BIBLICAL MANUSCRIPTS IN THE UNITED STATES

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To Martin Plessner
on his sixtieth birthday

I. GENERAL REMARKS

Every subject has its prejudices and professional myths. One of these is the widespread conviction that the United States is not the place to look for Hebrew and Semitic manuscripts. To be sure, the Jewish Theological Seminary in New York is excluded from this generalization and has become a Mecca for manuscript hunters. But for most scholars this seems the one exception to the rule.

This belief is not unjustified. The reason for it is twofold: It is a fact that the great European libraries collected their treasures long before there were suitable institutions in America interested in pursuing the hoarding of MSS as a deliberate policy. Whether through religious or colonial connections, whether by buying large private collections or by receiving estates and donations, the collections from Rome in the south to the British Isles in the north

1. The special abbreviations for this study are as follows:

   A = The Aleppo Codex.
   BH = Biblia Hebraica 3rd edition, Stuttgart 1937, etc.
   EMC = Enelow Memorial Collection, housed in the JTS.
   ENA = Elkan Nathan Adler Collection, housed in the JTS.
   HUBP = Hebrew University Bible Project.
   JTS = Jewish Theological Seminary, New York.
   L = Leningrad Codex of the Old Testament, B 19 a.
   NMT = Goshen-Gottstein, תוספות התוספות הטבריות (to be published in “Publications of the HUBP”).
   TBT = Tiberian Bible Text, i.e. the massoretic text according to the tradition of the Tiberian massoretes which became the textus receptus.
   TL = Goshen-Gottstein, Text and Language in Bible and Qumran, Jerusalem-Tel Aviv 1960.
had hoarded, at a rough estimate, up to ninety percent of their present holdings of Semitic MSS before any American institution had seriously embarked on a similar collection.

Furthermore, the holdings of the major European collections were quite adequately known by the beginning of this century. The Assemani-de Rossi-Kennicott way of description was, to be sure, different from that of the great bibliographers of the second half of the nineteenth century, headed by Stein-schneider (who, by the way, had little love for biblical MSS). But by and large, scholars had sufficient information at their disposal and could get hold of the MSS needed for their work. Every specialist working on a particular subject has, no doubt, encountered faulty descriptions, and has acquired the habit of checking the information offered in the catalogues. But all in all—leaving aside much of the Geniza material—the MSS in European libraries are “known”, and our ideas as to the methods of scribes and trends of textual developments are derived mainly from these MSS.

On the other hand, the MSS in the United States are, generally speaking, terra incognita. The major review of MSS in the USA does not include Semitic MSS, and there exists no census of these. No Semitic collection of any size has ever been described in a full-size catalogue, and only one major Hebrew and two Arabic collections have been made accessible through printed handlists. No list of any Syriac collection is available.

2. Spurred by Díez Macho's discovery of the Palestinian Targum in MS Vat. Neofiti i, which from the description seemed to be a MS of Onqeles, and by my own more modest finds of Syrohexapla material which had passed for Peshitta texts (cf. TL 182, and below, chapter IV), I have, of course, been on the lookout for such miscalifications. But in the case of biblical MSS these are naturally much rarer than in other fields.

3. All our references, if not specified otherwise, are to Hebrew, Aramaic, and Syriac MSS. For Arabic MSS cf. below, notes 6, 27; cf. also note 15.


6. L. Nemoy, “Arabic MSS in the Yale University Library”, Transactions of the Connecticut Academy of Arts and Sciences, 1956; Philip K. Hitti and others, Descriptive Catalogue of the Garrett Collection of Arabic MSS in the Princeton University Library, 1938. Although I believe my information to be correct with regard to Arabic collections in general, I should stress that my interest was strictly limited to Judeo-Arabic (and biblical) texts. Cf. below note 27. It may be added that the part of the collection of MSS of the late Abraham Yahuda now at Princeton does not contain any Judeo-Arabic material, as I gathered from my talk with Dr. R. Mach, who is in charge of cataloguing these MSS.

7. At the request of the Harvard authorities, I am at present preparing a handlist of the Harvard Syriac collection. Cf. Chapter IV.
Although a few libraries have tentative card listings or some other kind of inventory of some of their holdings, scholars are forced to work through the collections by themselves. To be sure, all the directors of the libraries concerned are keenly aware of the situation and are more than liberal in allowing the visitor access to their treasures. But for many years to come, they will hardly be able to change the situation, mainly for financial reasons. This will be understood more readily if we add that the number of Semitic MSS and fragments is, at a very rough guess, high up in the ten-thousands, and that the number of biblical items may be put at about four to five percent of these holdings.

These considerations will suffice to make it clear that the remarks offered here are not in the nature of a full report, nor do they attempt in any way to replace the missing check-lists. These notes deal rather with certain selected problems, and even the many hundreds of MSS and fragments photographed for further examination in connection with the work of the Hebrew University Bible Project (HUBP) still represent a very subjective choice, with a particular purpose in mind.

Nevertheless it is hoped that some of our notes may be found useful, even if only to inform fellow-workers what to expect and what not to expect. At the same time I have used this opportunity to comment on certain more general problems of biblical MSS, drawing also on the outcome of whatever experience I may have gathered in collections outside America.

8. While I am under obligation to the librarians of all the collections in which I was privileged to work, my deepest thanks go to Dr. N. Sarna of the JTS and his staff—especially Misses Kleban and Ginsberg—without whose special interest and courtesy I would not have been able to fulfill my task. I am equally grateful for the courtesy which enables us to reproduce the material.

9. It is useful to remember that the unusually large proportion of fragments—which in all collections are counted as separate items—raises the numbers. Most items in the Hebrew or Syriac catalogues of London or Oxford, for example, are codices; but perhaps half the items kept in the JTS are fragments, separate or loosely bound together.

10. I venture to hope that most of the biblical material which deserves further study has been photographed for more detailed examination at the HUBP. I wonder whether the public collections still hold many surprises. Only the Geniza fragments of the JTS, which need many months of study and deciphering, may still yield further interesting facts.

11. Cf. the opening paragraph of Dizd Macho’s paper in VT Suppl. IV (1957), p. 27. He was interested almost exclusively in non-Tiberian and certain Aramaic MSS.

12. Journeying for thousands of miles in order to see the biblical MSS in the Sutro collection at San Francisco is, indeed, slightly frustrating. Cf. below, and chapter III.

12a. Although these comments reflect my ideas in connection with certain basic assumptions of the new edition of the Bible, they do not necessarily reflect the opinion of the Editorial Board of the HUBP as a whole. Certain points have been summarized here for the English reader from NMT.

13. Many questions of a more general character concerning Bible MSS have been dealt
The examination of MSS in the United States was carried out in my capacity as editor of the HUBP, while I enjoyed the privilege and the pleasure of serving as a Visiting Professor at Brandeis University. Funds for photography were made available mainly through the kind cooperation of Hekhal ha-Sefer. Reproductions of most codices kept in European libraries being available in Jerusalem—for the Russian material cf. RTBT note 23 and below note 28—the editorial board of the HUBP thought it desirable to choose from the American libraries whatever seemed necessary. Since the MSS of the Greek and Latin versions have been investigated within the framework of other projects, my task was practically limited to Semitic texts, or rather to a comparatively small part of the biblical MSS in Hebrew, Aramaic, Syriac and Arabic. In my choice I was guided by the results of pilot studies carried out at the HUBP.

Bearing in mind that the number of Hebrew biblical MSS and fragments dispersed all over the world runs into the ten-thousands, it was evident from the beginning that no useful purpose would be served by aiming at a false comprehensiveness that would make the collations of Kennicott and de Rossi look like children’s exercises (cf. TL x f.). Analyzing the available data from both angles, viz. the character of MSS and variant readings and the development of the Tiberian Bible Text (cf. now RTBT), it became more and more obvious that the only practical way was to find a suitable boundary line. Introducing MSS from beyond that line would merely cause a disproportionate swelling of the apparatus with ever smaller gains. The particular facts of the development of Hebrew Tiberian biblical MSS happen to be such that the famous warning against equating recentiores with deteriores loses almost all its force.

with in RTBT, which was written concurrently with this study and complements it in some of its aspects.
14. I am much obliged to Dr. N. Glatzer, Chairman of the Department of Judaic and Near Eastern Studies at Brandeis University, who was kind enough to adjust my teaching duties in such a way that I could pursue my visits to the various libraries.
15. No Ethiopic MS has been found which was deemed of any importance for our work.
16. I am indebted to Mr. I. Yeivin and Mr. A. Hurvitz of the HUBP, who helped me with the work on Hebrew codices and with the analysis of the data from Kennicott and de Rossi in the light of earlier studies; cf. TL 51 f.; XI. The results of our analysis of data referred to later on may be published in detail at a later date.
17. I need hardly stress that the “philosophy” behind this approach and the method of work in Pro. are identical. Cf. note 20.
18. These are our main concerns in this context. The case of the massoretic stabilization in Syriac MSS (cf. Pro.) is basically similar.
19. This is not to be understood in the sense that a seventeenth-century MS cannot be “superior” to a fifteenth-century one. This would be a “superiority” of quite a different kind. The basic assumption of this famous philological rule-of-thumb is
All our previous investigations have shown that the inclusion of material later than the 13th century would be of very little use, and the hundredfold swelling of the apparatus, beyond any manageable proportions, by including MSS up to the 16th century could never be justified by the possible salvaging of one "real variant" in a thousand inside TBT.\textsuperscript{20} The thirteenth century is the dividing line which emerges from our studies. All the MSS up to that time\textsuperscript{21} were put aside for special examination, while later MSS were photographed only for special reasons. Foremost among these reasons was the interest of Hechal ha-Sefer in various types of MSS, regardless of their textual importance, especially in those which bear testimony to special artistic skill.

Since these are few, some examples may be adduced straight away. MS B 241 of the Hispanic Society of America in New York, written on the eve of the expulsion from Spain, is hardly known among Hebrew scholars\textsuperscript{22} because that collection does not boast other biblical MSS and is not frequented by manuscript hunters. But it is easily the most exquisite Hebrew biblical MS in the States and—without claiming any artistic expertise—I would compare it to B.M. Or. 2626–28. This is the only MS which is coloured and illuminated. All the other MSS which were not primarily photographed for textual reasons, are "only" masterpieces of perfect workmanship. Among these I would count another MS which is a unique Hebrew item in its library, a three-column German MS of the whole Bible in two volumes (Pentateuch with

that the texts involved underwent a stemmatically free development. The time between
the final stabilization out of the \textit{proto-receput} tradition and the invention of printing
and the careful guarding of the text in those centuries precluded any significant deve-
lopment. The study of later MSS according to ethno-geographic lines is interesting
for certain other reasons (cf. below, chapter II note 9), but is of little importance for
our edition of the Bible. Cf. for the problem \textit{TL XI}, and especially \textit{RTBT} chapter VIII.
\textsuperscript{20} Cited in the Index s.v. Variants. I again admit that one "real" variant in a thousand may
escape from our net, and I again maintain that we have to decide through our pilot
studies how close-meshed our net has to be in order to remain useful. Our results
may need adjustment as to the final drawing of the dividing line. But pseudo-
perfectionism spells doom for our editorial work. Cf. \textit{TL XIV}.
\textsuperscript{21} Students of Hebrew MSS will appreciate that I prefer a very liberal interpretation
of that date, which itself allows already a liberal margin. I do not think there is a
scholar alive who can always tell for sure whether a given MS—in square characters—
was written in 1270 or in 1340 C.E. This is not meant as a reflection on palaeography
in general, but on our present state of knowledge in medieval Hebrew palaeography.
I have tried to date the MSS by centuries, but I do not claim to be a more expert
palaeographer than others. Whatever proficiency I possess, is solely the outcome of
handling many thousands of MSS, and my dating is completely impressionistic.
None of the dates suggested are as yet the result of an exact palaeographic evaluation.
\textit{Mutatis mutandis} this holds good for the Syriac material, although Hatch’s \textit{Album
of dated Syriac MSS} (1946) is a useful tool, if used with caution.
\textsuperscript{22} The fact that there exists a MS in that library—without any details—is mentioned by
Diez Macho, \textit{VT Suppl. IV} (1957), p. 27.
Onqelos), written by Yehuda bar Yehuda the Scribe in 1204. This MS is kept in the General Theological Seminary in New York (Access. 9384–9385). Apart from such MSS, interesting mainly for non-textual reasons, all the material chosen for closer examination at the HUBP can be divided into the following groups:

1) Hebrew biblical MSS of TBT and fragments of massoretic character, including Geniza fragments, until the thirteenth century.
2) Non-TBT Hebrew MSS—excluding bowdlerizations of TBT (cf. below, chapter II)—irrespective of time limits.
3) MSS of the Aramaic Targumim (cf. chapter IV).
4) The Version of Saadiah, especially outside the Pentateuch.
5) Syriac manuscripts.

The division among the libraries is roughly as follows. First in Hebrew MSS, by any standards, comes the JTS. While the greatest number of items are, of course, the Geniza fragments, the largest single group of codices in the library are the Yemenite codices, most of which are part of the ENEW COLLECTION. The Sefardi codices are comparatively few, and the "Oriental-Mediterranean", Italian, and German codices are very few indeed.

This ratio among the MSS is very significant, and is obviously quite different from the composition of all European public collections, where Ashkenazi, Sefardi, and (especially in Italy) Italian codices are most prominent. The preponderance of Yemenite MSS in American collections—except Cincinnati—is no coincidence. By the time these collections grew, the Yemenite MSS were the only ones available in large quantities, and consequently America today possesses the largest concentration in public libraries of these MSS (cf. below, chapter III).

23. This MS would in any case have been photographed according to our general guiding lines. For other MSS of this type, cf. below, chapter III, note 9.
24. There is one other library in New York which possesses a sole Hebrew biblical MS: Union Theological College Library, Thompson Case CB 20 B 58 13 — is a 14th-15th century Sefardi MS (Josh.—Esther). It does not seem worthy of any special attention.
25. All MSS mentioned in this study are meant as illustrations of the various questions involved, unless stated otherwise.
26. This includes fragments of the Tiberian "non-receptus" system (cf. below, chapter II, note 34) and of non-Tiberian vocalizations.
27. According to the editorial division of labour, the Arabic versions fall into the province of Prof. C. Rabin (cf. Textus I, 211). While I photographed whatever I thought useful, no details on this subject will be given here.
28. In addition to RTBT note 23, I think it worth while to point out that the unique feature which we may expect in the Firkovich collection is the large amount of "East-Mediterranean" MSS, which are only barely represented in other collections. It is not generally appreciated that Firkovich was the only 19th-century collector who concentrated his efforts on that area. Cf. Strack, Zeitschrift f. d. lutherische Theologie und Kirche 36 (1875), 600.
Second in importance, though perhaps not in number—but very much behind the JTS—I would put the library of the Hebrew Union College in Cincinnati. The biblical MSS in that library do not fall into any groups, but rather seem to have come to the library by individual accessions. Among these, however, are a few early codices worthy of further study.29

No other collection can boast valuable codices, but Columbia University (New York) has at least a number of early fragments.30 Slightly fewer are found in the Philadelphia libraries. None of these have codices, but both Dropsie College and the Museum of the University of Pennsylvania (in its Egyptian department) possess a small number of biblical Geniza fragments (cf. below, chapter II note 6). No biblical material is to be found among the 30 fragments formerly belonging to David W. Amram, now kept in the Rare Collection of the University of Pennsylvania Library. Yale, Harvard, and the Library of Congress31 are practically barren.32

It is not without reason that I have not mentioned so far a collection which in size is third, if not second, in the country. The Sutro Collection (cf. above, note 12), now housed in the Catholic University of San Francisco, is perhaps so disappointing because of the high-pinned hopes of the visitor. The biblical part of this collection—the only one of interest to us—is practically worthless. It consists33 of dozens of the well-known 15th-18th century Yemenite scrolls and buff paper codices which—according to the information in the handwritten list available locally (see next note)—were sold by a smart Jerusalem bookseller at the end of the last century to the American millionaire Sutro (who either believed he acted as a philanthropist or else thought he was buying a great treasure).34

29. I should like to use this occasion to thank Dr. D. Yarden, who is in charge of the MSS collection in Cincinnati, for his kind assistance during my work there.
30. It is perhaps not too well known that Columbia University Library possesses quite a large collection of Hebrew MSS.
31. At the time of writing I have not checked the holdings of Yale. I am indebted to Dr. Nemoy for his kind information with regard to his department.
32. But cf. below, chapter IV, with regard to the Harvard Syriac collection.
33. Apart from its Yemenite Torah scrolls.
34. Since some handbooks still mention the alleged existence of the authentic Torah scroll of Maimonides in that collection (for the matter cf. Auth. note 1), it should be made clear that this was a figment of the imagination of the first curator of that collection, S. Rubin. From his notes there remains little doubt that on seeing a scroll—a 16th-17th century Yemenite Torah scroll—written according to the Hala-
khah as stated by Maimonides (the like of which he apparently had never encountered before), he concluded simply that this must be the scroll of Maimonides. By the way, the Yemenite codices kept there afford additional examples of the fact that Yemenite MSS of the Pentateuch had the Song of Moses (Deut. xxiii) written in 67 lines. Cf. for the matter Auth. note 102a. This fact as such has been correctly noticed by the compilers of the latest handwritten list of the holdings of that collection, J. Sal-
omon, J. Friedman, and J. Davidson.
Only students of Yemenite codices will be interested in this collection, or for that matter in most of the Enlow Collection of the JTS. The textual critic will be rather less interested in these collections except possibly for the research outlined below (ch. II, note 9).

From our short introduction it is obvious that the central position accorded to the library of the JTS is, indeed, beyond dispute. But there is much in other collections which scholars can ignore only at their peril.

II. Geniza Fragments

Since the discovery of the Geniza, work has been carried out on the various types of fragments according to their subject matter: Halakha, Poetry, History, Piyut, Kabbala, Grammar, Law, Economics, etc. One field has been practically neglected: the biblical fragments. This certainly sounds unbelievable, particularly if we think of how Kahle's book, entitled *The Cairo Geniza* (2nd ed., 1959), largely neglects the subject announced in its title and deals almost exclusively with biblical material (even though only a small part of it has any connection with the Geniza; cf. *RTBT*, note 110).

The solution is simple. When the Cairo Geniza started to become the pet subject of scholars, they were naturally interested in material up to then unknown. Almost every subject offered novel material; Bible fragments seemed uninteresting. It was only the fragments with non-TBT vocalization that aroused the curiosity of scholars. Working on biblical Geniza fragments meant: looking for non-TBT vocalization.

This was understandable, and under the circumstances it was the right thing to do sixty years ago. The gains in our knowledge of Hebrew derived from those fragments are permanent, and we are all grateful for the pioneer work carried out by scholars of the last generation, first and foremost to Kahle (cf. *RTBT* §6). On the other hand, this complete neglect of 95% of the biblical Geniza fragments—according to my estimate there are about 10,000 such fragments, including the very small ones (cf. *RTBT* note 20)—has in my submission contributed considerably to the hitherto unchallenged formulation of doubtful theories on the nature of our TBT. To my knowledge, no one has ever studied

35. For the Yemenite codices cf. chapter III.
1. It is not by chance that in Goltein's recent survey of the subject (RÉJ 3me sér., 1 (1960), 1 f.) this field is not mentioned. (Cf. now also N. Allony, *Aresheth* 3 (1961), 395 f.)
2. This has sometimes led to the slightly irritating result that a scholar who had arranged certain fragments in a box according to subject matter would find on his next visit to that library that the order had been disturbed and fragments displaced by a "vocalization hunter".
the types and characteristics of “Tiberian” Geniza fragments, and only very recently was a first attempt made to analyze a few fragments for their textual implications.4

“Geniza-fragments” are, to begin with, far from uniform. In fact, it makes as much sense to talk of “the reading of Geniza fragments” as it makes to talk of “the reading in a Hebrew manuscript” (cf. TL 52). Quoting a reading without paying attention to its source is completely meaningless and runs counter to the most elementary philological principles.5

For the purposes of our future reference to Geniza material, I should like therefore to attempt a basic, and very rough, classification of the fragments we find in the JTS Geniza collection.6 We distinguish the following basic types7:

a) Fragments of Massora Codices. These are typologically identical with the material of most biblical codices in all libraries. Just as we possess codices of the early TBT—partly very similar to the sub-system of Aaron Ben Asher

4. Hempel, Nachrichten der Akademie der Wissenschaften in Goettingen Phil. – Hist. Klasse 1959, p. 207 f., analyzed the data supplied by Mr. Rüger. This is not the place for a detailed review of his methods. My main criticism is implicit in what follows here. I wonder whether the major fault is not the fact that Hempel apparently never handled Geniza fragments himself in any sizeable amount, but simply analyzed collations supplied to him. Only thus was there any need to reach the final “result” (p. 235): “Man sieht, wie verschiedenartig die in einer und derselben Synagoge vorhandenen Handschriften sogar der Thora gewesen sind.... Auch unter den Schreibern von Thorahandschriften gab es sorgfältige und ungenaue....” His lack of any classification or identification of the documents used makes his study rather useless. Cf. below, notes 23, 27.

5. It is perhaps not superfluous to point out that the same scholars who permit themselves this treatment of Hebrew MSS would never dream of handling a Greek text this way. For my criticism of Kahle’s cavalier method of dating fragments with Palestinian vocalization, see RTBT note 110. It is obvious to anyone who has handled these MSS that it is often difficult to come to a clear-cut decision without a margin of error of a century or so. Cf. above, Chapter I, note 21. This is the case with codices and even more so with fragments. However, the main types can be clearly differentiated.

6. Others may have shared the experience of chasing Geniza fragments one by one in other libraries, while getting tired of the embarras de richesse in the JTS, or, for that matter, of Cambridge. Cf. chapter I, note 10. All the fragments photographed until now will be examined in greater detail at the HUBP; JTS 501-512 are to be singled out for special mention meanwhile. I have tried not to mention fragments announced by others. But the different systems of referring to them—JTS listings and old and changed ENA listings—may have caused some duplication which will be cleared up in subsequent examination.

7. After studying, however cursorily, thousands of fragments in other Geniza collections, I should think that the classification is valid for the Geniza material as a whole. Apart from the Antonin collection, the only large corpus of fragments which has not been examined is the material still packed in the large wooden crates at Cambridge (lately referred to as “New Series”).
and partly less similar, just as we possess “Sefardi”, “Yemenite”, “Oriental” etc. codices, so we find in the Geniza fragments of exactly those types. These are here termed Massora Codices in order to indicate that they are meant to reproduce what according to the tradition of professional scribes—at that time and at that place—was taken to be the exact and “official” text. Their distinctive signs are professional penmanship, full vocalization and accentuation, and massoretic marginal notes (hence the name).

The decisive fact for our evaluation of readings is that these Geniza fragments are a minority within the Geniza. I would judge them to constitute about 15-20% of all the biblical fragments. Since the non-Yemenite Massora Codices were overwhelmingly parchment codices (cf. chapter III), they have been preserved somewhat better than other fragments. In other words, originally these must have been about 10-15% of the biblical MSS in use at the time.11

I would suggest a rather simple reason for this proportion. Not only were the few prestige model codices for scribes expensive, especially if they covered the whole Bible (cf. RTBT § 14). But any kind of full-fledged parchment Massora Codex took a long time to prepare, and such were perforce owned only by few individuals. To be sure, most members of the community possessed at least the text of the Pentateuch; but these were not Massora codices (cf. below). For every ten “Bible texts”, there may have been one such codex, if indeed there were as many as that. In general only the very rich or the very learned among the congregation (or community, etc.) took the trouble to commission or acquire such an expensive MS.

Because of their special value, these codices were guarded much more carefully than the rest of the “Bible MSS”, and collectors from Renaissance times onward were naturally interested in this type of codex almost exclusively. This leads to an important corollary: these codices are the absolute majority in the holdings of libraries, and since the times of Kennicott and de Rossi it is upon this type that our ideas regarding “Tiberian Bible Texts” have been modelled.

8. Cf. RTBT § 42 f. I refer here roughly to the range of sub-systems from the Aleppo Codex on the one hand to the Cairo Codex and Sassoon 507 on the other. Cf. below, note 33.

9. The term “Oriental” is not good. “East Mediterranean” would be better, although not exact enough. It has never been investigated to what degree the possible palaeographical differentiation—Sefardi, North African, Egyptian-Palestinian, Persian, Yemenite, Greek-Turkish, Italian, German—corresponds to slightly different textual traditions within TBT. It is doubtful whether it is worth while to invest the energy needed for such an investigation; but it is a possible subject for future research. Cf. above, chapter I, note 19, and chapter III.

10. Not to be confused with “massoretic”.

11. As a very general impression, I would add that the JTS Geniza has a slightly higher percentage of paper fragments, which may reflect the slightly higher percentage of Yemenite MSS. Cf. chapter III, note 14.
Within the Geniza, however, these are a small minority. It goes without saying that only these fragments can be compared for their system and value to most of the codices known to us. Measuring the majority of Geniza fragments by that yardstick must necessarily produce absurd results.

This "value" judgment concerning the said type of Geniza fragments is, of course, of a general nature. It is meant to indicate that the analysis of those fragments undertaken up to now does not bear out either of two possible contrary and equally wrong assumptions: Geniza fragments (of this type) are not per se more valuable because they are fragments. The fact that a certain MS is described as a Geniza MS does not automatically raise its prestige.

But on the other hand these fragments are not less valuable either. There is the occasional fragment which may have been discarded because of certain mistakes. But we had better remember the rules about the fitness of Scrolls were never applicable to codices. Apart from certain rules about the Divine name, no mistake rendered a codex "unfit" (unless the scribe was so incompetent that he could not sell the codex?). The analysis of thousands of fragments—not only in America—does not confirm any hypothesis\(^\text{12}\) that fragments came to the Geniza because of their textual state. In other words, the testimony value of a fragment of a Massora codex is equal to that of the parallel part of a comparable codex without colophon.

Good examples of early fragments of this type are JTS 226\(^\text{13}\), one of the earliest TBT fragments with continuous leaves in all Geniza collections, containing chapters from 1 Kings, five leaves, three cols. It is an "Early Receptus" type, characteristic of the smooth transition from the "Proto-Receptus" tradition, just like B.M. Or. 4445, for instance.\(^\text{14}\)

JTS 222 (= ENA 2105) is a fragment of Haftaroth according to the triennial cycle, rather similar in character and slightly younger. Another illustration of this type is University of Pennsylvania Museum E 16512 (Ex. xxi-xxiii).\(^\text{15}\)

While this is not a full listing of early fragments, it gives some idea of the scarcity of 11th-12th century fragments as compared with the much larger amount from the 13th century onward (cf. above, note 5, and below, chapter III, for the codices).

\(b\) Fragments of Study Codices. The main difference between this group and the first is the practically complete absence of massoretic annotation. The term is meant to indicate that this codex was not meant for "professional" usage

\(^{12}\) Cf. my remarks against Kahle, \textit{RTBT}, note 48.

\(^{13}\) Wherever possible, ENA numbers are given. But not all Geniza fragments have numbers of the ENA catalogue. EMC numbers were never published, and have been given here only in certain cases, in order to facilitate identification.

\(^{14}\) For the theoretical discussion, see \textit{RTBT}, especially § 42 ff.

\(^{15}\) For that collection in general, see Goltein, JQR 49 (1958), 39 ff.
or to serve as an exact model to be referred to in cases of doubt (e.g. when there was a suspected mistake in a Torah scroll) but was used for learning and for study purposes in general. It was much cheaper to prepare than a Massora codex, and if we may judge by the execution, it often was not prepared by professional scribes. Nevertheless it was exact enough for the general needs of study, although the lack of the particular care bestowed on Massora codices is felt in the higher amount of slight errors. It was always vocalized and generally, but not necessarily, accentuated.

Since many codices of this type were also written on parchment, and many later collectors collected this type, too, we find it in many libraries, although it is almost always outnumbered by the Massora codices, for the reasons mentioned above. Among the Geniza fragments I would estimate that these make up about a third of the whole number, thus many more than the Massora codices.

Again the reason is not too difficult to find. Codices of the first type—excluding Yemenite codices, as has been said before—were seldom written on paper. The very care necessarily bestowed on such a MS made parchment (or leather) the natural choice. Study codices were more often written on paper. These hardly survived as codices in collections; but they came to the Geniza.

Any classification is bound to run into difficulties with certain borderline specimens. “Study codices” written on paper are often indistinguishable from the third type of fragments (cf. below). In general, the difference will be marked by the square character employed in study codices, as opposed to the possibility of semi-square or even “rabbinic”, etc., characters in fragments of the third type, by the quality of the penmanship and by the degree of “accuracy” in the TBT vocalization (cf. below). In other words, some study codices could be easily converted into Massora codices; they might even have been intended for that use in the beginning. Others are non-professional and not too exact, and border on the third type of fragments, the majority among all biblical Geniza fragments.

c) Fragments of Listeners' Codices. But for the fact that the biblical Geniza fragments remained neglected until now and that the problems of Hebrew

16. It is not only our own prejudice that we think of the three different graphic entities—consor: -letters, vowels, accents—as written down by the scribes in that particular order. No study on scribal habits with regard to this point has been carried out. My impression is that a biblical text accentuated but not vocalized is rather rare. The fragment Dropsie Halper 21 (Sefardi, 14th century) is an example of this possibility.
17. Plates exemplifying the different types will be published in NMT.
18. The term “Listeners’ Codices” or “Private Codices” is meant to indicate the use of these MSS. Some readers may appreciate that I would have found “Baalbatish” a very good term.
Bible MSS were studied in abstratto, without taking into consideration the everyday life of Medieval Jewish communities\(^\text{19}\), it would long have been realized that the problem of biblical Geniza fragments is largely the problem of our understanding of this type of fragments, which form about half of all the fragments, if not more. The bulk of biblical MSS were written “wholesale” by scribes—or else not too carefully by the future users themselves—for liturgical needs. They were not written as “sacred copies” but were meant for everyday use. They were not written in order to please future hunters of variant readings and do not represent in any way—as far as our analysis indicates—any hidden stream of tradition which remained, so to speak, outside the domain of TBT.

Because of certain inherent differences—also on the demographic, educational, etc., level—no useful purpose is served by calling them “popular” texts, bearing in mind the question of a MS like 1QI\(^\text{a}\). Perhaps we might venture to speak of “bowdlerized” TBT texts. There is perhaps still one chance in a thousand that any “deviation” might turn out to be something else than either the outcome of non-TBT reading habits (“Sefardi”, “Yemenite”, etc.) or simple bowdlerization because of lack of care or ignorance. Counting, e.g., the added or omitted matres lectionis in this kind of fragment, as Hempel has done (cf. above, note 4) is a waste of effort and is only misleading. After handling thousands of these fragments, it becomes obvious that we should not expect these “private” codices to reflect the exact spelling of a model receptus text any more than we would of a biblical quotation in a non-biblical text.\(^\text{20}\)

In quality these are the same differences that we encounter even in Massora and Study codices (cf. TL 43 f., 51 f.); in quantity per unit they outnumber them by far.

This phenomenon is quite natural if we bear in mind what these MSS were intended for. The recital of portions of the Bible was a central part of worship. For halakhah it was solely the reading from the scroll by the “Reader” that was important. It was not the reading of the congregant but the listening that counted. Many of the congregants, however, followed the reading in their own private copies, as is the practice in most places today.

Massora or Study codices would have been incongruous for this purpose (and probably very unhandy), even supposing they had been available. The

\(^{19}\) Cf. for other aspects of this attitude RTBT § 47.

\(^{20}\) Just as in my submission it is not permissible to use the ancient Bible Versions for text-critical purposes without being aware of the rabbinic material on the subject (cf. TL XI f.), so it is misleading to handle collations of such “biblical MSS” without seeking acquaintance with the problems of Hebrew MSS in general. Cf. for this problem TL 48 and “Medieval Translations and Translators, iv” (Tarbis, 30 (1961), 386).
Geniza discloses clearly what was used: the "hand copy" contained mostly one book of the Pentateuch (or the Psalms, etc.), and often one weekly portion only.21 In fact, the liturgical use (or lack of use) of a certain book of the Bible is strongly reflected in the amount of Geniza fragments of each book, so that, e.g., fragments from Ezra or Chronicles are rather rare.22 It is this habit that accounts for the small-size paper fragments which abound in the Geniza. If we wanted to press our point, we might say that these were little more than "hearing aids".23 They were not only written without massoretic notes, but each scribe—or rather copyist—copied the text with just as much exactitude as he wanted, and mostly did not take great care with the vowels and accents.24 This freedom in copying out these texts is of vital importance for our understanding of Hebrew reading traditions and linguistic habits (cf. RTBT § 6). But its textual importance is, as a rule, to be doubted, unless specifically proved for a given fragment.

In another context I have mentioned that we might possibly hope for some light from the study of the Geniza fragments, although it seemed very doubtful.25 I now feel even more that we would end up in a vicious circle, because deviations from TBT—in quantity and substance beyond the types known from Kennicott-de Rossi—will turn up precisely in these fragments.26 I am fully aware that what looks like bowdlerization may hide an "unpolluted ancient tradition". But the very character of these fragments as a whole makes it im-

21. Yalon (Kiriath Sefer 30 (1955), 257 f. and Perez Castro (Sefarad 15 (1955),3 f.) have dealt with MS Heb 8° 2283 of the National and University Library, Jerusalem. This is a good example of a "Portion" MS, though in this case it is a Massora codex (a rare phenomenon).
22. I understand from S.D. Goitein (cf. RTBT note 29) that the examination of booklists and documents in the documentary Geniza proves this impression to be correct.
23. In RTBT chapter IV, I have attempted to deal with the problem of oral tradition in textual criticism. In a way, these "Private Codices" are also to be understood in a similar light. There simply was no need for exactness, and it is misleading to subsume all written documents showing some text from the Pentateuch under the term "Thora handschriften" (cf. above, note 4). Anyone could copy the text on some piece of paper without vowels, with partial vocalization, etc., according to his needs. In law, he had to write for himself—or commission—a scroll of the Pentateuch which had to be an exact "Thora handschrift". But there is no injunction against such "baal batish" copies.
24. Often one feels tempted to think that the writer was content with a "broad phonetic notation" to help him read—almost like children's writing exercises.
25. In TL XI, I pleaded for a study of Geniza fragments beyond the limit of the 13th century set for codices, but added: "I would not be surprised if nothing came out in this case too—beyond strengthening the old hypothesis about the one archetype of our M.T. codices." The hypothesis—taking it as explained in the note there—has been considerably strengthened already.
perative that we absolutely refrain from such claims for the textual relevance of any document unless specific proof for such claim is produced. 27

Apart from these considerations of classification, we have to bear in mind that not every fragment of a biblical text can be strictly termed a biblical fragment. The often somewhat less accurate character of many Haftarah fragments—as opposed to comparable texts in MSS of the Prophets—may be explained by the fact that many of these are fragments from “listeners’” codices. Nevertheless these are to be regarded as biblical fragments. It should, however, be kept in mind that biblical texts also appear in prayer books, etc. 28, and these may outwardly resemble fragments of biblical codices. 29

Without a full investigation of this aspect, these should not be put on a par with biblical fragments. 30

All these remarks refer to MSS which ostensibly attempt to reproduce TBT. Although the influence of reading habits is not restricted to “private” codices, it is most noticeable there. This explains the fact that, e.g., Yemenite “Massora codices” are on the whole excellent receptus codices (cf. chapter III), whereas other Yemenite MSS tend very much to the indiscriminate interchange of Pathah-Segol, etc. 31 In contrast, MSS written by scribes of “Sefardi” habits show

27. This is emphatically stressed in contradistinction to Hempel’s method (loc. cit., 207). He significantly uses the singular: “wenn die innerhebräische Variante übereinstimmt...”. In TL I have stressed repeatedly (cf. XII f. and s.v. Variant) that readings must never be compared “in abstracto”. Either of two conditions must exist for a reading to be even considered as a “real variant”: Either it occurs in at least two prima facie non-interdependent documents (comparative evidence) or the document in question contains additional readings which look prima facie significant (contextual evidence). Only these are possibilities of variants. I am still confident that we shall find a number of Geniza fragments which contain significant variants (cf. note 25). In fact, much of our work on the Geniza is carried out in the hope that in these texts we may encounter such variants. But the hope is not to be translated into fact by lack of method. For the time being, we must suspect “variants” in these MSS to have arisen according to the “Law of Scribes” (cf. TL 159), unless the specific analysis of the document involved affords proof to the contrary.

28. This differentiation does not exist, of course, in the collations of Kennicott and de Rossi.

29. There is, to be sure, the odd case of a Pseudo-Psalm which will, however, be recognized immediately. Thus JTS 576 (ENA 3532) turns a Psalm into a curse (with the necessary details to be added; cf. Ps. cix, 6 f.).

30. Any investigation based on a fragment which may be of this type should clearly indicate its possible nature.

31. Cf. below, chapter III. Yemenite “lowered supralinear” vocalization of this type is still mentioned by some scholars as “interesting abnormal vocalization”.

32. This is used as a convenient a potiori label. Cf. RTBT notes 63, 107, 10. Of course, our terminology is not yet exact enough. We recognize the “Yemenite” or the “Sefardi” type, but we are baffled if a MS seems to be both Yemenite and Sefardi at the same time. Cincinnati non-folio 6 deserves further analysis in this respect. On the
the influence of reading habits very often even in Massora codices, so much
so that a further differentiation between Massora codices and “model codices”
might become necessary in this case.

All the fragments discussed above come from codices which ostensibly
present the TBT text.\textsuperscript{33} Basically different are the fragments from Tiberian
non-receptus codices, although these, too, may show signs of sub-systems.\textsuperscript{34}
These fragments are extremely rare among Geniza material. At a rough esti-
mate I would put them at about half of one percent (cf. RTBT note 13). From
what I have tried to suggest elsewhere (RTBT § 41) with regard to certain
local habits, it can be explained that this system was not common in the areas
from which fragments came into the Cairo Geniza. Because of their rarity,
all these fragments are worthy of special attention in order to augment our
knowledge of this system.

Although it has been stated that details will not be given in this study, I
should like to mention here certain fragments because of this importance (sub-
ject to confirmation upon further work, since the deciphering is sometimes far
from easy): JTS 501 (ENA 2017) fol. 8–9; JTS 512 (ENA 2118) fol. 14–15;
JTS 530 (ENA 2641) fol. 8–13; 23 f.; JTS 558 (ENA 2640) fol. 2 f.; JTS 571
(ENA 3251) fol. 10–11; JTS 593 A (ENA 2589). Also ENA 1755 and Dropsie
Halper 30 seem to belong here.

These are examples, and full listings will be given later. There is, e.g., an
unnumbered Cincinnati fragment of non-receptus—to give an example of the
questions involved—which must be fully described and tentatively numbered
before it can be mentioned. Furthermore this rather defaced fragment has

other hand, phonetic realizations which led to writing Pathah where TBT has Sherut
are rare. Thus the text in an unnumbered Cincinnati fragment, which can be traced
in the Blau misc. envelopes and seems to have been mentioned by Mann, HUCA II
306 because of its liturgical implications, reads: Ps. cxxxviii, 6 סָּכֶל אֵלֶּהוֹ נְהָיָה
Ps. cxxx, 5 יִשְׁפַּר עִּשְׂרֵי. Similarly a fragment in JTS 585 (now in Box K
reads in Deut. ix, 10-11 יִשְׂפַּר עִּשְׂרֵי מֵאֲבֹתֵךְ. It should be remembered
that this type of pronunciation was very widespread and that it cannot be taken as
a significant “ethno-geographic” criterion. At least in fragments from the whole
Mediterranean area—from “Palestine” to Spain and in Yemen—this pronunciation
can be found. Cf., e.g., Morag, Leshonenu 20 (1956), 117; Ben-Ḥayyim, ib. 135;
Ben David, Tarbiz 26 (1957), 398 f.

\textsuperscript{33} In view of RTBT note 120, it should be stressed that the emergence of the early
receptus tradition, heavily leaning towards the “Aleppo” side of the scales rather
than towards the Cairo-Sassoon 507 side (cf. above, note 8) is obvious both from
the codices and from the fragments. It was the main system in the 10th-11th century
MSS, and its gradual development into the prototype of our receptus prints can be
clearly traced.

\textsuperscript{34} This is my term for what Kahle mistakenly calls “Ben Naphtali” MSS. For the
problem, see RTBT chapter VII. A fragment which seems very near a “real” Ben
Naphtali text is, e.g., Cincinnati non-folio 11,2 (two leaves, Sefer, 14th century).
spellings like יִוְשָׁבַח but the problem of the Mappiq in the Alefs needs investigation (cf. for the relevance of these points for the classification, RTBT note 114). 35

Our remarks may perhaps lend some colour to the contention expressed earlier, that the biblical material from the Cairo Geniza is virtually still virgin soil. To what degree it will repay the effort of the textual critic to labour on it still remains problematic.

III. Hebrew Bible Codices

On this occasion I should like to deal with two kinds of Hebrew biblical codices: a) Codices written up to the thirteenth century; b) Yemenite codices.

a) There is no basic difference between American and European collections as regards the ratio between earlier and later Geniza fragments. But according to what has been said in chapter I, American collections had much less of a chance to acquire early codices. I would put into this group1 JTS 232 (ENA 346), a 3-col. Oriental square script volume of the Latter Prophets. Since we know little about the Persian script in the eleventh century, it is difficult to say whether it was written in that country (it was found there), but it is one of the comparatively few specimens of Oriental codices.2 In my opinion this is the oldest codex in the United States.

Although JTS 234a is, strictly speaking, not a "codex", it belongs here. The MS consists of two parts, the first of which contains eighty leaves, which is too much to be termed a "Geniza fragment". It is an 11th–12th century MS, also of parts of the Latter Prophets. Slightly older (probably early 12th century) is

35. In RTBT notes 110 f., esp. 114, I have tried to state what I regard as the marks of the non-receptus tradition. I should add that from the examination of Geniza fragments it becomes clear that the position of the two graphic components of a Ḥataf Pathah as ֶי cf. Kahle, Masoreten d. Westens II (1930) p. 58*) is no mark of a textual type. Whether these were local or temporal scribal habits remains to be investigated. Thus, e.g., JTS 236 (ENA 1781)—12th cent., "Oriental" square, 3 cols.—is not a non-receptus text. But it clearly writes י. This question might be investigated together with the habit of writing יִי, as on two leaves of a Pentateuch fragment of an 11th–12th century Ashkenazi parchment MS, Columbia X 893/B 2797: ZEE. Another habit in early MSS is to write the Shewa component of a Ḥataf slightly apart from the vowel component; this habit lasted longer than the two aforementioned ones. If I have observed rightly, this is especially noticeable with regard to the Ḥataf-Geḏel (> Ḥ).

1. In many cases my dating of JTS material is similar to that given in the handwritten notes of Lutzki, which are important to anyone wishing to use the collection. In other cases we differ. All the dates suggested in this study are in no way meant to reflect anyone's opinion but my own (cf. chapter I, note 21).

2. Cf. above, chapter I, note 28. The MS is a very good example of an early text, developed out of the proto-receptus tradition, near to and yet slightly different from A. Cf. RTBT § 43 I., and chapter II, note 33.
the 3-col. volume Cincinnati folio 1. This is the oldest codex of the Pentateuch and Hafatroth in the States. Although it did not enjoy the publicity accorded to Cincinnati non-folio 123, a 13th-century Sefardi MS described in detail by Blank4, it is in my opinion the most valuable codex in that collection.5

Borderline cases are the codices written around 1300. As explained in chapter I, note 21, codices written perhaps slightly later than 1300 were also taken into consideration, as a kind of additional safety margin.

Here we have to name first JTS 44a, a superb Sefardi model codex of the receptus text of the Pentateuch in 3 cols. (the note “Codex Hilleli” on the cover is, of course, a misnomer based on the fact that the Codex Hilleli is often quoted in this MS). JTS 421 (ENA 1782) is a codex of the Hagiographa, not to be compared in its execution to the former. ENA 1794 is the earliest Sefardi model codex of the whole Bible6 in three columns7 in that collection, probably early fourteenth century.

Turning to the few German MSS in this collection, we encounter first Accession 012838, a huge German 3-col. volume of the Prophets and Hagio
grapha (Psalms missing)9, written probably closer to 1200 than to 1300. JTS 126–127 contains the Pentateuch and Scrolls, and deserves further study because of what seems at first sight to be rather excessive plene orthography.

Two Cincinnati codices should also be mentioned here: Folio 11* (apparently now renumbered 11.3), a MS of the Former Prophets, and non-folio 10.

3. This number was decided on during my visit. It was formerly quoted as MS 13.
4. Cf. Sheldon H. Blank, HUCA 8-9 (1931-32), 229 f. I might add that the investigation of this codex, in which I was assisted by Mr. I. Yefin and Mr. A. Hurvitz of the HUBP, did not confirm that this codex represents in any way an alleged particular “Ben Naphtali” textual tradition. As to its masoretic nature, it was wrongly brought into connection with what Kahle calls “Ben Naphtali” MSS in the first place.
5. All our statements always refer to the biblical division of a collection.
6. On fol. 418 it contains the Targumic Tosefta to the following passages: איה מקדש: זכר הדרת: יערפמה שאולח תשב פאף: ויהי פאף ועט לועמה קינח.
7. This is a good occasion to challenge another myth in dating Hebrew MSS (cf. RTBT note 82). It is correct to say that early Massora codices were generally written in three columns, and that this custom changed slowly into a two-column system, allowing for local differences. But there is absolutely no evidence for asserting that the three-column layout of a codex is proof of its early date, as is often claimed. Massora codices—and the whole argument turns only on these—were written in three columns as long as Jews were living in Spain, for example, and writing Bible codices. Thus JTS 5 (apparently this is Kennicott 571) is a beautiful Spanish MS in three columns, written in May 1479 by the scribe Sasson (hence also referred to as Sasson MS).
8. There is no MS number, and the volume must have been locked away, apart from all the rest of the MSS, for a long time.
9. Cf. also the codex kept in the General Theological Seminary, mentioned above, chapter I, note 23.
This latter, in my opinion a North African MS, arouses our interest in its masoretic character because of the recurrent וּלתֶה (e.g. fol 65 b).  

b) As mentioned in chapter I, the percentage of Yemenite MSS is much higher in American collections than in European—somewhat similar to the situation in the National and University Library in Jerusalem. Both the Enelow and the Sutro collections feature almost exclusively Yemenite MSS, from the 14th to the 18th century, concentrated heavily in the 15th–17th century.

Leaving the problem of vowel-interchanges for the moment (cf. chapter II, note 31) we distinguish three main types of Massora codices: 1) Yemenite supralinear throughout; 2) Supralinear and Tiberian—i.e. the Hebrew text, especially of the Pentateuch, in Tiberian (or Tiberianized) vocalization and the Targum in supralinear; 3) “Tiberian” throughout.

Although MSS of type I were still produced in modern times, the change towards type 3 was gradual but steady. Because of the large numbers of Yemenite codices—altogether about 150—these collections enable us to get well acquainted with the various types. But these are in no way different from those codices known from British and Israeli collections.

While Yemenite paper codices from the 14th–15th century onward are abundant, the 13th century parchment codices are rare. One of the earliest is JTS 508, a very beautiful MS of Psalms with supralinear vocalization. On the other hand, an early fragment which shows “Tiberianized” vocalization is Dropsie Glass Frame Cairo 7, a heavily-damaged fragment of Gen. xxvii-xxx. The script is definitely 13th century, and as far as I can make out through the glass frame,

10. For this Shibolet cf. RTBT note 115.
11. After a hundred years of discussions the terminology is as confused as ever. I do not object to the term “Babylonian” for those cases where the supralinear graphemes go together with the “Babylonian” tradition. But “Babylonian” should be restricted to the tradition—provided it is understood that this is an a potiori label (cf. RTBT notes 63, 116). But using it for Yemenite supralinear MSS, which in their system are essentially “Tiberian”, has created too many misunderstandings; cf. now Kahle’s criticism of the confusion created in Sperber’s Bible in Aramaic (VT 10 (1960), 383).

The “Babylonian” texts in the JTS library have been announced on various occasions by Diez Macho.
12. Sometimes with a “Tiberian addition”, not necessarily by a later hand; cf. e.g., JTS 133(EMC 86). Double vocalization (often partly) is not too rare; cf. JTS 238 (EMC: 37).
13. For this gradual change-over since the 13th century, cf., RTBT note 131, and Qaft, Sina 29 (1951), 262 f.
14. As mentioned above (chapter II, note 11), the percentage of paper Massora codices is much higher among Yemenite MSS than amongst others. There are over twenty parchment codices in the JTS (e.g. JTS 64, 69, 74-78, 89, 95, etc.) and a very exquisite Tāfiah Harvard (MS*Heb e). Also Columbia (X 893/B 47) boasts a very beautiful copy in two volumes containing Ezekiel—Minor Prophets.
the vocalization is hardly younger. Examples: דָּמַר רְחֹל אֵל לֵאָה שַׁעַם. Similar in its character is JTS 238 (EMC 37).

In general, the types of “lowered” supralinear vocalization in these MSS can be classified according to the following criteria, which in all probability reflect the “original supralinear” system and certain reading habits: a) Interchange of Pathah/Segaḥ; b) Interchange of Sheva/Haṭaf; c) Interchange of Tiberian “full vowel” grapheme with a Haṭaf, which is meant to represent a supralinear grapheme for unstressed (or short?) vowel.

We have to bear in mind that the history of the Bible text in Yemen is unlike that of other communities because of the comparatively late acceptance of TBT. Remembering this will save us from misjudging the tradition of Yemenite scribes on the basis of these MSS. There is no doubt that, once TBT became the “officially” accepted text (cf. RTBT note 131), excellent model codices were available to Yemenite scribes, who took the same pride in their work as scribes in other countries.

Claims recently put forward force me, however, to make it quite clear that there is hardly any basis for postulating any special degree of relationship between these Yemenite codices and the model codex of Aaron Ben Asher. Yemenite receptus codices (cf. RTBT § 42 f.) are as “good” or as “bad” as Ashkenazi or Italian or other receptus Massora codices. The only point of reference we have at the moment is the Aleppo Codex (cf. RTBT § 31). It still remains to be worked out whether any of the major “Ethno-geographic” receptus traditions—which are all codices misti from the point of view of A (cf. RTBT § 43)—stands in a particularly near relation to A. Because of the degree of similarity within TBT, I doubt whether anything definite can be said

15. The Tiberian vowels are obviously transcribed, and the Sheva stands in many cases where TBT has a Haṭaf: שַׁעַם . This is not too uncommon even in somewhat later fragments. Cf. the fragments labelled S in Box Ke in the JTS: שַׁעַם . See for this phenomenon, e.g., Morag, Leshonenu 21 (1957), 106. It would, however, be a mistake to assume that this is only possible in Yemenite MSS. Cf. Yalon, "Tiyune Lashon" 1942, p. 31 f.; 1943, 54 f. Another type of reflexion in the “Tiberianized” vocalization is, e.g., (JTS 244): שַׁעַם .

16. I need not add that the vocalization of Codex Petropolitanus of the Prophets is illustrative of some related problems, but is not directly connected. Cf. RTBT note 109.

17. I am referring to the press announcement of S. Morag in Haaretz of May 26, 1961; June 16, 1961. Morag bases his claim on the notes found in some 15th-16th century MSS which read: שַׁעַם . One of the latest of these MSS is the one before us.

18. Codices in which the local reading tradition is reflected in the vocalization are not included in this statement.
on this subject. In any case, this is a matter of examination and proof, not of claims.

This much can be said already now: the Yemenite TBT tradition of Massora codices, as it emerges from the large American collections, shows little internal divergencies; and it is in no way identical—nor almost identical—with the original Ben Asher tradition as shown in the Aleppo Codex. No such special connection can be admitted, as can be seen from any sample analysis of Ga'yan and Ha'afis. The same type of 15th–16th century codices that came to Israel also reached England and America. In my submission we should beware of making announcements—in face of what is to be seen immediately and before any work has been done which proves the contrary—to the effect that the study of these MSS “will bring about an important change in our knowledge of the Massora” (Morag, Ha'areg of 16 VI.61).

I would not be so outspoken in counteracting what to me seems an unfortunate and misleading announcement in the press, had I not myself been a disappointed “discoverer”. JTS 62 and 136—both of the 15th century (JTS 136 is EMC 91, written in 1473)—put forward the claim, reiterated in a go many Yemenite codices with little change in wording, that

וחל ולע הפשר והדרים

משה דמל פד פפרים השופטים ופל אפר

(“Everything according to the arrangement of the famous codex of the complete Bible, corrected by Ben Asher, which was in Egypt”). Since one of the main objects of my search through libraries was to discover material related to the Aleppo Codex (cf. below, chapter V), I was overjoyed with the discovery. Analysis of the text soon revealed that it was a baseless claim.20

19. I have not seen the actual codices which Morag mentions, but I would be very much amazed if they differed substantially from all the codices of this type I have examined.

20. When I saw this note in these late Yemenite MSS, I remembered clearly that I had found a very similar inscription in certain Yemenite codices of the same period which I had seen in the British Museum (I had taken it so little seriously, I admit, that I did not even take down the exact text). I had already got somewhat tired of bestowing what seemed unnecessary attention on the colophons of these “factory” codices of the Enelow collection, when I noticed the wording and decided to record these examples. I am sure that on rechecking all the colophons of the Enelow and Sutro MSS, the note will be found in other MSS—and will be just as meaningless. Sometimes, as in JTS 62, the note may have been written by the owner (in order to enhance the value for prospective buyers?).

Morag’s first announcement may have been “discoverer’s enthusiasm”. But in Ha'areg of June 2, 1961, Rashabi mentioned further MSS having the same stereotype note. And once Morag (June 16) had himself found further copies, he might well have asked himself how to explain this abundance of 15th-16th century codices, all derived—according to his interpretation—from the 10th-century archetype thousands of miles away. These facts, to my mind, strengthen the explanation which was so obvious to me long before Morag’s first announcements. I regarded these notes then—and even more now—as a kind of “imprimatur trademark” of a certain school (or workshop) of Yemenite scribes who produced this type of MS almost wholesale. It was,
Apart from the textual differences—which are the decisive factor—it is hardly conceivable that all of a sudden in the 15th–16th century (and not before) there should have been a direct connection between a fair number of typical Yemenite codices—about ten are known by now—and the Aleppo Codex, which by that time was probably already in Aleppo. Since these MSS are no nearer the BA text of A than other receptus codices, we must perforce accept the note at its face value. If anything is clear it is that nothing in this note points to a direct connection to the BA text, and everything points to its absolute dependence on the wording of Maimonides. It proclaims that this codex exhibits the halakhically correct division of the text as declared binding by Maimonides. It is not at all the BA text—as we are made to believe—which was of importance to the scribe, but the fact that the MS has the arrangement of the writing of the Pentateuch (the note is found only in Pentateuch codices!) according to the way “sanctioned” by Maimonides on the basis of A. That the note was meant in the first place to create the impression as if the text were to be sure, a very reputable scribal workshop, which turned out excellent receptus codices as usual among Yemenite scribes. Sample comparisons with Yemenite Massora codices which do not have the same note, but look prima facie similar, have made it clear, to me at least, that these codices are not in any way nearer to the tradition of A than other Yemenite MSS. In other words, these codices are excellent representatives of Yemenite receptus codices—no less and no more. (Cf. also B.M. Or. 2348, credited to Benaihah himself, which does not contain the note.) After I finished this study, I received Ha’aretz of July 14, where Dr. Loewinger put forward a similar criticism of Dr. Morag’s claim. Loewinger stresses the fact that Morag neglected to inform himself in the first place that he had not discovered anything new and that this type of Yemenite MS was known. The MSS to which I had referred from memory are according to him apparently MSS 88, 90, 96 in Margoliouth’s catalogue of the Hebrew MSS in the British Museum. Loewinger deals with the school of scribes involved and sets down some further facts that speak against Morag. [Additional Note, Nov. 1961: On checking the B.M. MSS on the spot, I became aware of further indications of the “imprimatur” character of the said note. Not only is its position within the structure of the colophon page fixed after the verse count and before the colophon, but the scribe of Or. 2350 (one of the earliest MSS to contain the note) obviously did not take it to refer to the text of Ben Asher himself, as evidenced by his addition, with regard to his text: ﻢمﺎ ﻲأر ﻲم ﻲن ﻲن ﻲم. ﻲس ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲн ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲن ﻲم ﻲn which, characteristically, shows Yemenite vowel changes; see note 24 below). But the difference of writing on the colophon page of Or. 1379 reminds one of the state of JTS 62.]

22. Cf. Auth. note 1. Morag seems somewhat reluctant about admitting that the note in the codices is copied out directly from Maimonides’ Code: “These words remind us (!) of the well-known statement of Maimonides...”
23. This connection between the Yemenites and Maimonides has been stressed by Morag and Ra’ahbi (loc. cit.) Cf. RTBT § 45 f. Ra’ahbi is certainly right when he stresses that the acceptance of the authority of Maimonides went together with the influence of immigrants from Egypt to the Yemen. But the halakhic stature of Maimonides seems
according to that codex—irrespective of the fact that this was not the case—seems to me rather improbable.24

IV. ARAMAIC AND SYRIAC MANUSCRIPTS

Many of the Yemenite MSS contain the Targum, as often in supralinear Yemenite vocalization as in the Tiberian. None of these add basically to what we already know from many other MSS.1 On the other hand, non-Yemenite Targum texts are very rare in these collections. The Targum in the MS of the General Theological Seminary has already been mentioned (chapter I, note 23). JTS 126–129 contains the Ongelos from Exodus onward, and is slightly younger.2 Columbia X 893.1/BC Ti 7 is another good example (Ongelos on Deuteronomy), and is considerably younger still.

The non-Ongelos Targum texts are the only part of the JTS library for which a thorough search had already been made.3 Having gone through the collection after Diez Macho, I would say that very little has been overlooked.4 However, to me the primary factor, also in point of time.

24. As an afterthought I would consider the possibility that these scribes of model receptus codices also wished to state that their codices were purely “Tiberian,” and not just “Tiberianized” (with vowel interchanges). But this is no more than a suggestion for further thought; their main aim was no doubt to stress their compliance with Maimonides’ directives.

All these notes in Yemenite MSS are substantially different from the colophons in Cufut-Kale/Leningrad codices (i.e. L; Cufut. 1; Cufut. 43), which claim the agreement of those MSS with the copies of Aaron Ben Asher. In RTBT note 79, I have dealt with the question, and the completely different wording again strengthens our contention that the Yemenite notes did not claim such direct relationship. It is taken for granted that no colophon which is quoted from a Firkowich codex can be relied upon—or even discussed—before the original is checked. Even so I should like to point out a quotation which has disappeared for almost a century and also eluded me in RTBT, loc. cit. It was quoted by Oppenheim, Geiger’s Jüdische Zeitschrift 21 (1877), 30 f.: מנה יוסי בנו ישעיהי חכמים的父亲 יוסי phụcוס על יהוה ר”ת פרבר נב המעת דמח תרשינ והוה שמעות דמ זכ"י שמעות דא ממכרה תכלת יבש רבעות תנן עליות אליות ומעון חמשת שבעות קדש כיון עלו לפני יז הלשון מאסמריאי יש נצחיי חכמים יוסי חכמים

“1, Joseph son of Jacob the scribe, have written and vocalized and provided with Massora this model codex according to Aaron ben Moses ben Asher, and it is very correct. I did so according to my ability... Finished in Nissan 4882 (= 1122 C.E.) in Alexandria.”

1. Details will be given in later publications. For the Arabic Version, cf. chapter I, note 27.

2. The script is not a real Ashkenazi hand. It resembles one sometimes found in early Italian MSS.

3. I am aware that the items could be checked by following up Diez Macho, VT Suppl. vii (1960), p. 236. At the time of writing the Spanish journals are not available to me. Cf. above, chapter II, note 6.

4. JTS 501 (ENA 2107) fol. 1 (11th-12th cent.) must surely be mentioned somewhere, and either JTS 608 or 605 fol. 6-7 seems to be the fragment which McIntosh is sup-
his attention seems to have been fixed on the Geniza fragments and not on the Yemenite codices, which contain a number of Targumic Toseftas (cf. also above, chapter III note 6). Thus JTS 95 (EMC 41) fol. 60 a and JTS 169 (EMC 79) contain a Tosefta on Num. xxxii, 35; JTS 252 (EMC 24, 25), a 15th–16th century MS, contains a Tosefta on Jes. x, 32; and JTS 254 (EMC 64, similar in type to the preceding) has a Tosefta on Jud v.

From other libraries I can only name Dropsie Glass Frame Cairo 11, Tosefta on Ez. i, 1; and Cincinnati (unnumbered) on Jes. lxvi, 10. (Toseftas on fragments—as opposed to the “marginal” Toseftas of the JTS codices—are introduced by headings: הרמב במשלי; תosalפון אט ירושלם)

Jewish institutions and individuals in the States provided the means and the suitable background for the creation of some sizeable collections of “Jewish” Bible MSS. There was no similar incentive for collecting Syriac MSS. Harvard does not merely possess the most extensive collection of Syriac MSS in the States (cf. above, chapter I, note 7): it has the only collection in the proper sense of the word, one that centers around a core collected with a definite purpose in mind. Among European collections it resembles mostly the John Rylands collection in Manchester, both in size and in composition: a few early codices and mostly modern copies. All other MSS seem to be in their respective libraries almost by pure chance, odd pieces donated by individuals or sent home from a missionary outpost. Quite often the library authorities are hardly aware of their existence.

In the biblical field proper, the American collections are even more disappointing. To be sure, there are a few early N.T. MSS. But the earliest MSS of posed to publish. But at first sight the latter looks like a Pseudo-Jonathan (on parts of Ex xiv, xv, xvii, xix) rather than like a Palestinian Targum.

5. The text is not identical with Ginsburger, Fragmententargum (1899), p. 85, nor with the text of Neofiti 1.

6. For that collection as well as for the problem of this chapter, cf. my “List of some uncatalogued Syriac Biblical MSS”, Bulletin of the John Rylands Library 37 (1955), 429f.

7. Since much of the Harvard collection was acquired from J. Rendell Harris, this is not too surprising.

8. As an example: We were all rather astonished when I detected behind one shelf a ca.17th-century vocalized Serto MS of the Psalms in the JTS (MS JTS 680), which turned out to have been donated as part of the Simon Hertz Memorial Collection. From the textual point of view this MS is, of course, of little value.

9. It is probably due to my ignorance that I am unaware of any listing of Syriac N.T. codices. From the handbooks where I might have got a hint, e.g. Metzger’s Annotated Bibliography of the Textual Criticism of the N.T. (1955) and Ortiz de Urbina’s Patrology Syriaca (1958), I did not get any lead. I hope the N.T. specialists will allow me to write in a note what I happened to see (outside Harvard). The Pierpont Morgan Library in New York possesses a number of valuable N.T. codices, in fact the most valuable in any public collection (there are codices in private hands): Fragmentary codices of the Gospels are M 783, which I date in the sixth century, and M 784, which
the O.T. are from the period after the 10th century.10 Harvard Semitic Museum 1076 contains parts of an 11th–12th century codex, starting with parts of Jos., apparently a Sinaiticus. The similar A.O.S. MS kept at Yale11 is reputed to have been written at the same time. There exists, however, a lonely fragment of the Syrohexapla of Ez. xxxviii, written in an Estrangelo of the 9th–10th century similar to many Nitrían MSS. This can be found at Harvard as MS Harris Syr 14 = Sem. Mus. 3952.12

The most valuable Syriac biblical collection, however, is in the States “de facto” only. Most scholars will have recourse to the Library of Congress microfilms rather than to the originals in St. Catherine’s Monastery in the Sinai Peninsula. Since both checklists available13 offer only one-word identifications of subject and date, a few additional remarks on the biblical MSS in that collection may not be out of place.

The bulk of the biblical portion—to be exact, about thirty codices—consists of liturgical Psalter MSS of the 12th–15th century. To the student of the text these are of little interest. The rest of the collection, however, deserves closer study.

The parchment codices MS 28 (Kings) and MS 35 (Samuel) are both 7th–8th century codices similar in type to the Nitrían MSS of parts of the Bible and showing the “usual” slight degree of textual fluctuation.14 MS 28 contains

I date slightly later. M. 236 is a codex of the whole N.T., small Estrangelo, written 1061 Sel. (=749 C.E.). Next comes Princeton with four fragmentary 10th-12th century codices in the Library of the University and two in the Theological Seminary. (In this case an 18th-century Nestorian paper lectionary in the Seminary should be mentioned, too, because it also contains O.T. lessons.). Union Theological College Library in New York Cage CB 42.7 is a 10th-11th century Nestorian codex. I understand from Dr. Nemoy and Dr. Bowman that there is a tenth-century codex at Yale (identical with that mentioned in note 117) and (a photograph of?) an 11th-cent. (O.T.) Massorah MS in Chicago. Cf. above, chapter I, note 7, for the Harvard Collection.

11. During the whole period of my stay in the U.S., the Yale MS could not be located. The Reference Librarian now kindly informs me that it has been found, but owing to its state of preservation cannot be photographed, so that I have to defer its examination to a later occasion.
12. I only quote these MSS from the period before the 13th century. For the full listing cf. the list mentioned in chapter I, note 7.
13. Agnes Smith-Lewis’ list in Studia Sinaiitica I (1894) has, of course, to be used together with Kenneth W. Clark, Checklist of MSS in the St. Catherine’s Monastery (1952).
14. Cf. Prol. § 9 f. and § 23. At the time I only suggested those MSS for use which I had investigated myself, i.e. the codices kept in Europe. The codices investigated now have to be included. I might add that for Samuel and Kings we must probably also use the two Leningrad codices of ca. 700 C.E., labelled 1 and 2 in Dr. Pogoulevski’s list in RB 41 (1937), 83 f. (Her name is not given in the Russian fashion on that occasion). As far as I can see, her Katalog sirychskikh rukopisey (Moscow 1960 = Palestinskiy Sbornik 6 [69]) does not add anything substantially new.
I Kings vi, 16–2 K. xi, 3, and MS 35 contains 1 Sam i, 1—2 Sam xxiv, 1915, both with heavy lacunae and later additions.

I feel, however, that I may safely announce the existence of another early MS hitherto unnoticed.16 No. 49 is a palimpsest which—to judge by what I could see—could be almost completely restored to legibility through suitable photography. The leaves reveal a 7th–8th century MS of parts of the first half of the O.T. (which may turn out to be a lectionary).17

Already in TL 201 I have pointed out that until now Syriac lectionaries have been almost completely neglected in textual studies. The earliest lectionary in this collection is MS 818, whereas all the others—MSS 39, 49, 89, 94, 213 and also MS 234, listed as a Prophetologia—are ca. 13th-century codices. What is textually most striking about almost all the St. Catherine lectionaries is that the lessons from the book of Ezekiel—and these alone—are always given in the Syrohexapla version.19 We are thus becoming more and more alert to the various ways in which the two major Syriac versions were combined in various types of lectionaries.

V. THE MISSING PART OF THE ALEPPO CODEX AND THE MANUSCRIPT OF SAPIR’S COLLATIONS

In preparing this paper I have tried to concentrate on general questions. However, I should like already now to draw attention to one particular manuscript. In the beginning I hardly thought it worth while to give it a second glance. Why bother about someone’s notes, obviously written in the 19th century,

15. Not a MS of 1 Sam., as listed. Cf. also de Boer, VT 9 (1959), 408 f.
16. Also a Syropelean text in MS 8, fol. 159 has apparently not been noticed before. It is being prepared for publication.
17. Talking of palimpsests, I should like to voice a request. Among the Geniza fragments examined, a few turned out to have an underwriting similar to those fragments discovered in Cambridge, University of Pennsylvania Museum E 16507 is typical: a fragment of a midrash written on a Syropelean text. On the basis of what I have seen, I would suggest that Hebrew biblical texts were never either the lower (for obvious reasons) or the upper writing in Geniza fragments. For this reason there are no Hebrew biblical palimpsests to be expected. But non-Hebrew texts were used as lower writing under a non-biblical Hebrew text. Hence it is the students of non-biblical Geniza fragments who will usually find these palimpsests. It would be appreciated if such finds could be announced without delay.
18. Although I have not seen the original, I would date it rather in the 11th-12th than the 10th century, as listed. Like most lectionaries in the collection, it is a mixed O.T.-N.T. codex.
19. This is also true, as I see now, of Vat. 278. Cf. Prol. §23. These lessons contain usually, in various portions, Ez. i, i-iii, 3; xxvii, xliii, 27-xliv, 4. For the whole problem of the lectionaries, cf. our forthcoming volume on “The Syrohexapla and its Remnants”.
if your task is to examine early Bible codices? But MS JTS 729 soon turned out to be one of the most valuable finds.

From the catalogue of the late E.N. Adler it was to be expected that MS ENA 1632 should contain notes in connection with the Aleppo Codex, written down by the well-known traveller Jacob Sapir about a hundred years ago. These notes, which had come into the possession of Adler, were apparently lost in the maze of uncatalogued MSS in the JTS until good fortune made them reappear. Since one of the major aims of our search for MSS is to find out how to complete most faithfully the parts of A destroyed in the Aleppo pogrom, the value of the find can easily be imagined.

A hundred years ago, when the very existence of the Aleppo Codex first came to the attention of European scholars, some collations from the Pentateuch were published in order to make the public aware of readings which differed from those of the receptus prints. Because of external circumstances, the publication of the weekly concerned was interrupted for some time, and the printing of the collations was discontinued after the end of Exodus.

An analysis of the collations in Lebanon leaves little doubt that the list was a very sporadic kind of checklist. From the usual treatises on massoretic variant readings, such as Lonzano’s Or Torah, Norzi’s Minhath Shai, and the notes of Dubno and Heidenheim, Sapir was aware of the existence of certain differences within the TBT tradition. What seemed to him most significant he noted down in one column and asked for the collation to be written in the parallel column. In Auth. note 8, I pointed out that Lebanon was a weekly newspaper and that the possibility of printing-errors should not be ignored. Nevertheless these published collations were our only source for the readings of A in 95% of the Pentateuch.

MS JTS 729 consists of eight large-folio two-column pages, the first of which contains Sapir’s introduction written in 1855 (גָּשׁ בֵּית הָעָם). In content and often in wording, it is identical with the descriptions published in Lebanon 1,16 and in Even Sapir (1866) 12a. There is no doubt that our MS and the published collations are not independent witnesses. They stem from the same source and are practically identical. Only in one out of 70 common cases do we find

2. In Lebanon (גָּשׁ בֵּית הָעָם) 1 (1863), 16f., 23f., 31f., 76. See Auth. note 5.
3. To the best of my knowledge the publication was definitely concluded after Lebanon vol. 1.
4. The three descriptions are “free variants”, but they obviously come from the same source. In what follows, the abbreviation Lib. will be used for the list as printed in Lebanon, and Sap. for our MS.
5. A comparison of the readings in Gen. yields the following result: In 89 cases the readings are identical. In xxvii, 33 and xxxix, 15 Sap. has a plus, and in ii, 23, 24 the
an apparent contradiction: Gen. iii, 17 נאששת ר י is quoted with י accentuated with Munah according to Lib.; according to Sap. it has a Maqef. But this contradiction rests on an oversight, so that we can really conclude that both lists are identical.

Both lists clearly reproduce the collations made in Aleppo by Menashe Sithon and forwarded through the good offices of Jacob Berlin. I would, however, suggest that JTS 729 is not the original list sent to and from Aleppo, and this accounts also for the few additions and omissions. According to Even Sapir, loc. cit., Sapir had prepared a list to be checked by Sithon. At least in the cursive we would expect two different handwritings, one in the “question” column and another in the “answer” column. This is not the case. The same hand wrote everything, including the introduction. Since Sapir made it clear (loc. cit.) that he did not collate personally, our MS must be Sapir’s copy of his own original list. If this reasoning is correct, we cannot exclude the possibility of copying mistakes.

Examination of the details, however, proves that in general we may rely on it. Sapir was aware of the importance of the codex, and in copying the collations into a MS in honour of his father—he called it ממשה ית海淀区—he certainly exercised care. This can luckily be proved by comparing those parts of סֵפֶר which are extant in A, i.e. the end of Deuteronomy and the Haftaroth-portions which were also collated. Taking as a sample the readings adduced from the Haftaroth to Genesis and Exodus, we find that Sap. is identical with A in all 42 cases. To be sure, not all of these seem to us significant readings, but those that are show that the collations were definitely made from A (the only MS in Aleppo which could have had those readings). Thus: Jud. iv 9 נשתה, Jud. v. 19 דָּבָר, 1 Sam. xv, 1 כָּלָה; 15 ולָה; Jer. xxxiv, 11 שֵׁבַע (defective); Jer. xlv, 23 מֶהַר (without Metheg). In one case Sap. quotes the Massora exactly like A: Is. vi, 10 כָּלָה, כָּלָה.
There is, however, one case in which it seems for a moment as if Sap. disagreed with A. In Is. lxvi, 3 Sapir had written יש אלפיםapl and added בראות. The answer was: yes. At first sight this does not seem quite correct, since the reading was, of course, בראות. But Merkha and Ga'ya are often indistinguishable in A\(^1\), so that Sithon could answer both cases in one “yes”. We need not wonder about the wording: Sapir had simply used the terminology he found in Norzi’s Minhath Shai; and from that treatise, at Is. xlix, 7, s.v. ילמה משטח, it is clear that he read יש אלפיםapl, as he said, with two accents: Shofar and Ma’arih.\(^14\)

In other words: Where we can control Sap. in our sample, it agrees fully with the readings of A. While care still has to be exercised in checking each reading\(^15\), we are allowed, in my opinion, to claim that the material in Sap. may be taken as the genuine readings of the Aleppo Codex. Short of the rediscovery of the missing leaves of A, our list is the best available substitute for Ben Asher’s readings in A.\(^16\)

In two classic instances our MS is of interest in the light of Mishael Ben ‘Uziel’s Hillufim.\(^17\) In Gen. xli, 50, Sapir had asked about the Qamay of יש אלפיםapl, which is — as we know from Mishael\(^18\) — the reading of Ben Naphtali.\(^19\) The answer

10. My thanks are due to Mr. Y. Yeivin, assistant at the HUBP, who was kind enough to collate for me the readings in A.
11. Three others (from Obadiah) are missing in A, but the reading of Sap. can be found in L.
12. I do not quote cases in which Sithon simply answered: Yes. All the statements added here are explicit.
13. The possibility of interchange is not just graphical, as shown by the terminology sometimes employed. Cf. the examples given by Ben David, Tarbiz 26 (1957), 391.
14. In Is. vi, 7 Sapir writes clearly יש אלפיםapl and calls this a "two accents".
15. Cf. the chapter in NMT, where the readings will be fully analysed.
16. It was already clear a hundred years ago (cf. Even Sapir I, 13b) that the readings given in Ha-maggid 1 (1857), No. 47-48, reputedly from the copy of R. Moses Isserlis, are in no way an exact copy from A. As regards the other alleged source of readings from A, I have shown in Auth. § 11 that to the best of our knowledge Cassuto never made any collations from A. Recent statements which repeat this mistake—Loewinger, Textus, 1 (1960), 61 and Kahle, Der hebräische Bibeltext seit Franz Delitzsch (1961), p. 85—are apparently written without knowledge of these facts. The list now regained is therefore the only evidence we could hope for.
17. It is obvious from the start that only a few points can be common to the two lists.
18. I should like to use this opportunity to express my gratitude for Prof. Kahle’s helpfulness in putting his transcript of Mishael’s whole treatise at my disposal. Cf. RTBT note 3. I have checked the readings against the publications of Lipschütz, Ben Aser-Ben Naftali, 1935, and Perez Castro (Sefard 1955). According to Castro the original Qamay in L was changed by erasure into a Pathah.
19. This must have been a major difference within TBT, if we can believe the note in Or. 2363 fol. 41 b (quoted by Ginsburg, Introduction (1897) p. 696: מי הוה רם ו敬畏 אשם או אשם והוה: מי. As for the phenomenon itself, I
came: “No, with Pathah. And in the Massora outside with Pathah (see below and Ben David, ib.) a case could be made out for a text-critical emphasis, but I do not believe this either, since there are too many similar cases in which no hilluf is handed down. Here we might just read our modern ideas into the hillufim. A third possibility is exegetical connection with other cases of hillufim. None of these explanations seems satisfactory.

20. This seems to refer to the marginal Massora.
21. I have no photostat of L available at the moment, but I accept the statement of Perez Castro that in L the Qamas was changed into a Pathah; BH reads קַמָּשִׁים. Does Loewinger, Texta 1, 64 mean to say that A too, read originally a Qamas? From the plate this does not look likely, but I cannot check at the moment.
22. Cf. the plate in Texta 1.
23. It will be found useful always to compare Minhat Shai, because Sapir had become alert to the problems mainly through that work.
24. In spite of the fact that BH is not a diplomatic reproduction of L, I have to be content for the moment to quote it (cf. note 21). For this reason I have selected cases where the prima facie testimony of BH seems to indicate that there is a difference. But I have left aside quotations from the Massora. In general these, of course, agree with what we find in BH, but not always. Thus, e.g., Num. 15:15 אֲחַרְּךָ אֵלֶּה בְּעֶשֶׁר יָמִים אֲשֶׁר וַעֲשִׂיתָן יִלָּכְּךָ אֵלֶּה. A had also but read in the marginal Massora: אֲחַרְּךָ אֵלֶּה בְּעֶשֶׁר יָמִים אֲשֶׁר וַעֲשִׂיתָן יִלָּכְּךָ אֵלֶּה.
25. But in 2 Chr. xxvi, 18 BH reads קַמָּשִׁים.
26. This is noted down for all the verses in which the form occurs.
27. This needs careful study because of the wording of the list and the whole well-known problem of hillufim. On Ex. xii, 13 Sapir wrote as a question why and added: “And so all know with a Metheg.” The answer was: The first—apparently Ex. xii, 7—with Manah and Zaqef Qatan (cf. Mishael’s list, ed. Lipschütz p. 1) and the second with Metheg.”
Although we have no reason to doubt the exactness of the answers which Sapir had obtained, the cases in which we know the reading of A simply because of Sithon’s “yes” cannot be accorded quite the same degree of certainty as the preceding ones:

<table>
<thead>
<tr>
<th>Gen. xxvii, 41</th>
<th>BH</th>
</tr>
</thead>
<tbody>
<tr>
<td>נברכה</td>
<td>סָעָלַקְתָה</td>
</tr>
<tr>
<td>Gen. xxxii, 18</td>
<td>BH</td>
</tr>
<tr>
<td>נזקך</td>
<td>טָעָק</td>
</tr>
<tr>
<td>Ex. xiv, 11</td>
<td>BH</td>
</tr>
<tr>
<td>יָמָכַל</td>
<td>יַמְכָל</td>
</tr>
<tr>
<td>Lev. xxv, 51</td>
<td>BH</td>
</tr>
<tr>
<td>אָסָע</td>
<td>בָּשָא</td>
</tr>
<tr>
<td>Deut i, 38</td>
<td>BH</td>
</tr>
<tr>
<td>נָקַל</td>
<td>לֶא</td>
</tr>
</tbody>
</table>

On the other hand a number of cases are of interest where Sap. confirms the reading printed in BH:

<table>
<thead>
<tr>
<th>Gen. xxx, 41</th>
<th>BH</th>
</tr>
</thead>
<tbody>
<tr>
<td>הַשָּׁמֵש</td>
<td>בֵּית</td>
</tr>
</tbody>
</table>
Ex. xxviii, 18 הַיֶלֶת Ex. xxviii, 20 הָשֶׁר
Num. xxi, 4 לַעֲבָד (without Dagesh) Num. xxv, 12 שְׁלַשָׁה

It was an exceptionally unfortunate fate that caused the destruction of parts of the Aleppo Codex through the hands of pogromists and vandals. The loss was great when the desecration occurred. But only now, after the full value of this codex has become clear, can we appreciate what has to be regarded as lost forever. Sapir's list can only solve very few problems. But under the circumstances we have to be grateful that after a century the only evidence for the lost readings of the Aleppo Codex has re-emerged.

[Completed July 1961.]

39. Sapir naturally asked with regard to the famous וֹוֹ וֹכֶרֶד (cf. Minhath Shai, ad. loc.). The explicit answer was that the וֹוֹ is not different from any other.

40. I hope that in RITBT I have made out a case for the unique status of the Aleppo Codex much beyond what could be assumed even when Textus 1 was published.