousands of readings from medieval witnesses which were checked in this connection it was hard to find even a handful which were not immediately explicable as having arisen through harmonization, simplification, etc. To be sure, all these occur in Hebrew MSS from the first two periods and can be amply adduced from the Versions. But there they exist side by side with 'real variants' (2). It is precisely the existence of 'real variants' that has made the pre-medieval MSS a new challenge to textual criticism (3); it is their absence that marks medieval MSS (4). This is the reason why scholars have been disappointed ever since Kennicott's collections became known and why the study of medieval Hebrew MSS was previously said to be little more than an illustration of textual dynamics (5). On the other hand, it is the considerable number of variants, formally common to medieval Hebrew MSS and pre-medieval sources, which we tend to attribute to the ever active and repeated force of the 'law of scribes' that creates the illusion (6) of a genetic connection between medieval MSS on the one hand and pre-medieval ones and the Versions on the other. It is that apparent formal identity which has caused scholars ever since Kennicott's time to state that, in spite of the generally disappointing results, there are some readings of 'value' in medieval MSS, which

(1) For the various types cf., e.g., *TL*, p. 45 f., 57 f. The problem of 'simple' scribal mistakes and omissions is not dealt with here. Cf. below, n. 4, p. 279, and n. 6, p. 285. Cf. also the discussion in B. M. METZGER, *The Text of the New Testament*, 1964, p. 186 f.

(2) For the term cf. *TL*, p. XIII. The existence of borderline cases, here as elsewhere, does not invalidate the differentiation.

(3) Cf. now also TALMON, *Textus* 4 (1964) 95 f.

(4) This may, perhaps, be put differently: were we to put medieval and pre-medieval MSS together indiscriminately and have them sorted out according to types of variations, the new typological arrangement would turn out to be identical with the chronological one. Cf. *ISE*, § 17.

(5) As a corollary; because of this, any attempt at ordering all medieval witnesses into families according to stemmatic principles is doomed to failure, just like LAGARDE's attempt to assume a Hebrew archetype according to LACHMANN's principles. It may be truly said that the Massoretic activity made the usual criteria inapplicable. Cf. § 17 and n. 2, p. 278. — Cf. also *ISE*, §§ 17, 76. This is not to deny, of course, that individual MSS can be shown to be connected. Cf. already the discussion of DOEDERLEIN, *Auserlesene... Bibliothek* II, 9, 1783, p. 650 f.

(6) Cf. below, § 25 f. The connection would cease to be an illusion only after a specific relationship is proved.

'confirm' (or are confirmed by) the readings of the versions, and those readings have taken their place in the apparatuses next to the retroversions.

23. Having restated the argument against the assumption of genetic relationship between medieval and pre-medieval witnesses as seen from our newly-gained vantage ground (1), we may turn back and look at the attempts during the past fifty years at studying medieval MSS (2). It may be useful to distinguish between two main lines of inquiry: the attempt to detect MSS which deviate in their whole textual structure from the standard text (3) to such a degree that their deviations may be said to point towards extra-massoretic connections, so that these specially selected MSS may serve as the basis for the apparatus of medieval MSS (4), and the attempt to select from all

(1) Cf. above, n. 2, p. 274. In the past years I have repeatedly come back to the investigation of the problem, hoping that more positive results could be gained. The following paragraphs have been written after a fresh analysis of the material adduced in the various studies.

(2) The published studies known to me are (in chronological order: P. VOLZ, *Studien zum Text des Jeremia*, 1920, p. IX f.; J. HEMPEL, *ZAW* 48 (1930) 187 f.; S. H. BLANK, *HUCA* 8 (1932) 229 f.; J. HEMPEL, *ZAW* 52 (1934) 254 f., J. W. WEVERS, *ZAW* 61 (1948) 43 f.; H. GESE, *ZAW* 69 (1957) 55 f.; W. H. BROWNLEE, *The Text of Habakkuk in the Ancient Commentary from Qumran*, 1959, p. 124 f. Available to me are also the following unpublished Princeton dissertations: W. A. BELING, *The Hebrew Variants in the First Book of Samuel compared with the Old Greek Recensions*, 1947; J. F. ARMSTRONG, *A Study of the Alternative Readings in the Hebrew Text of the Book of Isaiah and their Relation to the Old Greek Recensions*, 1958.

(3) For this purpose it makes practically no difference whether we take as 'standard' the text of VAN DER HOOGHT used by KENNICOTT, the Venice *Biblia Rabbinica*, or the Aleppo Codex or use some statistical mean. Cf. n. 3, p. 278.

(4) This, again, is a restatement of observations such as DOEDERLEIN, *Auserlesene... Bibliothek* II, 9, 1783, p. 641 f.: "Kennikot [!] hingegen sammlet, ohne den Versuch... seine gefundenen Lesarten zu vergleichen oder das Gewicht seiner Handschriften abzuwägen". Cf. below, n. 1, p. 278. The first to restate this clearly is HEMPEL (*ZAW* 48 [1930] 195): "es ist unzulässig, ohne Prüfung des Characters der betreffenden Handschrift eine innermasoretische Abweichung kritisch zu verwerten oder auch nur mitzuverwerten" Cf. below, n. 1, p. 279. This is echoed more weakly by VOLZ, *ZAW* 54 (1936) 100 f. Since "Die Jesaiah-Rolle und das

the medieval readings taken together those that formally agree with readings in pre-medieval sources so that genetic relations are allegedly proved (1).

24. The analysis of all the studies which pursue the first line of inquiry (2) has yielded an unequivocal result: among all the MSS and fragments known so far there is not even one the deviations of which can be significantly connected with any non-Massoretic tradition (3). We possess no medieval manuscript (4) which, on the strength of its readings, may be termed 'valuable' or be worthy of our attention more than any other. Having attempted to come to an 'evaluation' of MSS (5) in order to fulfil the demand that MSS should not be counted

---

Problem der hebräischen Bibelhandschriften" (1954) I have tried to argue that the problems of the textual character of the apparently deviating MSS as a whole and of the typology of changes must be investigated before any further step in this field can be made.

(1) The second line of approach is basically the one usual in KENNICOTT's times, whereas the first one is methodologically novel — in spite of the fact that the most deviating MSS were, of course, noticed by KENNICOTT, BRUNS, etc. I need not add that I use the term 'structure' in this context as against 'atomistic' comparisons of readings; cf. *TL*, index. Since I am not well enough acquainted with the procedures in N.T. studies, I can only register that also in N.T. textual criticism the evaluation of readings as part of a textual structure is mentioned as a novel approach; cf. K. W. CLARK, *JBL* 85 (1966) 16.

(2) This includes the published studies by HEMPEL, GESE and myself as well as the work carried out at the HUBP. VOLZ did not really pursue this line, and BLANK only touched upon the problem by the way. Nevertheless, his study serves to point out — much against his own intentions — that the MSS wrongly attributed by KAHLE to BEN-NAFTALI (cf. *RTBT*, Ch. VI) do not stand apart from the textual side. For the problem of publishing all the details on which this summary is based; cf. above, n. **1**, p. **243**.

(3) My attempts to ' salvage' even one single MS onto which others could be pegged have proved abortive. Cf. *ISE*, § 77.

(4) Caution bids us say that none has survived, but I doubt very much whether there was ever any such MS. A chapter explaining the rarity of medieval Hebrew codices has been included in many studies since the days of WALTON; cf., e.g., STRACK, *Prolegomena Critica*, 1873, § 6. See also DE ROSSI, *Scholia Critica*, 1798, § 15 ("unde antiquissimorum codicum hebr. summa raritas").

(5) Our tabulations are based both on quantity and on typology.

but weighed (1), we are finally driven to admit that the history of the transmission of Hebrew medieval MSS is such that this famous rule is inapplicable (2). There exists no MS which deserves special attention in the sense that it 'weighs' more; there are only MSS with smaller or larger numbers of readings (3). But as one starts to investigate them, they melt into nothing (4), and the huge mass of variations does not finally yield a single variant which is significantly, decisively and undoubtedly connected with a pre-medieval tradition.

(1) The weakness of ' counting' readings was stressed two centuries ago, especially by those scholars who strongly opposed KENNICOTT's work; cf. O. G. TYCHSEN, *Tentamen*, 1772, p. 134 f. *Befreyetes Tentamen*, 1774, p. 150 f. See also H. L. STRACK, *Sem. Stud... Kohut*, p. 561 and above n. **4**, p. **276**.

(2) This is, in fact, also the result of H. GESE, *ZAW* 69, p. 66. Having written against the undiscriminating use of medieval MSS (cf. *TL*, p. xf.; *ISE*, § 18), I am forced to admit that I now doubt even more whether after the 'discrimination' there remains anything to use. This is, again, the outcome of the history of medieval MSS under the influence of Massoretic activity, and again these MSS defy the application of the text-critical rules of the game. Cf. above, § **17**, and n. **5**, p. **275**.

(3) For the standard, cf. above, n. **3**, p. **276**. It is with grave misgivings that I approach the conclusion that statements made in KENNICOTT's times to the effect that we possess only MSS with more or less scribal mistakes and that finally the *textus receptus* is our only yardstick, are not really so far off the mark, although they seem to carry us back to 'Buxtorfian fundamentalism'. Cf. above nn. **3** f., p. **253**. Now, that we can check the ninth and tenth century model codices, such a statement does not look as preposterous as it did when the attack on the *textus receptus* was in full swing. Again, I suspect that in the deprecation of the *textus receptus* there was also a good deal of carry over from N.T. studies. Cf. the somewhat similar remark in *RTBT*, § 44, and my cautionary note below, n. **3**, p. **289**. Were I not afraid of overstating my case, I would consider a very provocative formulation, i.e., that in our case the *textus receptus* functions practically like the 'archetype' and that we might disregard the MSS by way of *eliminatio codicum descriptorum*. This sounds, of course, like a *reductio ad absurdum* of the Lachmannian method Cf. below, n. **3**, p. **283**.

(4) Cf. now the result of GESE (above, n. 2) which should be seen in the light of HEMPEL, *ZAW* 1930, p. 193; 1934, p. 273, i.e., that those MSS which seem to stand out at first sight turn out to have the greatest number of obvious mistakes. HEMPEL's result with regard to K 69 in Deuteronomy, which only at first sight seems to have affinities with the Samaritan text, is borne out by our investigations of Genesis and Exodus.

25. In order to enable others to judge for themselves, a full new collation (1) of the MSS (2) which seemed most 'promising' is being published in the HUBP edition — considerably fuller than the collation published by Kennicott, not least because the vocalization and the differences of hands have been noted. From the thousands of readings checked from K 30 93 96 150 (3), it is difficult to 'salvage' a handful which might be possibly more than the result of harmonization etc. etc. — certainly a far cry from the claim of special value. Thus we get as the yield from Isaiah (4):

7,8	– MT	ובעוד ששים וחמש שנה
	K96	עשרים
11,6	– MT	ועגל וכפיר ומריא
	K30 (5)	+ ואריה
13,3	– MT	גם קראתי גבורי לאפי
	K96	ביכורי (?)

(1) Cf. for this issue, H. S. Nyberg, *ZAW* 52 (1934) 244 and see above, n. 4, p. 276.

(2) As against the more extreme suggestion *TL*, p. xi, four MSS were finally included. As long as we find it necessary to adopt this system, the details of the MSS to be chosen may vary from book to book. Cf. below, § 30 and n. 4, p. 283.

(3) The general character of these MSS had been noticed by Kennicott, Bruns, Doederlein, etc. It is interesting to remember that K93 was once said to be a MS written by a Gentile and that on the strength of the judgment of H. Opitz, K150 was thought to represent the 'true' MT until Kennicott, *Diss. General.*, 1780, p. 83. After prolonged examinations of K96 and 150 I now doubt — against *TL*, p. xi — whether K150 is really the more noteworthy of the two. But the initial impression (*TL*, p. 56), that K96 150 should be studied in the first place, has been confirmed.

(4) The present list includes those places where an explanation of the reading from context, parallel, etc. does not obviously suggest itself, without limiting the examples to cases where similar extra-Hebrew readings exist. It thus specifically excludes cases such as were mentioned as illustrative of the 'law of scribes' (cf. above, n. 1, p. 275), but may include what is actually a scribal mistake. On rechecking the collation of K–R others may, of course, find, an instance to add or to omit.

(5) Details as to second hands, lack of vocalization, etc., are not taken into consideration on this occasion.

36,17	– MT	ארץ לחם וכרמים
	K30	+ ומים (1)
44,14	– MT	לכרת לו ארזים
	K96	אלהים (2)
51,6	– MT	וצדקתי לא תחת
	K150	תאחר (3)

From the whole book of Isaiah one can quote only one variant which makes one really prick up one's ears:

18,4	– MT	כחם צח עלי אור כעב טל בחם קציר
	K30	ביום

This reading occurs in about a dozen other MSS as well as LXX, Pesh, Vulg.; and while the graphic similarities are such that the variant could have arisen again independantly in medieval MSS, the case remains remarkable (4).

26. It may be of some interest to mention in this connection some provisional results of our study of Geniza fragments (5). First and foremost: leaving aside the mass of 'pseudo-readings' which are connected with different methods of vocalization (6), the overall picture of the text in Geniza fragments is completely identical with that of the codices collated for K–R etc. The Geniza has not turned out to have been in any way a repository for aberrant texts which were,

(1) This case should probably be omitted, both because of the idiomatic character and because of the letters of כרמים.

(2) LXX: ὁ ἔκοψε ξύλον ἐκ τοῦ δρυμοῦ, ὁ ἐφύτευσε κύριος... is no more than an interesting parallel to the same train of thought.

(3) Cf. *ISE* app. *ad. loc.* This is the only case which may remain from K150. But this one too should probably be weeded out; cf. Is 46,13.

(4) This is the only instance in the entire book which fulfils the conditions which would make it a candidate for the 'extramassoretic trickle' (cf. below, § 29). The question of its 'value' *per se* is outside the scope of this discussion.

(5) The hopes expressed in *TL*, p. xi had already been damped in *BMU*, Ch. II.

(6) It is hoped that we shall be able, before long, to publish a study exemplifying the various types of vocalized texts using Tiberian vowel graphemes.

allegedly, condemned to oblivion. Since earlier studies of Geniza material were not concerned with the problem of variant readings and no systematic inquiry had been carried out, there was some hope that Geniza fragments might help in clarifying our ideas about the 'Massoretic recension'. This hope must be given up, although we may always run into a fragment which at first sight seems to contain a large number of readings (1). Though not all the Geniza fragments are known to us as yet (2), the following readings from Isaiah, culled from a few hundred fragments of this book checked so far, may illustrate our point. Leaving aside variants of Divine names and of the types מן מצרים / ממצרים , אל / על , ־יך / ־ך (3) as well as omissions, homoioteleuta, etc., we are faced with variants which can arise again and again. Thus (4):

22,12 – MT		ולקרחה ולחגר שק
F (5)		ולחגרת
37,26 – MT		הלא שמעת למרחוק אותה עשיתי מימי קדם
F		למימי
42,3 – MT		לאמת יוציא משפט
F		משפט יוציא
47,5 – MT		שבי דומם ובאי בחשך
F		*om.*
52,10 – MT		וחפץ ה' בידו יצלח
F		יצליח

(1) The large number of Geniza fragments has helped to enlarge the sum total of MSS, partly or fully preserved, and has thus given us an even broader basis for statements on medieval MSS. Quite often a reading quoted as unique by K–R can now be adduced from a Geniza fragment too. The fragments from 'Listeners' Codices' (*BMU*, p. 39 f.) have not turned out to change the picture. Inversions of textual portions in a very few fragments need further investigation.

(2) This summary is based, of course, on the study of fragments from the entire Bible. Altogether several thousand fragments have been studied.

(3) Cf. above, n. 4, p. 252.

(4) The following are examples of some of the types.

(5) F = Fragments. A system has yet to be devised for quoting the fragments. In the present context the exact shelfmark is given only where the fragment as a whole is of interest.

These instances can be multiplied manifold; they are paralleled by thousands of 'readings' in K–R and amount to nothing. The same is true for variations in vocalization which indicate a different sense (1). Thus, e.g.

18,6 – MT		יֵעָזְבוּ יחדו
F		יַעַזְבוּ
19,22 – MT		ונגף ... נָגֹף ורפוֹא
F		נָגַף וְרָפָא
52,6 – MT		לכן יֵדַע עמי
F		יָדַע

Only one fragment is interesting in this respect, i.e., T-S A 10,5, because it has a considerable number of changes, among them in

52,12 – MT		ולַפִּשְׁעִים יפגיע
F		ולִפְשָׁעִים (2)

Among the simplifications an outstanding instance, hitherto unattested, is

19,4 – MT		וסִכַּרתי את מצרים
F		ומִסַּרְתִּי (3)

We are thus left with the one and only instance of a remarkable variant in the Geniza fragments, and it need hardly be added that this is again ביום instead of בחם in 18,4.

27. For those books of the Bible which have already been checked (4) the results gained for Isaiah are paralleled, and there is no reason to assume that further investigations of MSS and fragments from

(1) These cannot be paralleled, of course, from K and only rarely from R. Cf. above, n. 2, p. 255.

(2) LXX: καὶ διὰ τὰς ἁμαρτίας αὐτῶν παρεδόθη In KENNICOTT's generation such a reading would have been hailed as being of significance.

(3) I cannot help remarking that this reading would have fitted perfectly into 1QIs^{a}.

(4) At the time of writing, complete or exploratory studies have been carried out on the Law and the Prophets. First results of an investigation of books from the Hagiographa indicate that the picture may be different

other books are going to enable us to uphold the notion of a 'valuable' medieval MS (1). Thus the provisional result for Jeremiah is that only two variants from the above named MSS are possibly worthy of note:

27,22 –	MT	בבלה יובאו
	K96	יובלו
48,2 –	MT	חשבו עליה רעה
	K150	מחלמה + (2)

There can be no doubt that our evidence fully upholds the 'one recension' (3) theory. But the existence of MSS like K 30, 93 96. 150 (4)

---

to a very slight degree only. Today it seems that the expression 'weak link' used *ISE*, § 18 is an exaggeration, born out of caution. What remains in medieval MSS to make us suspect a reading as genetically connected with pre-medieval sources is next to nil. I am afraid that with the disappearance of the 'weak link' our view is fully validated and practical consequences will have to be drawn before long.

(1) While studying the so-called 'Psalms Scroll' (11QPs^a) it seemed at first sight as if readings from medieval MSS are uniquely paralleled by the scroll. What finally remained was

Ps 119,2 –	MT	בכל לב ידרשוהו
	11QPs^a = K245	עת

For the problem of readings in that scroll cf. *PSCT*.

(2) Some other MSS read מלחמה instead of רעה. No reading in Jeremiah approaches the 'quality' of ביום quoted above pp. 280, 282 from Isa 18,4.

(3) According to the criteria applied here, the study of the variants — far from overthrowing the Lagardian thesis — comes almost near upholding it. Cf. above, § 17; n. 3, p. 268; n. 3, p. 278. It is rather for theoretical considerations that one is bound to limit LAGARDE's formulation to being only 'almost right'. I would go so far as to suggest that those studies during the past thirty-five years which have attempted to approach the problem by applying what amounts to structural criteria lead *de facto* to this 'almost right' formulation — of course not in the way LAGARDE had pictured the process nor on the strength of his arguments. From a different point of view cf. the summary of HEMPEL, *ZAW* 1930, p. 95: "Mit ungeheurer Zähigkeit hat die Judenschaft der Zeit nach den staatlichen Katastrophen *einen* Text durchgesetzt, so dass nur geringe Spuren der einstigen Differenziertheit dem Schicksal der Ausmerzung entgangen sind. Das ist das Richtige an der These LAGARDE's".

(4) The numbers may change, of course, from book to book. Cf. above, n. 2, p. 279 and below, n. 3, p. 287.

still remains a problem which forces us to tread cautiously. At least as long as we remain in the dark as to the state of MSS between the second and the ninth centuries, more than one explanation may be suggested (1). But no explanation which assumes the existence of what is in substance a greater diversity than can be encompassed within 'one recension' is acceptable.

28. The second line of inquiry (2) — the study and selection of readings as such, regardless of their source — could be hardly expected to yield results different from those suggested by Kennicott and his contemporaries. If every formal agreement between a reading in a Hebrew medieval MS and a pre-medieval source can be valued as indicative of a genetic connection (3), the result can be impressive, indeed. If that procedure is accepted, not only can the 'archetype'

(1) The possible influence of the type of codex — vulgar, private official, etc. — on the text has been suggested repeatedly. Cf., e.g., TYCHSEN, *Befreyetes Tentamen*, p. 159; STRACK, *Prolegomena*, 33; *BMU*, Ch. II. As regards the textual basis — as opposed to vocalization, etc. — no such differentiation emerges from the evidence at our disposal, although 'Listeners' Codices' were usually written with less care. It should be remembered that the four MSS quoted by us do not differ outwardly in any way from hundreds of other 'Massora'- or 'Study'-codices (according to the classification in *BMU*, *loc. cit.*). No evidence could be found for the existence of a vulgar sub-recension suggested by HEMPEL, *ZAW*, 1934, p. 254. In any case, during the last decade we have learned to be very hesitant in talking of 'vulgar' MSS. Cf. already *TL*, p. 67.

(2) It may be more than coincidence that this line of approach was developed in dissertations written under the guidance of H. S. GEHMAN. From the written material it does not seem that the approach taught by GEHMAN is a conscious theoretical renunciation of method, influenced by attitudes like the 'Critique Rationelle' in N.T. research. The remarks of W. H. BROWNLEE (cf. above, n. 2, p. 276) can be hardly termed a contribution to our discussion.

(3) WEVERS, *loc. cit.*, p. 75, maintains expressly "that the Hebrew variants have perpetuated pre-Massoretic traditions which were the basis for certain readings in G and the later Greek recensions". For BELING the comparison with the versions is sufficient proof "that these Hebrew variants from the Massoretic text in I Samuel are actually pre-Massoretic". NORTH (cf. above, n. 2, p. 253) goes so far as to accept any Hebrew variant for his exegetical needs, without any corroboration from the versions, provided it meets the following conditions: it is contextually not less difficult than the received text, it seems not to be attributable to scribal error and it is explicable.

theory be disproved (1), but even the 'one recension' theory can be made to look questionable. We have as yet only one objective way to declare out of court the evidence gained by such indiscriminate hunting for readings (2): Our newly gained insight into the typological limitation of readings in medieval MSS as opposed to pre-medieval witnesses (3). But even if we admit this yardstick and accept the conclusion that all medieval MSS must be dealt with as one family, representing one recension or even 'almost' one archetype, there remain those very few instances (4) which make one wonder whether the 'one-recension' central current did not allow the occasional drop to trickle in from the side. It must, however, be stressed that while this remains a possibility, we have not yet encountered even *one* instance where the assumption of a genetic relationship between a MS from the Massoretic period (5) and a pre-medieval source differing significantly from the received text is an absolute necessity (6).

29. This position may be formulated in a slightly different fashion in order to ensure fairness to possible opponents. Almost all our evidence from medieval MSS would be explicable as a secondary

(1) WEVER's dissertation was the first attempt to use the apparent identity of readings in Hebrew MSS with the Greek for disproving the 'archetype' theory. ARMSTRONG in his dissertation sets out to investigate also the truth in the thesis of O. H. BOSTRÖM, *Alternative Readings in the Hebrew Book of Samuel*, 1918. The main point of BOSTRÖM's thesis had also been taken up by ZIMMERMANN (cf. above, n. 1, p. 266) and by WEVERS, *JBL* 65 (1946) 307 f. The whole issue has lately been studied extensively by S. TALMON.

(2) For me, personally, the issue is settled. The problem remains how to prove the case ' objectively'.

(3) Cf. above, § 22.

(4) If we adopt the system of indiscriminate use of all MSS, the number of readings which are apparently connected in a significant way with pre-medieval witnesses is somewhat larger than in our example above, § 25, but incomparably smaller than suggested in the dissertation of WEVERS, etc. The material in *ISE*, Apparatus III may be used for illustrating the question.

(5) For the definition, see *ISE*, § 74e.

(6) It may be useful to remember that, of course, not all the apparently identical readings in a version, on the one hand, and Hebrew MSS from the first or second period, on the other hand, should be rated as genetically connected, the more so since the 'law of scribes' never ceased to be operative. Cf. above, § 22, and already *TL*, p. 67 f.

development from a common archetype (1) and practically all of it as belonging to one 'recension'. Were it not for the disturbing 'almost', the whole chapter on medieval MSS could be regarded as closed and our apparatus be freed from them once and for all. But since scholars, especially during the first half of this century, were justly forced to the conclusion that our witnesses cannot be reduced to one archetype, we cannot be absolutely sure that a few extra-Massoretic variants did not trickle into the central current even after the recensional decisions or standardizations were carried out in the first and/or second centuries C.E. Once this possibility is not absolutely ruled out in theory — though of no consequence in practice — the door is thrown open for those who wish to maintain that we possess no sure yardstick to decide, whether in a given case we can unreservedly deny that formal identity between readings may be interpreted as genetic relationship. It lies in the nature of the subject that the case against genetic relationship is based on the analysis of the evidence as a whole and that those who wish to ignore the typological proof mentioned above may use that apparent excuse in order to go on declaring as 'valuable' any reading from Kennicott, etc. they fancy. I can do no more than hope that this attempt at pointing out that a tiny remnant of doubt cannot be overcome will not be misused, and that scholars will accept the position advocated here: that the possibility of infiltration of extra-Massoretical variants into the Massoretical 'central current' is so negligible that for all practical purposes it may be disregarded, and that medieval Hebrew MSS are therefore without practical value for any attempt to reach back into the early history of the Bible text (2).

30. Yet another point must be mentioned to which insufficient attention has been paid. Since no one in this century has independently worked through the evidence from medieval MSS for the whole

(1) For the present argument 'archetype' may be understood in the sense of a master model codex, the tradition of which served as a basis for correcting other MSS.

(2) Anyone who wishes to base a reconstruction of the Bible text on Hebrew medieval variants must offer specific proof for his procedure. But since it is inevitable that a considerable time will elapse before the view outlined here is more widely accepted, fellow students would justifiably find fault with an edition that completely ignores the Hebrew evidence and thus does not serve scholars of different persuasions and demands. Cf. above, § 7 and below, n. 1, p. 290.

Bible, it is inevitable that scholars tend to take the results gained in the analysis of one book as indicative of the state in the entire Bible (1). In other fields of inquiry into the Bible text it has become customary in recent years to sound a note of caution and to stress the demand that each book, or group of books, of the Bible ought to be investigated in its own right and that the results gained for one book are not necessarily valid for all the books of the Bible as a whole (2). In our views of the growth of the Hebrew 'Massoretic Text', as gained from the analysis of both medieval and pre-medieval MSS, there is nothing that induces us to assume that the fate of all the books was absolutely identical (3). On the other hand, we have as yet no reason to assume slightly different histories for different books, apart from the obvious slight differences of 'spread' of readings connected with the liturgic position of the books (4). However, the analysis of readings such as in the Book of Kings may at least justify the mentioning of the possibility that different results may be obtained for different books (or parts) (5) of the Bible and that, accordingly, we may have to reckon with different 'breadths' of the 'central current' and different strengths of the 'trickle' from the side. But whatever the possible slight differences in detail — the main conclusion remains.

31. Our inquiry has thus brought us back to the point of departure at the end of what we have termed the second period in the history of Hebrew MSS, i.e., around 100 C.E. While future evidence may show

(1) For the MS basis for our statements, cf. above n. 2, p. 281, and n. 4, p. 282.

(2) Cf. *ISE*, § 10.

(3) For the difference of MSS as regards various books cf. above n. 2, p. 279; n. 4, p. 283. Thus, e.g., for Kings K70 seems to stand out and for 1 Sam also K 89 174 187 have to be considered. Among these is K 89, the alleged age of which has given rise to discussions. But this MS does not stand out at all in Isaiah.

(4) There exists as yet no comprehensive study of the relationship between the 'spread' of variants and the liturgical use of the individual book of the Bible. The relatively stronger standardization of the text of the Peshitta in the Pentateuch is a case in point. Cf. also *TL*, p. 175. In any case, the state in medieval Hebrew MSS is only one aspect of a larger question. On the other hand, Samuel and Kings offer specific problems because of the parallel texts.

(5) WEVERS is cautions enough to add that his results may hold true only as regards the Book of Kings.

that the 'recension' was rather broader than we tend to assume now, and that the end of the second period must be pushed beyond the second century C.E. (1), the analysis of Hebrew medieval MSS allows us at present to identify *grosso modo* the textual tradition of these MSS with that of the 'central current' tradition of *ca* 100 C.E. (2). However, as regards the choice of terms, the term 'recension' has acquired a sense slightly different from that understood in the generation of Rosenmüller. It is thus open to discussion because of the connotation of official interference (3). Since in the past whatever meagre evidence for 'official' approval and disapproval has been blown up into a picture of imposing the 'official' text of the Rabbis by force and consigning all rival texts to extermination (4), it may be more cautions to talk about the 'central current' tradition (5). On the other hand, the type of halachic discussion pursued by the Rabbis in the first century C.E. presupposes one more or less binding text. I prefer, therefore, the picture of a 'main current' tradition, constantly narrowing down (6) until its predominance ('central current') in the first century C.E. was a matter of course. The text used by the Tannaites for halachic discussions was, so to speak, the 'ideal' core of this tradition, with other texts, acceptable just as well to a broader public, differing from it only slightly. The 'official' text was thus part and parcel of a somewhat broader current of textual tradition and served at the same time as a

(1) This possibility is mentioned here in order to stress that the view outlined here allows for marginal changes of facts which may be caused by future surprises. Cf. above, § 6.

(2) No futher intermediary state is theoretically necessary (cf. above, § 6), but a certain breadth of the tradition with some further standardizing remains a possibility, though not on the exaggerated lines suggested by KAHLE.

(3) Cf. *ISE*, Ch. I, nn. 12, 15. The idea of official pressure appears in various descriptions under various guises. Cf., on the one hand, the statements mentioned above, n. 1, p. 265 and, on the other hand, e.g. VOLZ, *Studien zum Text des Jeremia*, 1920, p. XIV, who talks about the 'amtliche Arbeitsplan der Synagogue'. See also *ZAW* 54 (1936) 104 f. My resistance to previous exaggerations should not be interpreted as denying any 'officiality' altogether. Cf. *RTBT*, § 26.

(4) Cf. above, n. 1, p. 265, and *RTBT*, § 23 f.

(5) I am ready to accept any better term, if it helps to express the issue more clearly.

(6) Possibly with differences as regards different books; Cf. above, § 30.

centre for further standardization and unification. For binding halachic or liturgic (1) purposes there was thus, in fact, one model text; that text became the 'ideal' centre of the tradition out of which it had grown, together with which it continued to exist (2), and which it re-influenced to the point of creating the illusion of there having been 'almost' an archetype (3).

32. I trust that the preceding discussion has shown how the investigation of the problem connected with the arrangement of the apparatus of the HUBP edition has led us to suggest a theory which accounts for the facts known so far, and how the theory has become inter-

(1) See above, n. 5 f., p. 246. For the view of R. GORDIS, which takes into consideration the textual problem with regard to Rabbinic discussion, cf. above, n. 4, p. 269. Cf. also the views of S. TALMON, *Textus* 2 (1962) 14 f.; 4 (1964) 126 f. The Rabbinic discussion is such as presupposes *one* text, and the textual questions mentioned in it concern mostly *plene* and defective spellings. On the other hand, the analysis of readings in medieval MSS according to types shows quite clearly that the fluctuation was strongest in matters of copulative *waw*, prepositions, etc., as opposed to 'meaning-words', and with regard to the former full unification within the recension was probably never achieved. Hence the difficulty of dealing with these elements in the apparatus; cf. *TL*, p. 59 (which today I would formulate somewhat differently).

(2) To put it differently. Precisely because the differences had already become minimal, the texts could co-exist, until the few 'extra-central current' variants were finally swallowed up in the MSS or practically drowned in the mass of new secondary variants.

(3) Talking about LAGARDE and KAHLE above, n. 1, p. 257, and n. 1, p. 272) I have remarked on the tendency of scholars to advance the same type of explanation repeatedly for different sets of problems. KAHLE's way of solving the problems in various fields of biblical textual studies by a 'pluralistic' theory is an outstanding example; cf. *TL*, p. 66 f. The main doubt in the correctness of my views remains that I, likewise, try to understand the development of the consonantal *textus receptus* on lines similar to those of my view on the development of a *textus receptus* of vocalization and accentuation, as outlined in *RTBT*. But if no mistakes as to the facts can be pointed out against my theory, I am ready to take the blame that I repeat myself in rejecting the imputation of highhanded arbitrariness of which, in turn, both the Tannaites and the Massoretes have been accused, unless specific and unequivocal proof is offered. If we have to allow for LAGARDE's and KAHLE's personal equations to have influenced their theories, I might as well let myself be influenced by my own. Cf. *TPTC*, n. 18, For a kindred discussion in N.T. textual criticism cf. now K. W. CLARK, *JBL* 85 (1966) 5.

connected with the practice of constructing the apparatus of medieval MSS. The time is not yet ripe and the last remaining queries not yet fully disposed of. Hence an edition of this type cannot altogether dispense with the quotation of readings from medieval MSS (1). The practical solution — a separate apparatus exhibiting both full collations from MSS which appear to be of special interest and all the readings which may conceivably be judged by some scholars to be of value (2) — remains a compromise which, in practice, may not justify all the efforts bestowed on it. (3). I hope this discussion may turn out to be a modest contribution towards the clarification of our views on the history of the Bible text as a whole, and especially on Hebrew MSS — both on the level of suggesting a theory of textual development, and on the practical level of allotting a place to Hebrew MSS within the framework of the new edition of the Bible.

(1) In writing a text-critical study or commentary, unconnected with the HUBP edition, I would not hesitate leaving out those witnesses altogether. Cf. above, n. 2, p. 286.

(2) The separation of the medieval evidence in an apparatus of its own is for me a matter of principle. As for the size, it may be noted that Apparatus III in *ISE* gives the fullest selection to be published since the selection of J. C. DOEDERLEIN and J. H. MEISNER, edited right after the publication of DE ROSSI's collations. Cf. *ISE*, §§ 16, 77.

(3) It has been our endeavour not to evade the problems posed on the theoretical level, even if the practical gain for the understanding of the Bible text is next to nil.