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## THE GREEK GENESIS, THE GRAF-WELLHAUSEN THEORY, AND THE CONSERVATIVE POSITION

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IN *The American Journal of Semitic Languages and Literatures* for April, 1918, there is an important and significant article on "The Greek Genesis" by Professor A. T. Olmstead. A further contribution is promised, and will not improbably have been published before the present paper appears; but in these days I cannot rely on seeing the sequel by any given date, and there is too much in the first contribution that calls for early notice to render any postponement of the discussion wise. Indeed, an opportunity has now occurred where further debate seems likely to be exceptionally helpful. Unfortunately it is not possible for me, in the odds and ends of time which alone are at my disposal, to consider carefully every point that has been raised, and some of them must be left until a resumption of normal conditions makes it possible for me to tackle them in the ordinary course of my studies, but enough remains for fruitful discussion.

There are six main observations to be made on Olmstead's paper, and I will begin by stating them, because, in dealing with his views, I shall have to quote passages which illustrate more than one at a time. The importance of the paper is due to the first three. 1. It is enormously significant and entirely unprecedented that any higher critical organ in the English-speaking world should spontaneously publish a paper that so severely criticizes the treatment of the versions by the documentary theorists and concedes so much of the conservative case. 2. On a number of points Olmstead, working independently, has reached conclusions that closely resemble contentions that have been put forward in these pages. 3. On several other

points the differences are of such a character that further study and debate would probably remove, or at any rate reduce, them. 4. On the other hand, there has been an unfortunate delay in publication; and Olmstead, in order the better to show the independent resemblances between us, has intentionally refrained from bringing his article up to date. 5. He is under a misconception as to the standpoint of, I believe, many conservatives, certainly including myself. 6. He ignores the fact that the main attack on the Graf-Wellhausen theory has nothing whatever to do with the textual questions. To avoid any possibility of misconception, let me say at once that I do not believe that, if he had so much as hinted at the real state of affairs, *The American Journal of Semitic Languages* would have published him at all. Thus while I regard his attitude as unfortunate from one point of view, there is another standpoint from which it is wise and diplomatic. Better half a loaf than no bread. Better that Olmstead should succeed in printing some truth in *The American Journal of Semitic Languages* than that he should be excluded altogether because he wanted to tell too much.

The two following passages illustrate more than one of the foregoing comments:—

“The present paper was begun in 1914 and virtually completed in the summer of 1915. Numerous passages have been deliberately left unchanged in order that they might be compared with the results of Wiener, whose conclusions, published in the *Bibliotheca Sacra* in recent years, as well as in *Essays in Pentateuchal Criticism*, *Pentateuchal Studies*, and *Origin of the Pentateuch*, have, in spite of their totally different purpose and their apologetic point of view, been remarkably like those which the writer has discovered, working in almost complete independence and on the basis of the work done on Kings” (p. 148, footnote 1).

“The discussion which follows was already written down when there came to hand the study of this passage by Wiener, *Bibl. Sacra*, LXXIII, 140 ff. It has been left unchanged in order that the striking coincidences in results

obtained from such different standpoints may be the more clearly shown" (p. 156, footnote 2).<sup>1</sup>

Both passages rest upon a complete misunderstanding of my standpoint, which is called "apologetic"—whatever that may mean. The difference between Olmstead and myself is much less than he supposes. Both of us are seeking to follow the truth whithersoever it may lead; both of us started with a classical training. But here comes the distinction. He came to these studies as part of his historical work, and in the course of it has become dubious about the *documentary* theory: I came to them at a time when I had not studied the higher criticism or reflected on its implications, as the result of work on comparative historical jurisprudence which enabled me to see at a glance that the *evolutionary* hypothesis—the Graf-Wellhausen theory proper, not necessarily the division into documents—was utterly false, whatever might be true.<sup>2</sup> If the legislation (subject only to textual criticism)

<sup>1</sup>This quotation refers to Gen. xxxi. Parenthetically I may note an unfortunate result of Olmstead's method. I have not been able to discover how near he thinks we can get to the original form of this passage. Apparently he believes that the Greek gives us an "Elohistic" text with certain very late interpolations, rather than two separate E and J documents. It would have been better if, after reading my paper, he had appended a note saying exactly where he agreed and differed.

<sup>2</sup>Perhaps I may take this opportunity of correcting a story that seems to have gained currency in America. It is said that Dean Wace in conversation with a Jewish scholar about the higher criticism asked what the Jews were doing, and that my first book was the reply. The conversation is, I believe, authentic, but it had nothing to do with my coming to the Biblical field, and I heard of it for the first time after the publication of *Studies in Biblical Law*. It was a study of Sir Henry Maine's writings, unaided by any other external influence whatever, that led me to take up this work. On the other hand, it was an article by Dean Wace that introduced me to the *London Churchman*, to which I contributed for some years. The *Bibliotheca Sacra* I discovered through looking up an article of Kyle's on Egypt and the sacrificial system, the title of which occurred in the bibliography of the *Theologische Literaturzeitung*.

was to be assigned to Moses on historico-legal grounds, the current theory could not stand. Of course the discovery of the actual truth and of just where the critics had gone off the rails was quite a different matter, and for years it never even occurred to me to suspect that the entire philological and theological professoriate of the leading countries of the world had simply ignored the overwhelming mass of the textual evidence. They always professed to quote the versions, and an examination of the extant readings was such an obvious and indispensable preliminary to the formation of any theory of origin that I naturally supposed that their citations from the versions represented the maximum of what could profitably be gleaned from them. That they knew nothing whatever about law was obvious at the first glance, but they did pretend to know something about textual criticism.

The standpoint of the ordinary conservative is, I think, somewhat different from that of either Olmstead or myself. In studying the higher criticism he finds two main views: viz. (1) that the Old Testament is a fraud with which God had nothing to do; and (2) that the Old Testament is a fraud to which God was a party. If, for any reason, he is led to believe that there is a righteous God Who had something to do with the Old Testament, he is precluded from accepting either of the higher critical views. Hence his opposition. On the other hand, he can have no possible objection to the view of textual critics that, in the course of transmission by human beings on perishable materials, the text has suffered deterioration; and, believing his God to be the God of truth, he is ready to sympathize with investigations which have no other object than to recover as much truth as possible.

I now pass to the evolutionary question, on which Olmstead has said nothing. He has used language which rather seems to imply that the documentary theory and the Graf-Wellhausen hypothesis are identical. That is not so. In the abstract it is possible that a documentary theory might

be true, and that, nevertheless, the view of the history and of the development of the law might be false. If *no* documentary theory be true, then of course all hypotheses that aim at dating the supposititious documents are also untrue; but the work that has been done for the demolition of the documentary theory should not be allowed to obscure the fact that the current views of the history rest on blunders so colossal as to be barely credible, so shameful that nobody who is committed to the theory dare even mention the facts and arguments by which they have been revealed. Unlike the proverbial worm, the Wellhausen critics cannot even risk indulging in the luxury of turning when trodden under foot. It is true that, in the matter of the versions and their testimony to the worthlessness of Astruc's clue and many others, the higher critics have done their best to maintain silence as long as possible; but their treatment of this matter has been clamorous advertisement in comparison with their refusal to discuss the evolutionary hypothesis. So far as I am aware, there is *one* reference to *one* little point in *one* footnote of *one* book of the Wellhausen school, viz. König's "Die Moderne Pentateuch Kritik und ihre neueste Bekämpfung." In a note on pages 97 f. he goes so far as to mention with a bewildered air that I have pointed out that an altar of the kind contemplated by Ex. xx. 24-26 could have no horns, in view of the prohibition to work the stone employed in its construction. That is all. No article discussing the fundamental errors of the theory is ever admitted to a publication controlled by the Wellhausen critics.<sup>1</sup> I speak with knowledge, because at one time or another I have tried most of them myself. On the other hand, outside of the Wellhausen circle it is different. Eerdmans did not

<sup>1</sup> How far this is carried may appear from a single instance. On one occasion I resolved to try to get a short article on a minor contention of the Wellhausen critics into one of their periodicals. I knew my argument to be unanswerable, because I had laid it before a leading Continental professor, who was quite unable to say a word in favor of the hypothesis to which he was himself

hesitate to print a paper on the subject in the *Theologisch Tijdschrift* for 1913,<sup>1</sup> although my facts were equally destructive of some of the theories of his own recently-issued volume on Leviticus. He wrote me that he did not object to publishing views that did not quite agree with his own. I have always thought that this attitude did immense credit to his scholarly spirit. Incidentally it clearly reveals one of the causes of the extraordinary inferiority of the Anglo-American critics. On the continent of Europe men seek to arrive at truth: in the universities and learned publications under the control of English and American critics no effort is spared to suppress it. Thus it comes about that no notice whatever is taken either of my publications on the subject or of Reeve's article on "Sacrifice (OT)" in the "International Standard Bible Encyclopaedia"; and men who lack alike the power to defend the Wellhausen theory and the courage to break with it continue to propagate what has clearly been proved to be indefensible.

Wellhausen's own account of his position may be found on page 368 of the English translation of his "Prolegomena": "I differ from Graf chiefly in this, that I always go back to the centralisation of the cultus, and deduce from it the particular divergences. My whole position is contained in my first chapter." Here are a few of the points:—

1. Wellhausen holds that all slaughter of domestic animals for food was sacrificial till the time of Josiah, i.e. the centralization of the cultus. This is rebutted by the following passages: Gen. xviii. 7; xxvii. 9-14; xliii. 16; Ex. xxi. 37 (EV xxii. 1), (the cattle thief); Judges vi. 19; 1 Sam. viii. 13; xxv. 11; xxviii. 24; 1 Kings xix. 21. Either committed. Accordingly I applied to Dr. Orr, who was confident that he could get a note into the periodical in question. I wrote my paper, Dr. Orr sent it in, and it was accepted, but never published. After waiting for two years I wrote a mild letter of inquiry. Six weeks later my article was returned. The point in question has never been noticed in any higher critical book.

<sup>1</sup>"Is the Graf-Wellhausen Hypothesis Tenable," pp. 195-207.

his followers can answer this or they cannot. Hitherto they have invariably ignored it.

2. The law and the history alike contemplate two entirely different kinds of altars, both of which were in use concurrently. Here I would press my readers to turn to my illustrated article "Altar" in the "International Standard Bible Encyclopaedia." From the first two figures they will see how impossible it was for any contemporary to confuse the two. The one was a cairn of earth or unhewn stones, or a single large stone, necessarily varying in size and appearance with the materials of which it was composed. It was on the level, and, as appears from the reason given for the prohibition of steps, used by laymen, not by priests (who wore breeches). It could not possibly have horns. On the other hand, there was a horned altar of bronze (or wood) of prescribed size and dimensions. It was raised, so that one "went up" on it, and served by priests. The horns were an essential feature. Both these altars appear side by side in the early history long before the date to which Deuteronomy (let alone the Priestly Code) is assigned (contrast 1 Kings i. 50 f.; ii. 28 ff.; Amos iii. 14 with the sacrifices of Saul, Adonijah, Manoah, etc.). They served different purposes, just as individual and family prayer coexist at the present day with congregational worship in public structures. Wellhausen and his school have hopelessly confused these two kinds of altar.

3. While Wellhausen postulates a period during which a plurality of "sanctuaries" was permissible, followed by a centralization, the truth is that the whole theory rests on the mental confusion imparted by the use of the term "sanctuary" and Wellhausen's failure to collect all the passages that bear on the question. An altar of earth or stones was not a "sanctuary" in any true sense. The House of the Lord with its horned altar was. Both are found side by side in the legislation and history that Wellhausen considers early; but, in addition to neglecting the evidence of the passages from Kings and Amos cited



above, he has missed Ex. xxiii. 19; xxxiv. 26; Josh. ix. 23, 27; Deut. xvi. 21 (lay altars in Deuteronomy). His whole case rests on these omissions and his inability to distinguish between a house and a cairn once he has applied the fuddling label "sanctuary" to these entirely different erections.

4. So far does this go that many of his followers have pinned the ear of the slave of Ex. xxi. 6 to the door or doorpost of a cairn which they had previously called a "sanctuary" and then mistaken for a house. I cannot put the matter more clearly than I have done in a note on page 187 of "Essays in Pentateuchal Criticism": "I have repeatedly pointed out that the confusion engendered by the word 'sanctuary' reaches its climax in the writings of such authors as Driver and Robertson Smith. The latter writes: 'The local sanctuaries were the seat of judgment, and so in the language of S [so he designates this "source"] to bring a man before the magistrates is to bring him "to God" (Exod. xxi. 6; xxii. 8, 9, Heb.)' (Additional Answer to the Libel, p. 74.) It is well known that 'the seat of judgment' was the gate of the city, not a lay altar: and it is tolerably obvious that the door or doorpost presupposed by Exodus xxi. is lacking to a stone or mound, albeit present in a gate. The stoutest opponents of the higher critics would have thought it impossible that they should be so hopelessly incompetent as to be unable to distinguish between a mound and a house, and that merely because they had called both these objects 'sanctuaries'; but, unfortunately, the facts admit of no doubt. It is never wise in matters legal or historical to call a spade a sanctified excavatory implement."

5. Wellhausen's ignorance of the distinction between substantive law and procedure and his consequent failure to observe it in his treatment of the sacrificial law is responsible for much that he has written.<sup>1</sup>

These points, and many others, will be found elab-

<sup>1</sup> See EPC, pp. 203 ff.

orated in the articles "Altar," "Asylum," "Sacrifice," "Sanctuary," in the "International Standard Bible Encyclopaedia," the sixth chapter of "Essays in Pentateuchal Criticism," and other passages of my writings. Together they constitute the true answer to the Wellhausen hypothesis. It is futile to ask Olmstead to examine them, because he would never be allowed to publish his results in *The American Journal of Semitic Languages* or any other organ under critical control.

The questions relating to the sanctuary and sacrifice are, however, of great importance in dealing with the versions. As I have come to know more of the text, I have seen reason to believe that there has been heavy temple glossing; and this is a very material point in considering the relationship of the Samaritan, the Vulgate, and the LXX to the Massoretic text.<sup>1</sup> Moreover, I have been led to think that, while Wellhausen's main blunders are patent enough, the existing Hebrew text probably presents difficulties which were absent in earlier times.<sup>2</sup> I believe that the help we may expect from this source in studying the history of the sacrificial system is not yet exhausted, and that the future may yet have many surprises in store for us.

Olmstead's own general attitude appears from the following passages:—

"The independent scholar, who is not wedded to the current theory, cannot but admit that there seems considerable need of the restatement of the versions' importance. The new attack has forced the higher criticism to reconsider the basis of positions which were fast becoming a new and rather hide-bound orthodoxy, it has demanded a more radical criticism of the Massoretic Text, it has shown a surprisingly large amount of editorial redaction of a surprisingly late date. How needed was this attack can be realized when we find the leader of the now conservative critics asserting that 'while the LXX contains particular readings which are shown by internal evidence to be

<sup>1</sup> See BS, Jan. 1915, pp. 72 ff., 110 f.

<sup>2</sup> See BS, Oct. 1916, pp. 609-619.

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' (Skinner, *Divine Names*, 166).

"If therefore, a textual critic gives the preference to LXX readings, as such, he must be prepared to maintain the *general superiority of its text*. . . . But if he essays this, he will speedily land himself in a *reductio ad absurdum* of the critical axiom with which he starts. It is notorious that the LXX contains many readings which presuppose a Hebrew text, not only inferior to the MT, but absolutely inadmissible; i.e., one which no commentator with a regard for the meaning of the passage could possibly accept' (*Ibid.*, 168 ff.).

"After such a confession of faith, or rather lack of faith, it is not surprising to find that his elaborate commentary on Genesis has no section on the versions, and that when he quotes them he is far from accurate" (p. 146 f.).

And again:—

"A renewed study of the problem is therefore not out of place, specially by one who, because of his position as a teacher of history, must necessarily take a somewhat neutral point of view, who has never been committed to any one school, and who is inclined to find much of good in 'conservative' and 'critic' alike. The purpose of the paper is not, to be sure, the reconstruction of the original text of Genesis, nor is it primarily intended to test the higher criticism or the results of the new school. Rather it is the much less ambitious one of discovering the instances where the study of the Greek translation assists the historian in the problem of the sources, and other questions are only incidentally touched" (p. 148).

His conclusion is also worthy of careful attention. His study of Astruc's clue has unfortunately been marred by his decision to leave standing what he had written in 1914-15, without reference to later work. Thus his discussion is meager and unsatisfactory, and very different from what might be expected if he now examined care-

fully all that has been written on the conservative side. But even so the result is noteworthy:

"Now just what does this all mean? In a few cases Astruc's clue is certainly misleading, in a few other cases that possibility must be left an open question. On the whole the manuscripts and versions we would use with the utmost confidence agree essentially with the Massoretic Text in their readings of the divine names. If the current theory is incorrect, that must be proved on other grounds.

"Without the later paper it is obviously impossible to sum up all the evidence for and against the theory. In certain cases we have seen the theory corrected, and other examples will be given in a later paper. The corrections may considerably modify the details; as to the theory as a whole once more we must give a *non liquet*.

"The exact situation is not, after all, quite correctly expressed in the last sentence. The higher critic has sinned in not devoting more attention to the evidence of the lower, and in some cases this has unfavorably affected his results. He has also made a strategic error in not utilizing to the full the evidence which so regularly proves, and proves in later times than he had assumed, the processes which the critical theory considers basal. In Genesis we do not have editorial redaction to the same extent as in Kings, for example, but we have enough for proof, and it is the more emphatic in that it is found in the Law. If the Law, the most sacred of the Hebrew writings, was not free from editorial redaction until long after the date of the Greek translation, *a fortiori* we may expect more elaborate editing in the less sacred. Certainly, to the student who has familiarized himself with the editorial activities indicated by the versions, there is nothing strange in the similar activities postulated by the Graf-Kuenen-Wellhausen theory" (pp. 168-169).

So before the appearance of the work of 1915 and subsequent years, Olmstead had already been driven to a position so far from that of the documentary theorists. How remote it is from their conclusions he does not seem to realize. The "editorial activities" are not merely later than anything postulated by the documentary theorists; they are destructive of the theory. A concrete instance will best show this. Take the passage in Gen. xxxi. to

which he devotes attention. According to the documentary theorists this has been brought into existence through the interlacing (*circa* 650 B.C.) of two documents—J (*circa* 850 B.C.) and E (*circa* 760 B.C.). According to Olmstead there is only a single Elohist document, no J at all, and additions after 250 B.C. One document instead of two, Astruc's clue "misleading," and a difference of six centuries in date! That in his view proves "the processes which the critical theory considers basal." I should have thought that if there was any process which could be so described, it was the compilation from two or more independent documents, and that if Olmstead's view be right at all it absolutely disproves this "basal" process.

I desire to repeat and indorse what Olmstead says of the Hebrew MSS. :—

"While the additions by this means cannot be expected to be large or important, yet it is perfectly clear that the scholar who will undertake the laborious task of recollating and studying from the genealogical point of view the various extant Hebrew manuscripts will have made a distinct contribution to the final reconstruction of the text, and it is not impossible that startling agreements with the versions may be found" (pp. 148-149).

Is it too much to hope that some wealthy American University may see its way to undertaking this enterprise? What with the larger Cambridge LXX, the Benedictine work on the Vulgate, Von Gall's edition of the Samaritan, and the textual labors of the German universities, it may reasonably be thought that this field should be appropriated by the United States before other nations intervene.

Before we turn to Olmstead's remarks as to the Samaritan, his view of Gen. xiv. must be considered.

"At the first glance we observe that the Greek itself is somewhat strange, *φαραγε*=עמק; *ιπιπος*=רכוש; *περαιτης*=עברי. The last two are unique, the other unique for the Pentateuch. A subject for thought is that Aquila has *περαιτης*, virtually the same reading. We at once begin to suspect

that the passage may be a late insertion in the Greek and so in the Hebrew original" (p. 165).

Now before arguing that different translations of Hebrew words betray a different and later rendering of the chapter *as a whole*, we must see whether these words are consistently represented throughout the chapter by the expressions to which Olmstead draws attention. The facts are as follows:  $\rho\sigma\upsilon$  occurs five times (ver. 3, 8, 10, 17 *bis*). The second passage in verse 17, "the same is the king's vale," is an obvious gloss omitted by the Greek MS. L, rightly followed by Olmstead. The other Greek MSS. have  $\pi\epsilon\delta\iota\omicron\nu$ , not  $\phi\alpha\rho\alpha\gamma\chi$ . In three of the other four passages they all have  $\kappa\omicron\iota\lambda\alpha\varsigma$ . Thus the word on which Olmstead relies is *not* habitually used by the translator of this chapter. It occurs only in verse 3, where  $g$  has  $\theta\alpha\lambda\alpha\sigma\sigma\alpha\nu$ . How it came into the text I do not at present see. It may be the rendering of another translator which has here ousted the original Greek word, or it may point to a different Hebrew. In any case it does nothing to establish a different translator for the whole chapter, seeing that it occurs only in one passage out of an original four.

$\text{שׁוֹר}$ , which, be it noted, is spelt *defectively* throughout the chapter, occurs five times (ver. 11, 12, 16 *bis*, 21). In the second occurrence in verse 16 it is omitted by the Ethiopic,  $bw, m, o, r, c.$  The other MSS. read  $\tau\alpha\ \upsilon\pi\alpha\rho\chi\omicron\nu\tau\alpha$ . I think the Ethiopic is right, but neither text helps Olmstead's theory. In verse 12 our Greek authorities have  $\tau\eta\nu\ \alpha\pi\omicron\sigma\kappa\epsilon\upsilon\eta\nu$ , which is a perfectly good rendering of the Hebrew and does not confirm Olmstead. On the other hand,  $\iota\pi\pi\omicron\varsigma$ , which occurs in the other three places, is not merely unique as a translation of the Massoretic word; it is impossible. The Greek is here quite obviously following a text which had  $\text{שׁוֹר}$ , chariots, a reading which differs only in a single letter. It is very surprising that the LXX should have found this, and very important from the historical point of view, but the fact seems indubitable.

Olmstead's third word is a gloss omitted by d and the pre-Hexaplar Ethiopic, as I had pointed out on page 470 of the BIBLIOTHECA SACRA for July, 1916. I think, however, that Olmstead's observation about Aquila shows us the origin of the expression. The earlier Greek text has here been patched from that translator to bring it into agreement with the later Hebrew.

The facts, therefore, are totally unfavorable to the suspicion that the whole of Gen. xiv. is a late insertion in the Greek, and so in the Hebrew. But they reveal some minor glossing and one very important variant. Olmstead then proceeds:—

"This would well agree with the 'significant fact that the Maccabees were called *αρχιεπισ θεου υψιστου* (Jos. Ant. xvi. 163; *Ass. Mosis* 6) . . . the frequent occurrence of פִּלְבָּשׁ as a divine name in late Pss., the name Salem in one such Ps., and Melk in (probably) another' suggesting 'that the Melk legend was much in vogue about the time of the Maccabees' (Skinner, *Genesis*, 270 f.)."

I think that there will be general agreement that the exact probative force of this, from the point of view of the attack on Gen. xiv., is *nil*. Certainly Olmstead himself seems to feel this, for he proceeds: "In all this uncertainty one thing is sure." Then comes his trump card:—

"The story was known to Eupolemus in 142–141 B.C., but it was not in this form. According to him the enemy came from Armentia, and it was to this enemy and not to the king of Sodom that he freely remitted the captives. Furthermore, the sacrifice is placed at the hieron of the city of Argarizin, 'which is, being interpreted, the mountain of the Most High' (*Frag. Hist. Graec.*, III, 212). Argarizin is without doubt Mount Gerizim. This identification could be explained as due to Samaritan influence, and it is true that Josephus makes him a Gentile (*Contr. Ap.*, i. 23). Thus we might save the Massoretic Text, but if we do so, then we also condemn the Samaritan Pentateuch of having been conformed to the Jewish after this date" (pp. 165–166).

As a matter of fact Josephus (*loc. cit.*) expressly says

that Eupolemus could not read the Hebrew writings. Therefore there are only three alternatives: either he followed a text of the LXX, or a Greek translation of the Samaritan, or no text at all. In the third case he will have been dependent on what he learnt orally. Now when we examine the context of these statements, I do not think that any doubt can be felt as to the relation or lack of relation of his narrative to the Biblical text. He tells us that Abraham discovered astronomy and astrology, went to Phœnicia and dwelt there, and by teaching the Phœnicians certain astronomical facts won the favor of the king. Then comes the incident of the Armenian war against the Phœnicians. To my mind there never was a Biblical text, Jewish or Samaritan, Hebrew or Greek, that related anything like this. Eupolemus is reproducing a mixture of fact and legendary interpretation based on our Pentateuch that bears much the same relation to history as the Charlemagne of legend does to the emperor. The mention of Mount Gerizim shows that this came through a Samaritan source. The alternative is to assume the existence of a Samaritan Greek Pentateuch which subsequently to 140 B.C. disappeared without trace, or to suppose that the Samaritan Hebrew original was deliberately discarded in favor of a later Jewish text which did not support the Samaritan cult on Mount Gerizim. Such a theory based on the authority of such a tale as this seems to me quite untenable.

The attack on Gen. xiv., therefore, breaks down completely. With regard to the Samaritan Pentateuch the view just discussed is the "other evidence" mentioned in the following extracts, which are from Olmstead's remarks on the Book of Jubilees and its textual importance:—

**"A Jew of the most undoubted orthodoxy, a stout defender of the most legalistic faith, one in close sympathy with the Maccabean royal house, had before him a text which was very much farther away from our present Hebrew than is that which is today found among the Samaritans! Such a fact, for fact it undoubtedly is, challenges explanation.**



The most obvious reply is that, in its passage through the Greek, Latin, or Ethiopic translation, it was corrected to the Greek or to its versions, but the most superficial study of the agreements, especially in its combinations, will show this view to be untenable. That the Massoretic Text was revised to the Samaritan is unthinkable, scarcely less so is the converse, yet this last seems the only hypothesis, and there is other evidence which fits with it" (p. 151).

Earlier (p. 149) he had written: "The essential agreement between the Samaritan and the standard Hebrew has been much adduced for apologetic purposes, but the evidence is rapidly increasing to prove that its text is late (*AJSL*, XXXI, 206; cf. N. Schmidt, *Jour. Bibl. Lit.* XXXIII, 31 ff.; Wiener, *Bibl. Sacra*, LXXII, 83 ff.)."

It is to be observed that the view advocated in Volume XXXI. is different from his present contention. There he argued (on evidence that to my mind was inconclusive) that the adoption of the Pentateuch by the Samaritans was late. Here he apparently abandons the hypothesis of late adoption for one of late revision. The remarks of Schmidt are very guarded. He concludes (at p. 33 of his article) that "it is impossible to prove that the Samaritan Pentateuch has remained the same since it was brought to Shechem, or that it represents an earlier type than that used by G[reek] in the third century B.C." With regard to my own attitude it surely differs from Olmstead's. I had written:—

"Against these views I set the following conception of the history of the text as being in accordance with the known facts. Hebrew and Samaritan alike are descended from the recension that was in use in the second Temple. This represented a text with very numerous comments, ritual and other. But before the Samaritan schism there had already come into existence numerous copies of the Hebrew, which in many cases antedated the Temple comments and alterations. Of these the most important for our purposes were the ancestor or ancestors of the Egyptian texts, the first of which presumably dates from the

time of Jeremiah, and the ancestor of Jerome's text, which belongs to the Babylonian-Palestinian family, but is in many respects purer than the M.T., though it contains some corruptions from which the latter is free. At the same time, throughout the earlier period there was a greater tendency for MSS. of the same family to vary, and hence later authorities have often preserved better readings where earlier witnesses had been affected many centuries previously by some corruption that ultimately became widespread. Thus it is that we may see the Vulgate, the 'Hebrew,' or any other of the later versions stepping forward from time to time with an original reading that has disappeared from M.T. and LXX.

"After the Samaritan schism the Temple text continued to deteriorate. Nevertheless it was the central text of Judaism, though formed and maintained on non-critical principles, and there was a tendency to bring all other Jewish texts more or less into conformity with it. This operated partly by sporadic changes and partly by systematic attempts, such as fixing of the text by the school of Aqiba, the elaborate changes of the scribes affecting certain passages, and the fresh renderings into Greek and other languages.

"At some period in the history of this text (which was formed on principles of which we are totally ignorant), a single MS. must have acquired a dominant authority—otherwise how explain such a reading as that of our Hebrew in Genesis iv. 8? But the task of bringing all existing copies of the Bible throughout the wide Jewish diaspora into complete accord with a single type of text was impossible of rapid accomplishment when printing was an unknown art. It took centuries, and minor variations were inevitably made in the official text during the process. Fortunately for us there still survive MSS. (of which we must hope to have good modern collations some day) which contain large numbers of variants. Still more fortunately Jerome worked on a Hebrew original which

had often escaped the glosses of the standard text with the result that his version is frequently a most valuable guide. Further, as the process of assimilating our witnesses to a single type was necessarily gradual and unequal, it repeatedly happens that in many places one witness will preserve an earlier reading against all others. The last massacre of variants only came with the final triumph of the Massorettes. At no period in the long history of the transmission of the text were the principles applied such as would commend themselves to a scientific textual critic. This outline of the history can be filled in by further research which will be able to trace the stages better by the examination of innumerable agreements and differences between the various authorities. The natural course of textual transmission was modified from time to time by theological and other theories which swept across Jewry and left their marks on the Biblical texts.

“If we could assign a date to the breaking off of the Samaritan Pentateuch it would lend precision to our views, but unfortunately that is impossible. The arguments for *circa* 330 are stated by Skinner (*Divine Names*, pp. 118-121), those for 432 by König (p. 18). The weight of historical documents appears to me to be on König's side, for the Elephantine papyri confirm the approximate date of Sanballat that may be deduced from Nehemiah xiii., but the materials are too conflicting and uncertain for any definite conclusions” (BS, Jan. 1915, pp. 123-125).

I think that in this and other portions of his article Olmstead too readily attributes to *time* what might more justly be ascribed to *place*. For instance, the Nash papyrus in Egypt, some three or four centuries after the LXX, has readings that differ remarkably from the Hebrew and Samaritan. It does not follow that the Samaritan text was adopted or recast after the papyrus was written. Or take Jerome's remarkable reading in Gen. xxxi. 24, to which I drew attention on pages 140 f. of the BIBLIOTHECA SACRA for January, 1916, “and he saw God,” for the Massoretic

"and God came to Laban the Syrian." Unquestionably the Vulgate has here preserved an earlier type of reading than the Massoretic text, the Samaritan, or the LXX; but nobody would dream of inferring that the Samaritans adopted or revised the Pentateuch for the Hebrew in or after the fifth century of the Christian era. I would ask Olmstead to examine the variants from Hebrew MSS. and the Vulgate that I have been quoting for the last few years in the *BIBLIOTHECA SACRA* (notably Oct. 1914 and Jan. 1915), and say whether they do not rather confirm my view that the universal conformation of the Hebrew texts to a single type was a late result ensued only by the labor of centuries. Similarly I cannot agree that the Vulgate is hardly more than a MS. of the current Hebrew; and, while I believe that many of its variations are due to "retention of the Old Latin text," yet there seem to me to be others which should be attributed to a different Hebrew original.

In conclusion I would notice one other point on which I cannot accept Olmstead's views. In discussing Gen. xxxi. he quotes Jubilees: "Jacob made a feast for Laban and for all who came with him, and Jacob swore to Laban that day and Laban also to Jacob that neither should cross the mountain of Gilead to the other with evil purpose. And he made a heap there for witness, wherefore the name of that place is called (The Heap of Witness) after the heap." His comment is as follows: "From this we cannot discover the exact text which lay behind it, but evidently the story was briefer and more consistent than the one in our present Greek. One point at least seems clear, that there was no pillar in the original story" (p. 158). I agree that we cannot discover what text the author of Jubilees was following, but the omission of the pillar appears to me to be due to a very different cause from that assigned by Olmstead; viz. the influence of Deut. xvi. 22, "Neither shalt thou set thee up a pillar which the Lord thy God hateth." Later Judaism generally and the writer of Jubilees in par-

ticular always tended to read back current interpretations of the texts of the Law and to make the earlier history conform to them, and we may be sure that such an author would have omitted the pillar for that reason. Indeed, its mention in the Biblical narrative may have led him to regard the whole story as somewhat unedifying and prompted its compression.