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## ARTICLE III.

## THE PENTATEUCHAL TEXT, THE DIVINE APPELLATIONS, AND THE DOCUMENTARY THEORY.

## A REPLY TO DR. SKINNER.

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IN the London *Expositor* for 1913 there will be found a series of six articles by Dr. John Skinner entitled "The Divine Names in Genesis." They run from April to September, and in form they are a criticism of the first division of Dahse's "Textkritische Materialien zur Hexateuchfrage," and an attempt to defend the documentary theory against that portion of his assault which is contained in his discussion of the Divine appellations. The articles are limited to this question, all Dahse's other arguments against the theory being ignored. I emphasize this at once, because in this matter the articles are palpably unfair. Neither Dahse nor any of his allies can be held in any way responsible for the suggestion that the conservatives deal with the Divine appellations alone, which underlies much of what Skinner has written. Dahse himself has published a short reply in the same periodical,<sup>1</sup> and all who are interested in the subject should carefully consult this. But, in fact, certain portions of Skinner's articles deal with the present writer's work much more than with Dahse's; and there are matters which,

<sup>1</sup> Art. "Divine Appellations, Textual Criticism and Documentary Theory," *Expositor*, Dec. 1913, pp. 481-510.

for various reasons, must be treated in these pages, though I shall as far as possible avoid going over the same ground as Dahse. In the present discussion I abandon Skinner's order, and select, in the first instance, the important article that appeared in August, 1913 (the fifth of the series), so as to begin with .

#### THE PROBLEM OF THE SAMARITAN PENTATEUCH.

Some of the salient facts which must be known for the purpose of this inquiry are succinctly stated by Skinner as follows:—

“The Samaritan Pentateuch is a recension in Hebrew of the books of Moses, as used by the schismatic community whose religious centre was and still is the temple on Mount Gerizim at Shechem. Its MSS. are written in the Samaritan script—a degraded form of the original Hebrew alphabet; but otherwise they are simply a special group of Hebrew codices, and are in fact often treated as such by writers on the Old Testament text. The characteristic features of the recension appear in a series of intentional alterations of the parent text, due to editorial motives and tendencies. The most striking of these are, of course, the few changes introduced in defence of the legitimacy of the Samaritan temple and worship, . . . . Many alterations spring from the desire to produce a smoother, more intelligible and more consistent text: archaic or abnormal grammatical forms are eliminated, discords of gender and number are avoided, exegetical difficulties are removed by glosses and emendations, and inconsistencies are reconciled by corrections or short interpolations from other contexts. Besides these there are a number of lengthy insertions from parallel passages, which form one of the outstanding peculiarities of the Samaritan text. Some

of these alterations are clearly of Samaritan origin; but in other cases it must remain uncertain whether they are the work of Samaritan editors or were found in the Jewish MSS. on which the Samaritan Pentateuch is based. Taken together, however, they impart a distinctive complexion to the Samaritan text, which is recognizable as far back as we can trace its history.

“If now we set aside these characteristic differences, we find in Samaritan MSS. a consonantal text which very closely resembles the MT. The variants are for the most part of the same kind and the same order of magnitude as those found in extant Hebrew MSS., and the majority of them scarcely exceed in importance those which seem to have existed in Jewish MSS. of the Talmudic age” (*Expositor*, Aug. 1913, pp. 97-98).

What is the relation of the Samaritan Pentateuch to the Massoretic and Septuagintal texts? A long controversy on the subject was closed by a monograph of Gesenius which appeared in 1815. He came to the conclusion that the LXX and Sam. were derived from Jewish MSS. which resembled one another, and followed a different recension of the Pentateuch from that which subsequently obtained official recognition in Palestine. The reputation and ability of that great scholar caused his view to pass unchallenged for nearly a century. In 1909 Professor Kennett advanced a theory which Skinner rightly dismisses summarily. In September, 1911, I ventured to publish in the *Expositor* an article entitled “Samaritan, Septuagint, Massoretic Text,” in which I pointed to the faults of Gesenius’s method, and adduced evidence to refute his conclusions and support another theory. Now, of this, Skinner writes as follows:—

“The second theory demands a somewhat fuller examina-

tion, because it is more plausible in itself, and because it opens up questions which go to the roots of the textual problem of the Old Testament" (p. 108).

"It is right to say before passing from the subject that neither this theory nor that of Dr. Kennett has been before the minds of critics in investigating the structure of the Pentateuch: their task has been performed under the influence of a conception of the text springing from the common opinion as to the dates of the LXX and the Sam. So far as I know these theories are both recent arrivals. Now that they have arrived it will be necessary to examine them on their merits, and in the light of all the circumstances of the case. Such an examination is perhaps at the present moment premature. We shall do well to await the publication of a critical edition of the Sam. before committing ourselves to a final judgment upon them. The general acceptance of either view would certainly destroy or weaken the confidence hitherto felt in the Sam. as the earliest witness to the Hebrew text, and would to that extent affect the textual argument for the documentary theory" (pp. 115-116).

This theory of mine is that the Hebrew MS. or MSS. from which the LXX is ultimately descended separated from the stream of tradition which has given us the Massoretic text and the Samaritan *before* the Samaritan schism. In my view the first great bifurcation in the transmission which has left its mark on our textual witnesses was the separation of the Egyptian text from that of Babylonia and Palestine. Of course I admit that in those times there was far greater variation in individual MSS. than there would be to-day in MSS. of the Massoretic text. The scribes of those days were not guarded by the detailed Massoretic rules. But, subject to this, I believe that we have to distinguish two main streams

of tradition, and that the Samaritan belongs to the same main stream as the Massoretic in contradistinction to the Egyptian. Skinner's view of the importance of this theory is shown by the passages I have quoted. As he does not adopt it but still follows Gesenius, it is pertinent to ask how far he is able to defend him. In the *Expositor* for September, 1911 (pp. 202 f.) I wrote as follows:—

“Such questions naturally lead one to ask whether Gesenius is altogether a trustworthy guide in this matter, and if not, why not? On examining his monograph I found that the explanation was exceedingly simple. Gesenius came after a long controversy as to the relative merits of the Samaritan and Massoretic texts and a minor controversy as to whether the LXX. was translated from the Massoretic or the Samaritan. Hassencamp, for example, wrote a monograph to prove that the LXX. was translated from Sam. Now in this controversy a curious error of method seems to have been made. Instead of comparing the three texts, M.T., Sam. and LXX. with one another, the controversialists appear to have compared M.T. and Sam., and then to have consulted LXX. *only in the places where the other two differed*. But this is quite faulty. If I desire to compare three articles *a*, *b* and *c*, I must compare *c* with *a* and *b* in addition to comparing *a* and *b* with each other. If I only compare *c* with the differences between *a* and *b*, I run the risk of reaching wrong conclusions through ignoring the matters on which the two latter agree against the former. And this is what has happened to Gesenius. He even quotes Hassencamp in the most unsuspecting manner, never dreaming that this writer might have left out of account the most important part of the evidence. Consequently the whole of the current views as to

the history of the text in so far as they rest on this monograph of Gesenius stand in urgent need of revision."

To this, Skinner has made no direct answer at all. But he goes further, and makes concessions to me which appear to destroy Gesenius's view altogether. To me it seems that Skinner's article is written from the standpoint of one who has not yet completely emancipated himself from the old view, but has, nevertheless, been strongly influenced by the new; and I cannot help thinking that further consideration may produce further modifications. Indeed, it is in this and the preceding article (to be discussed later) that I find some of the most cheering indications of progress in the Pentateuchal problem that I have yet seen. Therefore, bearing in mind the fact that Skinner has not attempted any defense at all of Gesenius's method, I ask for the most careful consideration and comparison of the two following extracts from his article:—

(1) "In what follows I will adopt the view which seems most plausible in itself, and at the same time is most favourable to those who would use the Samaritan Pentateuch as a witness along with the LXX against the MT: viz., the theory of Richard Simon and Gesenius, that Sam. and LXX are both derived from a group of Hebrew MSS. containing a text which, while not quite homogeneous, differed as a whole (and occasionally for the better) from the official Jewish recension which is the parent of our Massoretic text" (p. 99).

(2) "(a) The agreement of Sam. with MT. is greater than that of LXX with either . . . . (a) will, I believe, be generally conceded" (p. 109).

It seems to me that the statement of fact contained in the second passage, written under my influence, militates against the theory stated in the first. If Sam. and LXX are both

derived from a group of Hebrew MSS, containing a text which, while not quite homogeneous, differed as a whole from the official Jewish recension which is the parent of our Masoretic text, how comes it that Sam. resembles M.T. much more closely than LXX? Four possibilities might occur to the mind, but Skinner will have nothing to say to any one of them:—

(1) The Samaritan may have been influenced by the Masoretic text later on; in fact, there may have been what Skinner calls “a progressive assimilation of the Sam. text to the later Jewish, due to the friendly relations subsisting between the heads of the two communities” (p. 107, *n.*). His reply to this is as follows: “Of that position I can only say that in the first place it is too nebulous to admit of discussion; and in the second place that it seems so improbable that it could only be accepted as a last resource to relieve a problem otherwise insoluble” (*ibid.*). This therefore may be dismissed with the remark that if it should be held that Sam. has been influenced by M.T., there is an end of the view that it is an external witness to the correctness of M.T.

(2) It might be suggested that the divergences of the LXX were due in whole or in part to alterations of the Hebrew after its separation from the Samaritan. To this there are two answers: readings of the LXX that are superior to those of M.T. and Sam. (of which more hereafter) cannot be due to this cause: and Skinner himself raises the following objection: “On Wiener’s theory we are bound to suppose that the MS. used by the LXX translators was not an officially guaranteed MS., but a private codex, distant (it may be) by many removes from the central stream of tradition, though it happened to conserve some readings superior to the standard text” (p. 115). Clearly if Skinner believes that



the Hebrew of LXX was an officially guaranteed MS., the theory of alterations is untenable.

(3) It might be urged that the LXX editors or translators introduced differences. Now here again the superior readings stand in the way: further, the tendency of those editors of whom we have historical knowledge was to approximate to a Hebrew text of their day: it is unintelligible that men should have troubled to get a copy of the best official Palestinian text of the day simply to knock it about afterwards in translation without rhyme or reason, and, lastly, Skinner writes in another connection: "Nor am I prepared to admit that even the LXX editors and copyists ever introduced wanton changes of the names of God. In comparison with the Jews and Samaritans they were somewhat careless, and they may often have substituted what they believed to be the better reading; but that they would have made *systematic* alterations of the kind here supposed I see no reason to believe" (May, p. 406). But such discrepancies as those that occur in the last chapters of Exodus could only be due to intentional change on one side or the other or to two independent editions of the same material.

(4) The changes may be due to subsequent corruptions or alterations of the Greek text. To this suggestion most of the answers that apply to (3) may be made.

For these reasons it appears to me to be absolutely impossible for Skinner to maintain the whole of his positions with regard to the Samaritan. On further consideration he will have to move in some direction or other. At the same time his discussion seems to me to contain some very just criticisms of my article, and also suggestions of value for the further progress of the inquiry. Of criticisms I quote the following:—

“Divergences of the LXX from the joint Sam. and Jewish text may be of four kinds: (1) They may be readings of the original text which have been lost in the Heb.-Sam., but preserved by the LXX; . . . . Where the LXX has clearly the better text we must assume that its reading falls under this head. (2) They may have originated in Heb. MSS. before the Greek translation was made . . . . (3) They may have come in in the translation itself . . . . through actual errors, misreading of the Hebrew, non-literal renderings, explanatory additions, and so on. (4) In the subsequent history of the Greek text of the LXX . . . . I have pointed out in the last note that in my view Wiener makes no sufficient allowances for changes of the class (3). I have now to add that he does not seem to recognize at all the existence of class (2). He appears to fancy that when he has proved that a reading of the LXX rests on a Hebrew variant, he has recovered a text which goes back to the point . . . where the Hebrew of the LXX parted from the other line of descent; and thus identifies the text of a Hebrew MS. of say 250 B. C. with the text common to all MSS. at the unknown period when the LXX is believed to have broken away from the joint transmission. Whereas it is only where the LXX has preserved the intrinsically *superior* text that such inferences are legitimate. Perhaps the fallacy lies in thinking of the LXX as representing an *older text* than Sam., because its Hebrew basis started on its separate course at an earlier time. It is surely superfluous to remark that the Heb.-Sam. diverged from LXX just at the same time as LXX diverged from Heb.-Sam.; and that it is sheer inconsequence to think of the one text as older than the other on this account” (pp. 110-112).

With much of this criticism I am in agreement. I think

I did recognize Skinner's class (2) when I wrote my article, but omitted to mention it because I was dealing mainly with cases which could not be due to it. Still, Skinner is right in making the point. As to (3) there is a difference of opinion between us. But with some of the latter portion of his criticism I am not in agreement, for he has overlooked one point. I sometimes think of the LXX as, on the whole, the older text than Heb.-Sam., because I think there is evidence to show that in some respects Heb.-Sam. may have suffered revisions after the Egyptian tradition separated from that of Babylonia and Palestine. On pages 216 ff. of my *Expositor* article I wrote as follows: "Considerable weight, again, must be attached to differences of reading that suggest different views of the history of Israel or of the historical situation of the writer. As reference has been made to the law of the king in Deuteronomy xvii. I may briefly explain that there is considerable reason for supposing that the LXX did not read king at all. The matter is not one that can be discussed shortly, but I have sent an extended note on the subject to the *Bibliotheca Sacra* for July, 1911."<sup>1</sup> I should have welcomed a discussion by Skinner of this and the variants adduced in the fifth chapter of my "Essays in Pentateuchal Criticism."

I pass now to Skinner's suggestions. In answering Dr. Kennett he writes: "We must bear in mind that long before then there was a numerous Samaritan diaspora in Egypt, if not in other lands; and even if the Jews had succeeded in destroying all MSS. of the Pentateuch in Samaria itself, it must surely have been possible to procure one from the Egyptian colonists" (p. 108). What is sauce for the Samaritan goose is surely sauce for the Jewish gander. If a

<sup>1</sup> Now reprinted in *Pentateuchal Studies*, pp. 157-168.

Samaritan diaspora would have copies of the law which furnished the rule of Samaritan life, how can it be contended that the Jewish diaspora had no copies of the law that furnished the rule of Jewish life? In the *BIBLIOTHECA SACRA* for April, 1913, I have myself urged an argument that is identical in principle with this of Skinner's: "It is difficult to believe that the Jews to whom Jeremiah could say: 'Because . . . ye have not obeyed the voice of the Lord nor walked in his law, nor in his statutes, nor in his testimonies' (xliv. 23; cp. 10) could have been ignorant of the existence of a law or could have denied that it possessed some binding force. The appeal made is to something known and authoritative. If that is so, how can we suppose that all the Jews of Egypt had no copy of the Law from those days onwards? To me it seems most probable that the original divergence of the two lines of tradition took place in the age of Jeremiah, that the LXX was translated from MSS. of the Egyptian line, and that that is the reason for the frequent superiority of its text to that of the Massoretico-Samaritan, which was possibly edited in Babylonia" (p. 289). But here I come face to face with a theory that Skinner regards as axiomatic: "As all living men trace their ancestry to Adam, so all MSS. of the Pentateuch descend alike from the edition of the Law promulgated in the fifth century" (p. 112). That of course begs the whole question. It is really sufficiently answered by Skinner's own statements as to the necessity for examining the theory on its own merits and as to the action of the critics in speculating on the structure of the Pentateuch under the influence of a conception of the text springing from the common opinion as to the dates of the LXX and the Sam. But, in fact, this idea that all MSS. of the Pentateuch descend from Ezra's copy is due to the evolu-

tionary theory, and rests on nothing more substantial than the confusion between a cairn and a house. If Skinner had studied the sixth chapter of my "Essays in Pentateuchal Criticism" and pages 225-289 of my "Pentateuchal Studies" with the care that he has devoted to portions of my Samaritan article, he would not have taken this line. And a second point that must be met in this connection is stated as follows: "I think that the theory under discussion has a claim to consideration as a feasible solution of an intricate problem; though there are two reasons that make one hesitate to accept it. One is that tradition and historical probability are against it. What history tells us is that the Samaritan Pentateuch is older than the Greek translation. If we dismiss the statements of the pseudo-Aristeas as unworthy of credence, there is still a probability that the Jews of Alexandria would not have been content with a version derived from any less authentic source than the official Palestinian text of the time. Still, if it should be found that the relations of the three recensions can only be explained by allowing a higher antiquity to the Hebrew archetype of the LXX, the historical presumption may be overridden by literary evidence, and the view we are considering may have to be adopted" (pp. 112 f.). Now with the view that the Jews of Alexandria would have been content with a version derived from any less authentic source than the official Palestinian text of the time I cannot agree. Apart from facts, there is at least an equal probability that they desired a rendering of the law with which they were familiar. It is general knowledge that communities are very tenacious of their Bibles and do not readily substitute an unfamiliar form of the text for what they know and love. Nor can any tendency to resort to the Palestinian text of the day be discerned in the Septuagintal translations

of other books, such as Jeremiah. Again, the Nash papyrus has shown us that Hebrew texts of the Law differing widely from Heb.-Sam. but strongly resembling LXX had currency in Egypt for centuries after the Greek translation was made. Surely a community that was satisfied with the old Egyptian Hebrew text and used it for centuries afterwards cannot reasonably be suspected of hankering after the official Palestinian form, especially as the Nash papyrus shows that the LXX does in fact correspond to a genuine Hebrew type of text. Once more, though I reserve this point till I have cited Skinner's second objection to my theory, the nature of the differences between LXX and Heb.-Sam. is alone sufficient to dispose of the idea. Finally, I have to notice a rather curious matter. Skinner has overlooked the fact that he himself does not believe that the Jews of Alexandria had recourse to the official Palestinian text of the time. For, in his second objection, he puts forward the theory he favors with the words "Sam. and LXX being both derived from an *unofficial* Hebrew recension" (Skinner's italics!)! And on page 273 of his September article he says: "The MT is the lineal descendant of the official Palestinian recension of the OT; the LXX represents at best an Alexandrian recension." For these reasons he will be unable to maintain the first objection to my theory.

The second objection is as follows: "But in the second place we may hesitate to decide that this is the only or the best solution of the textual problem. We should have to inquire whether the resources of the theory of Gesenius, are not adequate to the situation with which we have to deal . . . . Sam. and LXX being both derived from an *unofficial* Hebrew recension, which had ramified into an indefinite variety of texts, of which the Sam. and the LXX are the

only surviving representatives" (p. 113). We have already seen that this passage is in contradiction to his first objection to my theory, in that it regards the LXX as being derived from an *unofficial* Hebrew recension and not, as originally suggested, from "the official Palestinian text of the time." It must be added that it is also in utter contradiction to his own contention about the Samaritan which is stated on the next page in the following words: "It is a legitimate supposition—conceded in fact by Wiener, although we can have no absolute certainty on the point—that the Samaritan Pentateuch was derived from the canonical text of Palestinian Judaism" (pp. 114–115). Very well: if the Samaritan was derived from the canonical text of Palestinian Judaism, it does not come from an *unofficial* Hebrew recension, and consequently cannot be regarded as sharing with the LXX the modern representation of that recension. But how comes it that Skinner has involved himself in this mass of contradictions? I think that the answer lies in the second part of my Samaritan article. My claim was that my arguments were "partly quantitative and partly qualitative" (Expositor, Sept. 1911, p. 205). Skinner conceded the quantitative portion with the admission that "the agreement of Sam. with MT is greater than that of LXX with either." Of my qualitative arguments he makes no mention. Yet I had written: "But to estimate the matter properly one has to take into consideration other factors. While the number of variants should have some weight, still more should be attached to their character. Variants that through their intrinsic superiority appear to preserve an original text against later corruption or glossing carry with them a very different amount of conviction from variants that can be explained as being due to editorial causes" (*ibid.*, p. 215). Let me further cite what I say

on page 217: "It is impossible to do more than just refer to other classes of evidence. But it must be remembered that where the LXX. diverges most from M.T. Sam. follows the latter with its usual fidelity. In the concluding chapters of Exodus the LXX. differs widely from M.T. In the opinion of Robertson Smith 'the variations prove either that the text of this section of the Pentateuch was not yet fixed in the third century before Christ, or that the translator did not feel himself bound to treat it with the same reverence as the rest of the Law.' Yet the Samaritan follows M.T. with its usual closeness except that it places xxx. 1-10 *after* xxvi. 35. This appears to me of very great importance." This seems to me a crucial case. If Sam. and LXX both come from the same "unofficial recension," how is it that the one supports M.T. and the other does not? This is not a matter that any fresh critical edition of Sam. can elucidate, for the differences are too enormous to render possible the idea that Sam. may be shown here to agree with LXX. If LXX was translated from a contemporary official Palestinian copy, how comes it that it differs here so immensely from Heb.-Sam.? Look at it which way we will, this point is fatal to Gesenius's theory and all its modifications. And what applies to the concluding chapters of Exodus holds also of every other major difference between LXX and Heb.-Sam. (see further, *ibid.*). It is inevitable that any attempt to defend Gesenius should land the apologist in difficulties of this kind, because in reality Gesenius did not ascertain the facts before putting forward his theory, with the result that it is entirely out of relation to them.

Some other aspects of Skinner's argument require notice. In a note on page 109 he writes: "Of course I am aware that if even a few superior readings in the LXX can be pro-



duced, they are sufficient to prove that it depends in part on an older Hebrew than Sam." It is surely common knowledge that, even in the view of members of the modern higher critical school, there are innumerable passages in the Pentateuch where the LXX has better readings than Heb.-Sam. I say advisedly "even"; for, in common with most textual critics, I am unable to approve their method of treating the text. To my mind the true course is to examine all the available material in the light of all the resources of textual criticism and so form a critical text,—not to take the Massoretic text on trust, except where it presents some more or less insuperable difficulty or runs counter to some pet theory, and then patch it from some Version. But as Skinner would refuse assent to my textual principles, I must content myself for the moment with pointing to the fact that even evolutionary critics recognize Septuagintal readings as superior in very many passages. Then Skinner writes: "But in the same way, the joint MT and Sam. text has a few errors which the LXX does not share, just as the LXX contains a number of undoubted corruptions which are not found in the Heb.-Sam.; and again we infer that both these classes of errors are later than the divergence of the Hebrew basis of the LXX from the parent stock of the MT and Sam. But that obviously means that the Hebrew basis of the LXX has branched off from the common stem at a point nearer the origin than the bifurcation of Sam. and MT" (p. 110). Of this argument he says: "To the principle involved in this abstract reasoning it does not seem to me that any exception can be taken." But he has done me something less than justice in his statement. In formulating my position I was careful to write: "Variants that through their intrinsic superiority appear to preserve an original text against later corruption or glossing

carry with them a very different amount of conviction from variants that can be explained as being due to editorial causes" (Expositor, Sept. 1911, p. 215), and in a footnote I say: "For numerous examples see my Origin of the Pentateuch and the literature there cited." So far as I can see, Skinner has entirely ignored this. Yet it has always been an integral portion of my argument.

Hitherto I have said nothing about the Divine appellations, because I felt that it was more satisfactory to treat the Samaritan problem on broader ground. On pages 114 f., however, Skinner deals with these, and it is right that I should point out shortly the main answers to his reasoning. "Are we to suppose," says Skinner, "that this fidelity of Jewish scribes to the text dates only from the time when the Pentateuch was taken over by the Samaritans? Is it not a reasonable assumption that great care had been exercised in this respect from the beginning by the central authorities in Jerusalem?" If the separation of the Egyptian tradition dates from the days of Jeremiah, this reasoning falls to the ground. The Egyptian text will then have been unaffected by scribal activity in Babylonia; while, on the other hand, Heb.-Sam. will be free from Egyptian developments. But, if so, we must disabuse our minds of the idea that during the whole period of the bifurcation there were "central authorities in Jerusalem" exercising a vigilant control of the text. That phase can at best have begun only after, not during, the exile, and we shall see hereafter that it may only have begun much later. Secondly, it may be possible to establish the operation of a theory on one side or the other from the effects of which the alternative line of transmission may have been free, and with regard to the Massoretic text I shall do so in a subsequent section of this article. "Un-

deniably," writes Skinner, "variations have occurred in one line of transmission or the other, but whether these be due to accident or caprice or deliberate tendency, there is no presumption whatever that they are on the side of the MT" (p. 114). Evidence will be adduced hereafter to prove: (1) that M.T. shows signs of having been altered partly through a deliberate tendency operating on the Divine appellations, and (2) that there are cases where the reading of M.T. is demonstrably wrong as against some Septuagintal reading. On the other hand, it is no part of my contention that LXX is always right and M.T. always wrong. I believe that there are cases where M.T. is demonstrably right, and that neither line of transmission is infallible. As to the accuracy of Greek copyists in the matter of the Divine appellations, I shall have something to say at a later stage. But, first, I desire to ask what this Massoretic text is that Skinner trusts so implicitly; and accordingly I proceed to consider

#### THE MASSORETIC TEXT AND THE ARCHETYPAL THEORY.

This subject is discussed by Skinner in the fourth article of the series, which appeared in the *Expositor* for July, 1913. He states some of his salient positions as follows:—

"The leading fact is that for the last eighteen centuries at least there has existed a recognized *standard text*, which has been the norm by which the correctness of all MSS. has been judged. Of course the standard text is represented only by MSS. and (since the fifteenth century) in printed editions; but the consensus of MSS. does not constitute its sole or chief authority. Its transmission has been carefully guarded by a succession of official custodians, at first by the Sopherim or scribes, and later by those known as the Massorettes; and these authorities have sought to regulate it and maintain its

purity, not merely by extreme care in the copying of MSS., but still more by the invention of the elaborate system of rules and observations which is called the *Massora* (= 'tradition'). Many of these observations go back to a remote antiquity (some probably to pre-Christian times); most of them perhaps date from the flourishing period of the Massoretes from about the sixth to the tenth century; but the development and expansion of the system was not arrested till the introduction of printing towards the end of the fifteenth century. That the scheme was not entirely successful appears in the fact that in spite of its slight differences do occur in MSS.; that it was very nearly successful is shewn by their surprising unanimity" (pp. 26-27).

"The standard text contains stereotyped errors and defects which were recognised as such by those responsible for its maintenance; and also eccentricities which, though not exactly errors, are purely accidental, and have no value in themselves apart from some traditional prejudice. There are words omitted which are necessary to the sense, and which were accordingly supplied in the reading; and others inserted where they make nonsense, and omitted in reading; words and letters marked by peculiar dots (*puncta extraordinaria*); letters written too large or too small, or suspended over the line; vacant spaces in the text; and so on. Yet the scribes and Massoretes, though perfectly aware of these errors, nevertheless endeavoured to perpetuate them with the same assiduous fidelity as the essential elements of the text. How can this singular procedure be accounted for? It is plain that the eccentric phenomena just described must have originated as accidental peculiarities of a single imperfect codex, which for some reason was regarded with such veneration that its very faults were canonised. We are thus driven to the con-

clusion that some one defective MS. has been adopted as an 'archetype' by the authors of the standard Hebrew recension, and that a persistent effort has been made to bring the whole MS. apparatus into mechanical conformity with it. Since the standard text can be traced back to the middle of the second century, it follows that the archetypal codex is at least of older date than that. Probably it was some highly venerated MS. which had survived the storm of the Roman wars and the rebellion of Bar Cochba, and was accepted on account of its antiquity as the best available norm for the sacred text at the time when the scribes were engaged in forming an official recension of the Old Testament scriptures.

"From these facts many of the most distinguished of recent scholars have drawn the very plausible conclusion that all existing Hebrew MSS. have been produced by a succession of slavishly literal transcriptions from the original code which chance or necessity had elevated to the position of an archetype for the whole authorised recension" (pp. 29-30).

"It follows further that in the best event we can never gain more from a comparison of Hebrew MSS. than the readings of a single imperfect codex, to whose authority all earlier types of Hebrew text have been ruthlessly sacrificed" (p. 31).

"For myself, however, I am free to confess that I am not so satisfied of the truth of the extreme form of the archetypal hypothesis as I was at one time. For reasons which need not here be gone into, I have come to think that, while the influence of a single archetype is undeniable, it has been brought to bear on the current text not solely by the way of slavish copying, but partly through the operation of a set of Massoretic rules taken from the archetype and applied in the writing and correcting of MSS. Hence we must allow for

the *possibility* that some readings which are older than the official recension have survived as MS. variants; and it is possible that some of these have managed to slip through the ever narrowing meshes of the Massora and appear in late codices. That must be admitted as a possibility. But on the other hand, there is usually a greater probability that the variations have come in through mistakes in transcription since the establishment of the standard recension" (p. 31).

Thus, when we ask what the Massoretic text is, we are driven back to the answer: It is the text of "a single imperfect codex, to whose authority all earlier types of Hebrew text have been ruthlessly sacrificed." Mark the word "earlier." This archetype was nothing like as old as the LXX, though, of course, its text *may* have been better in the main than the Hebrew originals of the Septuagintal translators. But to questions of the age of a MS. I cannot attach the same importance as does Skinner. The really material question is, "What is the character of the readings of a MS.?" A late MS. may represent a late or bad form of text; but, equally, it may be merely the last careful transcription of an early and excellent form of text. Hence we must look beyond the date of MSS. to the character of their readings. If we attempt to do this in the case of ordinary Hebrew MSS., Skinner offers us two alternatives. The first, in which he does not believe as firmly as he did at one time, is that every extant Hebrew MS. is ultimately derived from the single imperfect archetype. The second, which I hold to be correct, is that the influence of a single imperfect archetype has operated partly through rules taken from it and applied to the writing and correcting of MSS., with the result that there is a possibility that some of the variants of Hebrew MSS. are not due to errors in propagating the

archetypal text, but represent readings of other texts which have "managed to slip through the ever narrowing meshes of the Massora and appear in late codices." With regard to the standard text itself, in addition to the far-reaching admissions Skinner is compelled to make in the passages cited above, he writes: "The selection of MSS. may not always have been judicious, or the standard text itself may be at fault; and therefore it is perfectly in order to argue . . . that a different text from the Massoretic is to be preferred" (page 28, *n.*). Now in these admissions he virtually concedes all that I require. Grant that the Massoretic text is nothing more than the text of a single faulty archetype, grant that extant Hebrew codices may have preserved some early readings; and all that is necessary to prove that they in fact have done so is to produce divergent readings of theirs which have ancient support from some independent source. On the other hand, it should be noted that if Skinner's modified view of the archetypal theory is true, there will be a tendency for those MSS. which originally showed the greatest variations from the standard text to exhibit signs of correction. Hence it may easily be that, in the eyes of an eighteenth-century critic like de Rossi, a MS. appears to be a bad witness, when, in reality, the very features that made him think badly of it are due to its having originally preserved early readings that differed from those of the standard text. For this reason it appears to me that Skinner is wrong in attaching importance to de Rossi's judgments of the Hebrew MSS. and their ages. The true course is to consider the age of their readings, and this is the course I have always followed. Skinner himself ends by admitting this. "But in justice to my opponents I must now go on to note that they rely not so much on the unsupported evidence of Hebrew MSS. as on the *agreements*

of many of their variants with readings found in one or more of the ancient versions. This, they think, is a very strong proof that the readings in question are derived from a Hebrew original independent of the MT. Now in so far as the Samaritan Pentateuch and versions other than the LXX are concerned, the matter will be considered in the next article; and it is enough for the present to point out that corroborations from these quarters are very rare, (Sam. 3, Pesh. 1, Vulg. 2) and do not all told amount to a serious challenge to the soundness of the Massoretic text" (p. 37). .I have cited the agreements of Hebrew MSS. with Greek MSS. or daughter versions of the LXX for two reasons: (1) to show that the variants were Hebrew and not internal to the Greek tradition, and (2) to show that they were ancient. I have always felt that where one or even a small number of Hebrew MSS. differed from the standard text, the variant might be due either to error in transcription or to the preservation of an ancient Hebrew variant. To attribute it to the latter is justifiable only when there is external ancient support or internal evidence to show its origin (such as intrinsic superiority or palæographical grounds). Of course it is always possible that an isolated Hebrew MS. may have kept the true reading in some passage where all other known authorities are wrong and where there are no internal grounds to show it, but in practice it is impossible to rely on such considerations. On the other hand, it is to be remembered that most of the daughter versions of the LXX are extremely old. The Latin, Sahidic, and Ethiopic probably represent texts that are largely pre-Hexaplar. Even such a version as the Georgian was made centuries before our oldest Hebrew MS. of the Massoretic text. Again, late Greek cursives often represent early forms of text, as is shown by the support they frequently re-



ceive from citations in the early patristic literature and in extant fragments of old uncials. Consequently the method applied is sound, particularly when it is remembered that the Massoretic text is the text of a single faulty archetype, which in its turn was the result of accidents in transmission and the erroneous critical principles of bygone ages. But before proceeding with the consideration of the Massoretic text, I must pause to point out that Skinner's idea that it is confirmed by the Vulgate is not correct.

#### THE MASSORETIC TEXT AND THE VULGATE.

It is very important to note that the view that the Hebrew of the Vulgate was practically identical with the Massoretic text cannot be maintained. It seems likely to be refuted very thoroughly when the new materials that are now being collected for the edition of the Vulgate which has been intrusted to the Benedictines become generally available. All who are interested in the critical controversy should study in this connection an article by Professor Hugh Pope entitled "Where are we in Pentateuchal Criticism?" which appears in the *Irish Theological Quarterly* for October, 1913 (vol. viii. pp. 375-398). It is clear that the Vulgate, so far from supporting the Massoretic text, is likely to prove second only to the Septuagint as a quarry for variants. "Thus the Vulgate omits the Divine Name altogether in i. 4, 5, 7, 17, 26, 28 — where Alexandrinus also omits it, and in ii. 3, 16.

"And if we extend our enquiry further and include chapters i.-xi. we find the Divine Name either absent in the Vulgate or in a form differing from that in the Massoretic text in iii. 22, iv. 1, vi. 3, vi. 5, vi. 6, vii. 9, viii. 1, viii. 21, xi. 9. The courtesy of the Benedictine revisers of the Vulgate at Sant Anselmo in Rome has enabled us to examine the pho-

tographic collations of manuscripts of the Vulgate which they have so laboriously gathered together. Twenty manuscripts have been collated for Genesis, and we give here the result of an examination of eighteen of them, though it must be noted that the final examination of the results has not yet been carried out, and the following statements, though tolerably certain, must not be regarded as definite or final" (p. 386).

Pope then enumerates his results for these chapters in detail, and continues: "The evidence thus tabulated leaves no doubt as to the reading of the Vulgate save perhaps in the case of iv. 1, and vii. 9. But there remains the delicate question: How far can we trust St. Jerome's minute accuracy on such points?" (p. 388). After a discussion he comes to the following conclusion: "We are justified then in saying that the Hebrew text of Genesis at the time St. Jerome made his translation, *circa* 404 A. D., was not identical — at least as far as the Divine Names are concerned — with that which the Massorettes have handed down to us" (p. 390).

But this is not his sole contribution to the study of Jerome's Latin text. The following extracts speak for themselves: "And an examination of St. Jerome's *Liber Hebraicarum Quaestionum in Genesim* will fully confirm the conclusion we have arrived at . . . . In vi. 3, however, instead of the *Deus* of the Vulgate we have in *Heb. Quaest. Dominus Deus*; in iv. 4, while remarking that Theodotion has *Dominus* which he himself retains in the Vulgate, St. Jerome reads *Deus* in *Heb. Quaest.*; in iv. 26, instead of the Massoretic and Vulgate *Nomen Domini* he reads *Nomen Domini Dei* as in LXX.; in xiii. 13, instead of the Vulgate and Massoretic *coram Domino* he agrees with LXX. in retaining *coram Deo*; similarly in xv. 7, the Vulgate and Massoretic *Ego Dominus* is

replaced by *Ego Deus*; in xvi. 11, the same phenomenon occurs, the Vulgate and Massoretic *audierit Dominus* is replaced by *audierit Deus* with LXX.; in xvii. 4, against Vulgate, Massoretic Hebrew and LXX., he reads *Dominus*; and, as a last instance, in xxii. 2, the impersonal *ait illi* of Vulgate, LXX., and Massoretic Hebrew is replaced by *Et dixit ei Deus*.

“ Now the treatise entitled *Liber Hebraicarum Quaestionum in Genesisim* was compiled either in A. D. 388 or 389 and was a preparatory study to the great work of translating the entire Hebrew Bible into Latin. St. Jerome tells us what his object was in compiling it: *Studii nostri erit, vel eorum qui de libris Hebraicis varia suspicantur errores refellere; vel ea quae in Latinis et Graecis codicibus scaterere videntur, auctoritati suae reddere*. And the instances recorded above shew that the Saint had before him at the time a Hebrew text which, at least so far as the Divine Names are concerned, differed from that which he employed when he translated the Hebrew into Latin some twelve or fourteen years later. We are not aware of any instance in which he speaks of the existence of different Hebrew readings, but it seems impossible to hold that he had before him the same Hebrew manuscript when he translated the Pentateuch in A. D. 404 as he had when he compiled the *Hebrew Questions* in 388–89. And other instances than those affecting the Divine Names shew that the Hebrew text he had before him was not the same as the present Massoretic text” (pp. 390–391).

I lay stress on the divergence of Jerome’s text from the standard Massoretic in other points than questions relating to the Divine appellations. I have myself frequently appealed to it (see *Pentateuchal Studies*, *passim*). Here let me once more quote Pope: “ It will be of interest to enquire how the

Vulgate stands in this respect. St. Jerome had access to Hebrew manuscripts which have, of course, perished long ago, and compared with which our present manuscripts are children. Moreover, he had the advantage of living on the spot and of conversing with Jews who were skilled in the Law. It is commonly said that the Hebrew text witnessed to by the Vulgate translation is substantially the same with that preserved in the current Massoretic text. But this statement is liable to misinterpretation. It is true if by 'substantially' we mean that there are not to be found considerable passages in the one which are wanting in the other. It is not true if by 'substantially' we mean that the Hebrew Massoretic text is practically word for word the same as the Hebrew text underlying the Vulgate. If anyone needs proof of this he has only to compare the Vulgate text of Exodus with the Massoretic text in chapters xxx., xxxv., 17-35; xxxvi., 1-16; xxxvii., 7-19; xxxix., 8-21; xl., 9-23, etc. It will be seen at once that St. Jerome has a consistently shorter text. He has apparently omitted nothing, but is translating from a text which existed in a more condensed form than the present Hebrew text. At times, too, he shews clearly that he has a slightly different text before him" (p. 385). In his days at any rate the single faulty archetype so revered by Skinner and the Massorettes had not attained to universal pre-eminence even in all the passages where it is supported by Sam. If it represented the official Palestinian recension, it can only be said that that recension had as yet attained a very restricted currency.

#### THE MASSORETIC TEXT.

So far we have seen that the Massoretic text is merely the text of a faulty archetype, that extant Hebrew MSS. some-

times contain genuine ancient variants derived from some source other than this archetype, and that whether or not this archetype really possessed the antiquity attributed to it by Skinner, its text had by no means attained general authority even at the end of the fourth century of the Christian era. It may be added that the testimony of the other Greek versions goes some way to supporting this. Further, in my "Studies in the Septuagintal Texts of Leviticus"<sup>1</sup> I have had occasion to note that recensions other than the Hexaplar exhibited signs of approximation to a text resembling the Massoretic in passages where Origen appears not to have known the Massoretic readings. It is therefore probable on the Septuagintal evidence alone that, even in the days of the authors of the Greek recensions, various Hebrew texts were current resembling M.T. in varying degrees in different passages. Thus the evidence of the Septuagintal recensions independently confirms the conclusions to which we have been led by the Vulgate and the other Greek translators. We have now to ask what is known of the antecedents of the archetype of M.T. and the critical principles on which its text was formed, and how far its readings are universally accepted by modern scholarship.

There is a passage in the Jerusalem Talmud (*Taanith* iv. 2), cited on page 408 of Ginsburg's "Introduction to the Massoretico-Critical Edition of the Hebrew Bible," according to which three MSS. of the Pentateuch were kept in the court of the Temple. Where they differed the reading of the majority of two was accepted against that of the dissentient codex. Only four differences are mentioned in the passage, none of much importance, save that in one place one of the codices had a reading which was not merely bad but extraor-

<sup>1</sup> *Bibliotheca Sacra*, July, 1913-January, 1914.

dinarily bad. It was not a Hebrew word at all, but seemingly corrupt Greek. Doubtless they disagreed in other cases too, but they seem to have belonged to a single type of text. Therefore the official Temple text of that time was based on three MSS. only of a single recension, and one of those MSS. contained at least one phenomenally bad reading which could have originated only at a relatively late date when Greek was current in Jerusalem, i.e. not before the days of Alexander the Great at the earliest. Yet this late MS. was of equal authority with the other two, which may have been no older. On the whole, the story suggests that it relates to a period considerably later than the time of Alexander. Quite possibly these three MSS. go back only to some Maccabæan codex which was taken to the Temple when Judas Maccabæus rededicated it. That was certainly not a time at which a sacred text could have been fixed in accordance with the best modern critical principles.

This may be supplemented by the fact that we definitely know that the scribes introduced changes into certain passages for various purposes; e.g. in Genesis xviii. 22 they substituted "and Abraham was still standing before the Lord" for the original text, according to which the Lord was still standing before Abraham. Further, the removal of indelicate expressions became a textual principle, as also the safeguarding of the Unity of the Temple and of the Divine appellations. Thus we definitely know that, at some unknown period in the history of the Hebrew text, its official guardians deliberately altered it for motives which, however praiseworthy in themselves, are not those on which a modern textual critic could proceed.

Accordingly the Hebrew standard text is nothing more than the text of a single imperfect archetype deliberately

altered on certain non-critical principles, which, if it be descended from the official Temple text, has to trace its ancestry through only three MSS., one of which is known to have contained a corrupt Greek word, and therefore to have been a late and bad authority, and all of which may probably descend from a single copy brought to the Temple in the year 166 or 165 B. C., and not based on any official transmission or critical principle. The Septuagint was at any rate older than this.

If now we ask whether this text is greatly venerated by modern editors, the answer is in the negative. I have already explained that I think their method of treating it fundamentally wrong; but a glance at the margins of Kittel's Bible will show that, even for those who accept a Massoretic text with the minimum of patches as the ideal of scholarship, the standard Hebrew has to be set aside time after time on every page.

#### THE TEXTUAL HISTORY.

Hitherto our investigations have taught us that the Massoretic text is nothing more than the text of a faulty archetype which in turn was the result in part of erroneous critical principles and errors in transmission, that while Sam. generally supports it, there is every reason to think that the Egyptian tradition separated from the Palestinian before the Samaritan, and that in any case as late as the end of the fourth and the beginning of the fifth century of the Christian era there existed MSS. much older than any now extant which were accepted by Jerome as giving the *Hebraica veritas* and differed both in the Divine appellations and in other matters very considerably from Heb.-Sam. This latter was still very far from being recognized.

On general grounds the story of the transmission would be somewhat as follows: In the days of Jeremiah the textual stream separated into two—the Egyptian and the Babylonian. The latter gives us a form of text which may have been edited during the exile and is the parent of Sam. and M.T. The Samaritan broke off from the central Palestinian tradition, but this was merely an imperfect form of text conserved and altered on non-critical principles through very stormy times. It did not enjoy any general recognition, and the Hebrew texts used by subsequent translators and editors down to the time of Jerome often differed from it very considerably, sometimes for the better. Some single imperfect MS. descended from this central Palestinian text, however, ultimately became the archetype of the Hebrew standard, and so far as possible all Hebrew MSS. were brought into accordance with it, though in some codices that are still extant we have occasional readings which are shown by comparison to be genuinely old.

#### THE LXX AND THE DIVINE APPELLATIONS.

Yet it may be argued that, at any rate in the Divine appellations, the Massoretic text is to be preferred. Thus Skinner writes:—

“On general grounds, the MT has substantial claims to be preferred to a variant of the LXX in all doubtful cases. (a) The MT is the result of successive transcriptions in one and the same language; the LXX is a translation from one language into another. It is not denied that a version *may* represent a purer text than a recension in the original language; but in the absence of proof that this is the case, the presumption is all in favour of the original, because it is not subject to the uncertainty which inevitably attends the mental pro-



cess of translation; especially when, as is abundantly clear in the case of the LXX, word-for-word translation was not aimed at. (b) The MT is the lineal descendant of the official Palestinian recension of the OT; the LXX represents at best an Alexandrian recension whose text was certainly not transmitted with the same scrupulous fidelity as that of Palestine. For (c) as regards the divine names, the Samaritan Pentateuch shows that the Palestinian text has undergone practically no change from a time prior to (or at all events not much later than) the separation of the Palestinian and Egyptian recensions. The LXX text, on the contrary, has been in a state of perpetual flux as far back as its history can be traced. It makes no difference whether this be due to accident or (as Dahse has tried to shew) to deliberate revision: on either view the fact remains that the names of God have been handled with a freedom which was not allowed to Jewish scribes. (d) While the LXX contains particular readings which are shewn by internal evidence to be superior to the Hebrew, yet an examination of its general text proves that on the whole it is inferior to the Massoretic Hebrew. I do not think that this will be disputed by any competent Old Testament scholar. The MT is often emended from the LXX, but practically never except for some superiority, real or supposed, attaching to the reading presupposed by LXX, in particular cases. (e) The liability to error is far greater in Greek than in Hebrew. In the original text we have the distinction, not easily overlooked, between a proper name יהוה and a generic name אלהים. In Greek we have only the difference of two appellatives *κύριος* and *θεός* (often contracted in MSS. to *κς.* and *θς.*), a difference without much significance to a Greek-speaking writer, and

therefore apt to be effaced through the natural predilection for *θεος*' (Expositor, Sept. 1913, pp. 272-274).

"But it will be asked, Is it credible that the MT has preserved the original names with even the substantial fidelity which is the necessary condition of successful analysis? Granted that it is the best text available, it is still a stupendous assumption that any text can have retained even approximately the names as they stood in the hypothetical primary documents of Genesis! It would certainly be a very remarkable phenomenon. But before we pronounce it incredible, we must reflect that the divine names have in fact been transmitted with only the slightest variation since the fourth century B.C. A century more brings us to the redaction and promulgation of the Pentateuch; and it is only reasonable to suppose that during that century the preservation of the canonical text was as carefully attended to by the Temple authorities in Jerusalem as in the ages that followed. . . . We do not know what precautions were taken to safeguard the integrity of the text; and we do know that Jewish scribes were capable of an astonishing degree of accuracy in transcribing the names for God. From the examples given below, it will be seen that it is by no means an extravagant assumption to hold that in the MT we have a substantially correct reproduction of the divine names as they stood in the original documents" (pp. 278-279).

Much of this has already been answered in the foregoing. I now propose to say something about the work of the Septuagintal copyists, and to show that the Massoretic text is not trustworthy in regard to the Divine appellations. This will be done, first, by pointing to the fact that either scribes or (more probably) editors did in fact make far-reaching alterations in the Divine appellations of the Biblical

documents; and, secondly, by showing that in certain passages of Genesis the Massoretic text is demonstrably inferior to some of its rivals.

As to the Septuagint I may quote what I said in the *Expositor* for September, 1911. It will be remembered that Skinner has studied this, and hence significance attaches to his complete silence as to my argument: "Such too are the four instances in this chapter [sc. Gen. xiii.] where the LXX. has *θεός* for the Massoretic Tetragrammaton. That these divergencies do in fact represent different Hebrew readings I have shown elsewhere.<sup>1</sup> But in the light of recent discussions I have been led to examine two other passages of the Pentateuch to see whether there are similar divergencies between the Greek and Hebrew throughout in this matter. In the M.T. of Leviticus xvii.–xxi. I counted 51 occurrences of the Tetragrammaton. In one place (Lev. xxi. 21) the LXX. has *τῷ θεῷ σου* where the addition of the pronoun proves that its Hebrew text read 'thy God.' In all the other 50 instances *κύριος* occurs in every MS. of the LXX. with two exceptions. In xviii. 30 a single cursive (f) has *ὁς* only (for 'the Lord your God'), and in xxi. 6 one cursive of Holmes has *τοῦ θεοῦ*.<sup>2</sup> In Leviticus, at any rate, it cannot be contended that the translators shirked using *κύριος* or that Greek scribes were habitually unable to distinguish it from *θεός*. My other passage was Exodus xiv.–xviii. Here I found the Tetragrammaton 69 times in the Hebrew text; and in 10 instances some or all the LXX. MSS. presented

<sup>1</sup> *Essays in Pentateuchal Criticism*, pp. 13 ff., 36 f.

<sup>2</sup> "It should be added that in xix. 37 b k w omit the whole phrase. There are also a number of instances in these chapters where some or all Septuagintal MSS. add 'thy, your God' though the M. T. does not present such a phrase; but with the exceptions named there is no example of their reading *θεός* for *κύριος*."

the variant *θεός*. In seven places there was enough authority to give some ground for supposing that the LXX. originally had *θεός*, viz. xiv. 13, 31 3<sup>o</sup>, xv. 1 1<sup>o</sup>, xvi. 7 2<sup>o</sup>, 8 3<sup>o</sup>, 9, 33. In three other cases a single MS. [xvi. 7 1<sup>o</sup> (A), xvii. 1 (y) 15 (73 of Holmes)] has *θεός* for *κύριος*.<sup>1</sup> These figures show that with experience it is generally easy to detect the differences between genuine variants of the LXX. and intra-Greek corruptions in the matter of the Divine appellations" (pp. 209 f. with footnotes).

One other point. Skinner writes (Sept. 1913, p. 269) of the LXX that its original text "when recovered . . . cancels all the variants in its MSS. and daughter-versions." He has overlooked the fact that Hebrew texts were used by Origen and Lucian and probably by other revisers, and also by the other Greek translators such as Aquila, on whose materials Origen and others draw. Consequently a Greek variant may not be the reading of the original LXX and may yet represent one or more Hebrew MSS.

With these remarks I proceed to consider

#### THE MASSORETIC TEXT AND THE DIVINE APPELLATIONS.

From the first, textual critics have pointed to the fact that in the Massoretic text we have documents or portions of documents that occurred in two forms, marked respectively by the Tetragrammaton and Elohim (hereafter referred to as J and E in the tables). Notable instances are to be found in the parallel narratives of Chronicles and Kings, to which I drew attention on page 12 of "Essays in Pentateuchal

<sup>1</sup> "It should be added that in Ex. xiv. 10 one cursive (m) omits the whole phrase: in xvii. 1 sp supported by the old Latin omit the word on its second occurrence in the LXX (its first in M. T.); and in one or two passages there are additions, especially in xv. 26 2<sup>o</sup>, where B adds 'thy God.'"

Criticism," and in the Psalter. I here transcribe the figures relating to the latter given by Professor Pope in the valuable article in the *Irish Theological Quarterly* to which I have already referred.

|   | Book I. | II. | III. | IV. | V.             |
|---|---------|-----|------|-----|----------------|
| J | 272     | 30  | 43   | 103 | 236            |
| E | 15      | 164 | 44   | 0   | 0 <sup>1</sup> |

And, as everybody knows, there are parallel Psalms, the same Psalm being repeated with different Divine appellations (cp. xiv. with liii., xl. 13-17 with lxx.).

Textual critics have thought it sufficient as a general rule merely to refer to these instances,<sup>2</sup> because they were so well known as not to seem to need detailed comment. When we find Skinner making this amazing claim for the accuracy of the transmission of the Divine appellations in M.T. we naturally ask what he has to say in reply to this argument. The answer is short enough. On the analogy of Kings he says nothing at all! There is not an attempt to meet it. As to the Psalter, in a footnote on page 406 of the article for May, 1913, he writes: "The regular substitution of one divine name for another in writings *not yet canonised* affords no ground for the supposition that at a much earlier time sporadic changes might have been made in the oldest and most venerated part of the Canon, the Law." Having regard to the great differences in the text of the Law between LXX and M.T. and also Vulgate and M.T. in other matters, it is quite clear that Hebrew recensions exhibiting great differences among themselves were in fact in circulation until a relatively late date—many centuries after the period at

<sup>1</sup> Ps. cviii. is left out of this calculation, because it is made up of portions of lvii. and lx.

<sup>2</sup> For example, Dahse, *Textkritische Materialien*, vol. I. p. 51.

which Skinner would claim that the canonization of the Law took place. There is therefore nothing in this contention. But he cites a paper of Herrmann about Ezekiel. It seems that in the M.T. of that author Lord J occurs 217 times and J alone 218 times. Herrmann has formulated rules and exceptions to account for these; but, as Dahse has pointed out, these numbers bear their artificiality written on their face. It cannot be accidental that they are so nearly equal; and, unless we are to suppose that Ezekiel went carefully through his work and counted the occurrences to obtain this result, we must allow for editorial activity. In this connection the numbers for Book III. of the Psalms (43 J, 44 E) should be recalled. Undoubtedly the scribes have preserved the Divine appellations with great fidelity, but only after they had been fixed in an artificial form by editorial activity.

Now how far does this apply to Genesis? Is there any ground for suspecting the numbers of the occurrences in the M.T. of that book of artificiality? Here I base myself on the valuable paper by J. Hontheim entitled "Die Gottesnamen in der Genesis" which appeared in the *Zeitschrift für Katholische Theologie* for 1910 (vol. xxxiv. pp. 625 ff.). Dahse has drawn attention to it in his reply to Skinner in the *Expositor*; and, while I am unable to agree with Hontheim in all matters of detail, I think that he has made some very important discoveries and has probably shown the way to more. Hontheim operates on the Massoretic text, and for the purposes of his lists he neglects occurrences of *Elohim* where it is not a *nomen proprium* of the true God but a mere *nomen appellativum*. That is to say, he counts only the occurrences where the Tetragrammaton could be substituted, and omits instances where it is used in the construct state, or with suffixes, or in the phrase "to be for a God" (xvii. 7, 8; xxviii.

21), or where it denotes false gods (xxx. 30, 32; xxxv. 2, 4). These omissions are clearly right. Where the two words occur in chapters ii. and iii., he reckons them as instances of J not E, and under E he includes all instances of the use of *Elohim* for the true God either with or without the article (*Elohim* or *ha-elohim*). His results are very striking, for it appears that in Genesis J and E occur 165 times each, and other curious figures emerge. I begin by giving his lists.

## LIST I.

|     | Passage            | Number | E  | J  |
|-----|--------------------|--------|----|----|
| (1) | i. 1-iv. 26        | 70     | 40 | 30 |
| (2) | v. 1-ix. 29        | 37     | 24 | 13 |
| (3) | x. 1-xi. 26        | 7      | 0  | 7  |
| (4) | xi. 27-xvi. 26     | 29     | 0  | 29 |
| (5) | xvii. 1-xx. 18     | 34     | 15 | 19 |
| (6) | xxi. 1-xxv. 18     | 45     | 18 | 27 |
| (7) | xxv. 19-xxviii. 22 | 24     | 6  | 18 |
| (8) | xxix. 1-xxxvii. 1  | 42     | 32 | 10 |
| (9) | xxxvii. 2-1. 26    | 42     | 30 | 12 |

## LIST II.

|  | Passage        | Number | E  | J  |
|--|----------------|--------|----|----|
|  | i. 1-xi. 26    | 114    | 64 | 50 |
|  | xi. 27-xxv. 18 | 108    | 33 | 75 |
|  | xxv. 19-1. 26  | 108    | 68 | 40 |

Now, as already remarked, it is very striking that J and E should occur 165 times, each, in Genesis.<sup>1</sup> If we look at the second list we shall find that the two together occur 216 times altogether from the entry of Abraham onwards, i.e. in

<sup>1</sup> It may be added that the Samaritan text would give E 164 J 167.

the patriarchal history. Hontheim has pointed out that in the sexagesimal system of Babylonia 216 ( $6 \times 6 \times 6$ ) is equivalent to our 1,000 ( $10 \times 10 \times 10$ ). Surely that scarcely looks accidental, and perhaps it is not immaterial to note that the figures that confronted us in Ezekiel were 217 and 218. These look like equivalents of 1,001 and one more than 1,001 respectively. But to return to Hontheim's discoveries in Genesis. This number 216 is divided into two exactly equal halves (108 each) by the death of Abraham. One half of all the occurrences in the patriarchal history are given to the life of the great believer. Then it appears that in the history of Isaac the figures are 56 E and 55 J. Of the 55 J exactly 27 occur in the portion before the death of Abraham (No. (6) ) and 28 in Isaac's subsequent history (Nos. (7) and (8) ).

Thus far Hontheim. His other arithmetical calculations do not appear to me always to be convincing, though some of them may contain elements of truth. But he has said enough to show that the numbers of M.T. are purely artificial, though he may not in all cases have successfully tracked the motives at work. For myself I cannot help thinking that some of the occurrences may have been influenced by the sort of considerations one finds so often in rabbinic literature. Thus when I observe that the Tetragrammaton occurs 75 times in the life of Abraham I cannot help wondering whether the editors may not have been influenced by the fact that "Abram was seventy and five years old when he went forth out of Hāran" (xii. 4). If we see 30 E in the story of Joseph, it may possibly be due to the statement that "Joseph was thirty years old when he stood before Pharaoh" (xli. 46). Jacob served 14 years for his wives, and from the time of his arrival at Paddan Aram till the completion of this



service we find fourteen occurrences of J and E (J xxix. 31, 32, 33, 35; xxx. 24 + E xxx. 2, 6, 8, 17, 18, 20, 22 (*bis*) 23). It is to be remembered that in xxx. 32b K 170, 185, e, n, and Chr omit the E, and that its removal leaves a neater sentence. Is not the number here purely artificial?

It is probable that the researches of the future may reveal other instances of numerical juggling with Divine appellations alike in Genesis and other books. But the above are sufficient for our present purpose. They show that the Divine appellations in the M.T. of Genesis are the result of manipulation, and consequently worthless for any sort of documentary analysis. But further they suggest a reason for the preference for J exhibited in many passages of M.T. as contrasted with LXX. If it can be shown that in a number of passages the Greek alternative is correct it will become probable that some, at any rate, of the readings of M. T. have been influenced by the desire to make the occurrences of J and E precisely equal. The LXX, on the other hand, has escaped this particular revision, and presumably has not been directly influenced by the desire to have 216 Divine appellations in the patriarchal history. I say "directly influenced," because of course there was a constant tendency to assimilate the text of its MSS. to the current Hebrew texts of later days.

#### INFERIOR READINGS OF M.T.

I now come to passages where the M.T. is on internal grounds "demonstrably wrong." Skinner here says: "I reply that I do not believe any such case exists, and that certainly none of those that have been adduced will be found on examination to bear out the contention" (*Expositor*, July, 1913, p. 39). On page 268 of the September number he fol-

lows this up with the claim: "It has already been shewn that there is no case where a reading of the divine name can be either accepted or rejected on *internal* grounds" (p. 268). What truth is there in these statements? Dahse has shown that in xxviii. 20 the LXX is right in reading the Tetragrammaton for the Massoretic *Elohim* (pp. 49, 96) as is proved by the connection with verse 21b. Skinner admits (May, 1913, p. 408) that, "as regards xxviii. 20 he seems (pp. 96, 106) to make out a good case for יהוה as the original text"! Therefore there is at least one passage where, in Skinner's view, M.T. can be set aside in favor of a Greek reading on internal grounds. Further, in my "Essays in Pentateuchal Criticism" I discussed a number of passages where the variant is to be preferred to the Massoretic reading on internal grounds. Skinner says nothing of some of them. I do not know whether he would claim that he has at any rate attempted to answer all Dahse's passages: but, if so, his claim could not be sustained, because on page 24 of "Textkritische Materialien I" (cp. p. 33) Dahse expressly adopts what I have said on pages 17 f. of my "Essays." That gives us iv. 1 (on which see, further, Dahse, pp. 27, 42); xxx. 24 (on which see, especially, Dahse, p. 44), 27 (Dahse, p. 42), xxviii. 13b (cp. Dahse, pp. 8, 27, 96), as well as various other cases of omissions that Skinner would probably dismiss.<sup>1</sup> Now on page 417 of the May number Skinner writes of xxx. 24: "The textual evidence for E (against MT) is stronger than usual (LXX, Aquila, Symmachus, Peshitta); on the other hand *all* Hebrew MSS. and Sam. have J, which is the reading that naturally commends itself to those who believe on other grounds that two differ-

<sup>1</sup>But, in view of the numbers discussed in the preceding section, omissions may become very important.

ent explanations of the name Joseph are likely to have been derived from different sources." Note those words "*on other grounds.*" If one is determined to believe the documentary theory, then M.T. will be accepted, but of answer to the solid argument that the old name Joseph-el is to be seen gleaming through the Septuagintal "God hath added" there is no word. He then proceeds to defend the M.T. of verse 27 against Dahse. While I think the alternative reading better, I do not see that it is possible here to produce any argument of the cogency of Joseph-el. Skinner also considers Dahse's views on xxix. 32; xxxii. 24 ff. (where Dahse argues that the *Elohim* of M.T. is right!); and Genesis xix. and incidentally xiii. 10. As to xviii. 1 (see Dahse, p. 50), Skinner has a reply (*Expositor*, May, p. 413).

We are therefore justified in emphasizing the fact that while a number of passages have been adduced by the textual critics to show that on internal grounds M.T. is sometimes demonstrably wrong, Skinner has not ventured to answer some of these and in one passage (xxviii. 20) actually admits their contention. But he lays stress on his answer to our arguments on xvi. 11, where the name Ishmael is explained, and accordingly I turn to this passage. In the Massoretic text the Tetragrammaton is used, but one Hebrew MS. has *Elohim*, and this is supported by the Septuagintal *bw*, the old Latin, the Arabic and we may now add with Pope by Jerome in his "*Liber Hebraicarum Quæstionum in Genesim.*" Skinner has here essayed a defense of the Massoretic Hebrew. This falls into two parts. Of these the second is erroneous but intelligible, while the first is erroneous and unintelligible. For this reason I shall deal with the second part first.

In 1 Samuel i. 20 we read how Hannah called her son's name שמואל, Samuel, כי מיהוה "אלהיו" "for from the LORD

[*not* God] I asked him." Skinner falls into the error of saying that the case of Ishmael is "on all fours with" this (Expositor, July, 1913, p. 41). Then he maintains that either the textual critics must here substitute *Elohim* for the Tetragrammaton against all the evidence and thereby stultify their method, or else they will find that their argument in what he calls the "parallel instance of Ishmael" breaks down.<sup>1</sup> Of course the fallacy of this reasoning lies in not observing the distinction between the two cases. The explanation of a name *may* be etymological or quasi-etymological, and in that case the words from which the name is (rightly or wrongly) held to have originated or which it suggests by assonance must be given. But it may also be paraphrastic or parallelistic — assigning the reason for the name in more or less synonymous language, but avoiding the actual roots or expressions used in or recalled by the name. The respective explanations of Ishmael and Samuel illustrate this distinction, as anybody can see from attending to the first parts of the names. While nobody can doubt the connection of the Ishma (שמע) with the verb "to hear" (שמע, of which it is a part) used in the explanation, it is equally impossible to suppose that the Samu of Samuel could in the view of the narrator have had any connection whatever with שאל "to ask." In each instance a test is provided by the other half of the word. Once given the first part of the name Ishmael and the verb of the explanation, it is as difficult to maintain the Massoretic reading as to doubt the names in the "Pilgrim's Progress." On the other hand, the verb explaining Samuel requires as its yoke-fellow some word which shall not suggest to the ear the last syllable of the

<sup>1</sup> Query: does Skinner not stultify his method when he removes the Tetragrammaton from an inconvenient passage of P against all the evidence?

name so as not to spoil the parallelistic effect which is artistically reinforced by the chiasmic order.<sup>1</sup> Such a word was found in the Tetragrammaton, which here, as so often, provided the necessary parallelistic balance to a Hebrew word for God.

I now come to the first part of Skinner's discussion of this passage. His first point is that Ishmael is not "a fictitious name whose form could be changed according to the taste and fancy of the speller." Of course it is not. If it had been, there would have been no cogency whatever in the argument it furnishes against the Tetragrammaton. It is because that name is an immutable element, that we are compelled to see the corruption in the Divine appellations once it is recognized that the explanation while bearing on its face the mark of being etymological is not so in our present Hebrew. Here he has misunderstood my contention. Next he proceeds: "But they [the textual critics] all alike labour under the illusion that *El* and *Elohim* are convertible terms." I do not think they do. They know that *El* is an entirely different word from *Elohim*, but they also know that the difference between *El* and the first syllable of *Elohim* consists of two dots and a single dot in writing and of a single vowel (not always distinctly pronounced) in speaking. They further know that many etymological or quasi-etymological derivations of names with *El* contain *Elohim*, so that the choice lies between adopting it and introducing *El* into numerous passages where it is not at present found. No modern critic could possibly say definitely that, e.g., in Genesis xxxii. 29, 31 (28, 30) *Elohim* is an impossible reading. Indeed, in the former of these passages the assonance and balance with the Hebrew word for men constitute a strong argument in favor

<sup>1</sup> Samu : el :: LORD : ask — *not* ask : LORD.

of its originality. Now with regard to *El* Skinner writes: "It is an archaic name for the Deity which had ceased to form part of the ordinary spoken language before these narratives were reduced to writing, and which had to be replaced by one of the two names for God current in common speech" (p. 41). As all our witnesses to "these narratives" are in writing, it is not clear how Skinner knows — as he says he does — what form they were in before they "were reduced to writing," or that the word "had to be replaced by one of the two names for God current in common speech." But let us assume provisionally that his assertion that he "knew all this before Wiener was ever heard of" is true. What follows? Why this: that we substitute one document using *El* for two or three using *Elohim* and the Tetragrammaton. If it be really the case that this should be done, what becomes of the documentary theory, the hypothesis that we have three writers, two of whom use or prefer *Elohim* in Genesis and the third the Tetragrammaton? Is it to be replaced by a new speculation in which all our textual authorities — including the Massoretic text and the Samaritan which are so precious and trustworthy when this contention is valuable to the higher critics — are unceremoniously set aside in favor of something which, *ex hypothesi*, had ceased to form part of the ordinary spoken language, and neither is nor ever was in any of our authorities? Let me just recall Skinner's language on pages 24 and 25 of this very article: "It will be the chief object," he says solemnly, "of the remaining articles of this series to uphold the thesis that in the MT we have a recension of the divine names which . . . may fairly claim to represent, at least approximately, the names that stood in the original Genesis, or in the documents of which it was composed." And in what relation do his contentions stand to his jeer at

the textual critics for not having maintained that *El* which is found in the first hand of one codex of de Rossi's is the true reading here? If the word had to be replaced by another in our written sources and never was used in the autograph of Genesis, it would seem to follow that the testimony of de Rossi's codex must here be based on a mere scribal error.

So much for Skinner's view if we accept his assertion that he "knew all this before Wiener was ever heard of, and did not think fit to waste words in exposing his transparent incompetence." But the assertion is untrue, for it is not the fact that *El* "had to be replaced by one of the two names for God current in common speech" before these narratives were reduced to writing. In reality the word occurs a number of times in the Massoretic text of Genesis (see xxxiii. 20; xxxv. 7; xlvi. 3; xvi. 13; xxi. 33; xiv. 18, 19, 20, 22; xxxi. 13; xxxv. 1, 3, and the occurrences of *El Shaddai* (God Almighty)). Skinner's statement is therefore quite incomprehensible.

One word is, however, due to the reading of R 754 — *El*. It may be that this is correct. Equally it may be that *Elohim* and the Tetragrammaton have ousted *El* in a number of passages of Genesis where it once occurred. These things *may* be; but if we are to desert all our authorities on suppositions of this nature, there is an end to scholarly research. I originally rejected this reading, because I felt that, in view of the rarity of *El* and the frequency with which *Elohim* is used in explanations of this sort, the evidence of the first hand of a single MS. where that same first hand inserted the ordinary reading in the margin was not sufficient evidence of a genuine ancient variant *El*. The scribe of the archetype of this copy might have written the first two letters of the variant *Elohim*, thus giving the consonants of *El*, or a copyist

may have written *El* itself by accident and without any MS. authority before him, and perceiving his mistake have corrected it in the margin. The evidence is in my view too slender, though of course I concede the possibility that *El* is right.

THE DIVINE APPELLATIONS AND THE DOCUMENTARY THEORY.

Throughout his articles Skinner endeavors to minimize the importance of the Divine appellations for the documentary theory: but here, as usual, he speaks with two voices. In his first article "a scribal error, as some think" is invoked to get rid of the Tetragrammaton in P (Gen. xvii. 1; xxi. 1b) (Expositor, April, 1913, p. 299), and in the footnote on page 303 we are told that it stands in the first of these passages "by error, as I believe." Yet on pages 267 f. of his last article (Sept., 1913) we read: "It is a sound critical maxim that the correctness or originality of a reading is not to be questioned when it presents no inherent difficulty, and when all documentary evidence is united in its support. It has already been shewn that there is no case where a reading of the divine name can be either accepted or rejected on *internal* grounds; it must now be added that a considerable number of readings cannot rightly be challenged on *external* or documentary grounds. In all such cases the true text must be regarded as established." Why is this "sound critical maxim" to be set at naught when the Tetragrammaton occurs in P if the Divine appellations are without importance for the determination of that supposititious document? The answer is not far to seek. "The alternation of the Divine appellations was regarded as so important and so certain a basis for the Pentateuchal analysis, that, e.g., Dr. Driver, in the eighth edition of his 'Introduction to the Literature of



the Old Testament' (p. 21), reprints a passage from the earlier editions in which he balances the cumulative evidence of all the other criteria throughout the entire Pentateuch against the occurrence of the Tetragrammaton in two passages of P in the Massoretic text of Genesis, being obviously unable to conceive a P that used the Tetragrammaton in Genesis. It would be easy to cite numerous dicta from other leading critics to the same effect. Indeed, the three main documents that were supposed to be represented in Genesis (J, E, and P) all owed their very names to the clue, P having long been called the (first) Elohist. The theory was regarded as invulnerable, and is still so treated in many new popular books" (*Pent. Studies*, preface, pp. v, vi). And Skinner himself comes back to something very like the old position of the documentary critics on pages 276 f. of his last article.

Now it is not my purpose to weary my readers further with a detailed examination of all the matters that Skinner has brought against me or with all his numerous self-contradictions, because I think that enough has been said to enable them to judge for themselves the quality of his work, and in any case my object in this controversy is not personal. But one point must be noticed. I observe that Skinner has detected an oversight of mine. On page 26 of my "Essays" I inadvertently stated that Origen had added the word "LORD" in Genesis iii. 23 on the authority of a note in Field which referred to iii. 22 by a different numbering. I am glad to take this opportunity of correcting this slip, and I am deeply gratified to think that this is the only slip Skinner has succeeded in finding in my work after nearly four and a half years of controversy.

There are, however, two matters that must be strongly

emphasized in conclusion. The textual evidence as to the Divine appellations is most certainly not the only argument that has been advanced to show the unsoundness of Astruc's clue, as Skinner would have his readers infer. Further, the conservative case has always dealt with the other critical contentions and has also brought forward innumerable other facts and arguments to show that the documentary and evolutionary theories are rotten to the core.

On the first of these heads I need only remark that in my "Essays in Pentateuchal Criticism" I treated the textual evidence as only one of five branches of my argument as to the Divine appellations. I pointed out (1) that it is "not in fact, possible to divide the early portions of the Pentateuch into three main sources (P, E, and J), each of which shall be self-consistent in the use of the designations of God and shall also conform to a uniform practice" (p. 7), and I quoted the instances to the contrary in P (admitted above by Skinner who here wishes to set aside M.T. in order to get rid of them), E and J, showing that in the case of the last document we are confronted with the fact that the Tetragrammaton was used before (according to J) it was known, and that in addition *Elohim* occurs frequently. Then (2) I pointed to the impossible divisions of the text which the clue rendered necessary. The third argument related to the text of the Divine appellations. Fourthly, I assigned reasons for holding that in Exodus vi. 3 the correct reading is "I made known." Of this Skinner writes: "I propose for the sake of argument to make Dahse a present of this reading" (Expositor, April, 1913, p. 301). This is not as generous as it may look, because it had already been adopted by Driver and McNeile in their commentaries on Exodus. However, Skinner concedes the point, though (pp. 311 f.) it is clear that he

has not understood my reasoning, apparently thinking that when I refer to an "Israelite of the Mosaic age" (*Essays*, p. 53), I mean thereby the Deity, and ignoring the fact that I have shown by sundry citations that the ancient Hebrews had certain primitive notions about names.<sup>1</sup> Lastly (5) I showed (pp. 42 f.) that the division into J, E, and P does not meet the facts of the case, leading to the postdating of narratives by a thousand years or more and to the most absurd divisions even when not based on Divine appellations. On this again Skinner says nothing, though he has certainly been confronted with some of these arguments time and again. It is not only in my "Essays" that I have dwelt on such matters. In "The Swansong of the Wellhausen School" (now reprinted on pp. 49-89 of my *Pentateuchal Studies*) I tested Skinner's presentation of the critical case in many ways. I cannot see that he has a word to say in reply to what I there adduced.

With regard to the second head, in addition to this article there is all the other material in the various publications of Orr, Eerdmans, Dahse, and myself. The critics have shown abundantly that they are quite unequal to dealing with the matters that have been brought forward in these. Indeed, I do not know that Skinner could render a more valuable service to Biblical studies than to select some of the main topics with which we have dealt, and treat them at length in a series of articles similar to that on the Divine appellations. The fact that he would have an impossible case to defend makes no difference to the value of the service, for truth is often best advanced by having every conceivable argument put forward in favor of every possible contention.

In concluding my discussion of Skinner's articles it is right

<sup>1</sup> Cp. especially A. Troelstra, *The Name of God in the Pentateuch*.

that I should advert to the comprehensive way in which he has sought to cover the textual field. Whatever his faults, and whatever the weaknesses of his position, he has endeavored to deal with many problems of the Pentateuchal text with the result that the present controversy cannot die away until a new era has been opened in the textual criticism certainly of the Pentateuch and probably of the whole Old Testament. As illustrations of this, I instance the discussions not merely of the Septuagint and its recensions, but also of the Samaritan and the Massoretic text, to which we may now, in view of Pope's work, add the Vulgate. The issues now involved touch not merely the Divine appellations but the whole problem of the text, and it is already certain that the old views which have done duty for so long will be challenged all along the line. Whatever the ultimate outcome may be, the service to the cause of knowledge is indisputable.