

Theology on the Web.org.uk

Making Biblical Scholarship Accessible

This document was supplied for free educational purposes. Unless it is in the public domain, it may not be sold for profit or hosted on a webserver without the permission of the copyright holder.

If you find it of help to you and would like to support the ministry of Theology on the Web, please consider using the links below:



Buy me a coffee

<https://www.buymeacoffee.com/theology>



PATREON

<https://patreon.com/theologyontheweb>

PayPal

<https://paypal.me/robbradshaw>

A table of contents for *The Churchman* can be found here:

https://biblicalstudies.org.uk/articles_churchman_os.php

THE
CHURCHMAN

JUNE, 1897.

ART. I.—THE AUTHORSHIP OF THE PENTATEUCH.

No. IX.

IN my last paper I spoke of the "fingers of a man's hand"—that of Professor Hommel—writing on the wall the approaching downfall of the school of Wellhausen and Kuenen. This time I have to speak of the handwriting of another German professor, who has added his "Tekel" to the "Mene" of Professor Hommel. I refer to Professor Harnack, who has gracefully and candidly confessed that the main position of the Tübingen school of New Testament criticism cannot be any longer maintained, in the face of unquestionable facts. Professor Sanday, in his comments on this very notable surrender, has very fairly remarked that Professor Harnack's admissions do not necessarily carry with them the abandonment of the positions at present held by Old Testament critics, because the cases of the Old and New Testament are not exactly parallel. Yet Professor Sanday will possibly forgive me for pointing out that one or two important aspects of the question appear to have escaped him. The circumstances with which the New and the Old Testament critics have to deal are doubtless very different. The former was written in a period of which we have abundant and accurate information. The latter carries us back ultimately into a period of cloudland and myth, such as the early history of every nation under heaven invariably is. Consequently, in the one case the myth or legend theory has a considerable amount of antecedent improbability against it, while, in the other case the conditions are reversed. Thus much must in all fairness be conceded. But what Professor Sanday has failed to observe is this: (1) that not only the conclusions, but the methods, of the German school are shown to be unsound by Professor Harnack's admissions, and that these

are the methods which are relied upon to establish the conclusions of the Old Testament criticism associated with the names of Kuenen and Wellhausen; (2) while the very fact that in Old Testament history we are dealing with a period of which we have very little authentic information outside the books which are being submitted to criticism ought of itself entirely to preclude that tone of confidence which German critics and their followers in England are so prone to adopt. It is absolutely unscientific to represent the conclusions of criticism alone as established truths. They may be right, or they may be wrong. But until reinforced by ascertained facts, they are at best only in the position of the researches of Leverrier and Adams, before Arago had pointed his telescope in the direction indicated, and found the star asserted to be there. The chief complaint which I, personally, am inclined to make against Professor Driver's "Introduction" is, not so much the suggestions that are made in it, as the fact that he represents the view he takes of Hebrew history as practically settled, because a few clever linguistic critics in England and abroad have agreed that it is so. I contend, on the other hand, that the question is not, and cannot be, settled upon the subjective grounds on which these unquestionably able men in their own department—which is not, be it remarked for the twentieth time, that of historical inquiry—have been content to rest it. Their conclusions are mere opinions—opinions entitled, no doubt, to respectful consideration, but still mere opinions. Established truths on scientific grounds they cannot be, until they are supported by undeniable historical testimony.

I have elsewhere¹ enumerated the points on which the Tübingen methods have failed in regard to the New Testament. I will briefly repeat them here, in order to illustrate the weak points in the theories I have been endeavouring to combat in the pages of the *CHURCHMAN*. First of all, the Tübingen school boldly denied, on *a priori* grounds, the genuineness, authenticity, and early date of nearly every book in the New Testament. Next, they insisted that the creed of the Christian Church was not contemporaneous with the author of the religion, but was a subsequent development. Thirdly, they maintained the existence of various parties in the Christian Church, and attributed the phenomena of the New Testament to the victory of one party, the Pauline, over all the others, and supposed that they had deduced the existence of these tendencies, and the victory of one of them over the rest, from the pages of the New Testament itself. These

¹ "Principles of Biblical Criticism," pp. 183-185.

theories, be it observed, are extremely similar to those laid down in the case of the Old Testament, and like them, rested on no basis of fact, but simply on the application of an extremely ingenious, minute and laborious criticism to the contents of the New Testament. This criticism was also remarkable for the largeness of the conclusions based upon very slender premisses. The results to which it led, it is now admitted, were wrong results. We are not, as Professor Sanday rightly reminds us, entitled thence to conclude that the conclusions of recent Old Testament critics are wrong also. But we are entitled to point out—as critics we should be much to blame if we did not point out—that the failure of certain methods of investigation in dealing with one set of facts justifies us in regarding with suspicion the results of these methods when dealing with another set of facts. The German school of investigators, since Professor Harnack's retreat, stand before us as partially, though not, of course, wholly, discredited. They have no right to complain if we demand a still more rigorous demonstration of the soundness of their positions than we should have been entitled to demand while the German criticism of the New Testament continued to hold its ground.

The course of our investigations now brings us to the genealogies in Genesis x. I have already, in my last paper, pointed out that there is a difference of about a thousand years between the subjective critic Professor Driver and the objective critic Professor Hommel as to the date of certain portions of this chapter. I proceed to discuss the assertion that chaps. ix. 28 to x. 7, and x. 20, 22, 23, 31, 32, are the work of P. The rest, with the exception of editorial additions (regarded by Kautzsch and Socin as consisting of vers. 9, 16, 17, 24), are said to belong to JE. Of the grounds for this singular manipulation of the materials here there is not much to be said. *Primâ facie*, it would appear to ordinary persons that when a historian inserts genealogies he would naturally insert bodily the document which contains them. The improbability of the patchwork theory is at its highest in a chapter such as chap. x. Professor Driver gives us no reasons for his anatomization of so interesting a document, beyond the fact that the critics are agreed on the point. If we refer to Wellhausen or Kuenen on the composition of the Hexateuch, we meet with nothing in the least degree resembling a scientific demonstration. These passages are assigned to various authors, not because a candid investigation of the phenomena of the chapter suggests such an arrangement, but because the assumptions of certain theorists require it. Once more, then, it is not the phenomena here which

suggest the theory, but the theory which demands this interpretation of the phenomena. The phenomena, so far as they suggest anything, suggest the very opposite of the theory. If any words in the Old Testament seem to breathe the atmosphere of high antiquity attributed to this passage by Professor Hommel, it would be the allusion to Nimrod in vers. 8, 9. Yet these are assigned by recent critics to a work composed in "the eighth or ninth century B.C." More recent criticism still, I understand, assigns vers. 8 and 9 to *different* periods. But did any author that ever was born jumble up his citations from various genealogies in such an incomprehensible way? And if he had, is there any critic that ever was born who would be able, by *à priori* methods, to detect it? Historical critics will not fail to be reminded of Gibbon's famous note: "Abu Rafe says he will be witness for this. But who will be witness for Abu Rafe?" There would seem to be absolutely no grounds for this assertion, beyond the fact that לר' is used here instead of הוליר, and that the former has been declared to be characteristic of JE, and the latter of P. Surely this is rather a slender foundation on which to rest an established conclusion. But I have already shown that this notion is extremely problematical.¹ I may go further. I may remark that if a principle such as this is sufficient to guide us to the various authors, we require a *third* contributor in vers. 1-7, and neither JE nor P. For we have here neither לר' nor הוליר, but "the sons of." Moreover, I may venture once more to ask for an explanation of the reasons for which the redactor shifts from one to the other of his authorities in ver. 8. They must have been going over the same ground. P has brought us to Cush, in vers. 6 and 7, as the son of Ham. Why does the redactor leave the one authority in ver. 8, and betake himself to another? His reasons have frequently been asked for. But they have never been given. Why not? Is it to be the unquestioned privilege of the higher criticism to pull the Old Testament Scriptures to pieces, to put the fragments together at will, to call its authors names (as Wellhausen does), to bring coarse charges of folly, incapacity, exaggeration, and forgery, against them, while it is the flattest blasphemy to ask any questions whatever about the sacred conclusions of some modern critics—to want to know the why and the wherefore of the matter on which they have dogmatized so freely? Is scientific inquiry denied the right of asking for a *rationale* of contemporary criticism of the Hebrew Scriptures—for an explanation of the reasons which induced

¹ CHURCHMAN for 1896, pp. 343, 344.

a redactor, whose existence, by the way, has never been proved, to use his authorities in so capricious a fashion? This question has often been asked, but no answer has been given. The time when an answer must be given may be delayed, but it will come at last.

Moreover, the whole character of the chapter is archaic. Were we dealing with any history but Scripture history, and even with Scripture history on any principle but the logic of foregone conclusions, we should catch at such an interesting early chronicle, and eagerly avail ourselves of the light it throws on primitive history. The language of ver. 9 is unquestionably archaic. The expression גִּבּוֹר צַיִד, for instance, has every mark of early antiquity. We meet frequently, it is true, with the well-known expression גִּבּוֹר הַיָּל. But in it the latter substantive has an adjectival force. A mighty man of valour means a very valiant man; a hero of the chase, on the contrary, means an expert and successful hunter. גִּבּוֹר again, sometimes follows the noun as an adjective. But here it is the word which follows גִּבּוֹר which is the substantive. The words "a mighty man of [the] hunt" would have an archaic flavour in any language in the world. And then the passage has every appearance of being a quotation from some early poem or narrative handed down from times contemporaneous with, or not so very much later than, those of Nimrod himself. Ewald, who, though of course not infallible, was at least as good a Hebrew linguist as subsequent writers, has remarked on the hoar antiquity of some of these old quotations. We are not entitled to dismiss Ewald on the mere *ipse dixit* of later scholars. If there be anything we learn more certainly from the history of science than anything else, it is the evanescence of theories, and the duty of modesty in maintaining them.

Professor Hommel has told us that the genealogy of Gen. x. was based on political, not on ethnological considerations, and that it fits in with the period of Thothmes III., and with no other. This view seems exceedingly probable. Certainly, the information concerning the Philistines, that they originally were connected with Egypt, does not seem to be an idea of the days of the early Kings of Judah. If first published then, but handed down orally for a considerable time, we have surely a right to some information about the period when it actually did originate, and how these curious and interesting details reached the author who has first recorded it. On the supposition that the sources of Genesis are of pre-Mosaic origin, we have at least a reasonable explanation of the phenomena presented by this chapter. Let it not be forgotten that on the subjective theory we have no satisfactory explanation whatever, either of how the infor-

mation reached the writers of the two genealogies, or why the redactor jumbled up portions of each in so strange a manner. If we are told, as of course we may be told, that P was not recording, but inventing his facts, we have further a right to ask what he was inventing them for, and why his inventions were considered worthy of notice by the redactor. Professor Hommel, it is true, has been warned by Professor Cheyne in a recent article in the *Expositor*, which strikes one as somewhat amusing, that he (Professor Cheyne) will be compelled to withdraw his support from his brother professor if the latter is not more careful what he is about. Why Professor Hommel should have more reason for dreading the withdrawal of Professor Cheyne's countenance than Professor Cheyne that of Professor Hommel "doth not immediately appear," as Richard Hooker would have said. But I trust that Professor Hommel will not be deterred from prosecuting his researches by the threats of the subjective school. He may be guilty of "learned trifling." But so may other people. Whatever his methods, he at least draws his conclusions from facts, not from hypotheses. His conclusions may possibly be wrong. The history of science is a history of successive approximations to truth. Its path is strewn with the results of imperfect inductions. Yet at least there is progress when inductions are built on facts. But everyone acquainted with the history of science knows that it stood still for two thousand years while it rested on dogma. It postulated its "must be's," it formulated its propositions on which the "best authorities" were "agreed," it refused to allow those propositions to be questioned. It was not content patiently to accumulate facts, and modestly to wait until those facts were at length sufficient in number and in range to enable the investigator to extort from them the secret they had so long concealed.

There is not much more to be said about chap. x. But it may be noted that there are not wanting signs that chap. x. is by the same hand as the much-vexed chap. xiv. In these two chapters alone do we find the cities of the plain described as Sodom, Gomorrah, Admah and Zeboiim.¹ If Zoar is omitted in chap. x. 19, chap. xiv. 22 supplies the reason. If we ask why Sodom and Gomorrah only are mentioned elsewhere, the reply would naturally be that they were the two most important of the five cities. But the fact that so early as the

¹ This fact gives the critics a little trouble, therefore to some later ones it is an interpolation. I wonder why? No reason is given. This reasoning does not strike me as altogether mathematical. I wonder what Euclid would have thought of it?

time of Abraham these cities, with the exception of Zoar, were destroyed by a terrible volcanic catastrophe, to which the whole neighbourhood at the present time bears witness, would explain how it is that only the names of the two more important of the five cities were transmitted to later ages. The general historic credibility of the narrative in Genesis xix. is, as has just been said, evidenced by the physical characteristics of the country round the Dead Sea. But there are also other slight hints in the same direction which we must not allow ourselves to neglect. The occurrence of the names Admah and Zeboiim in chaps. x. 19 and xiv. 2 tends, on the principle just enunciated, to prove the antiquity of both narratives. The omission of Zoar in chap. x. 19 is an undesigned corroboration of the early origin of chap. xix. 22. The recent discoveries of archæologists in connection with chap. xiv. have somewhat "fluttered the Volscians," who, relying on the subjective criticism, conceived themselves to have sufficiently established the unhistoric character of Genesis xiv. To this subject, however, I hope to return. For the present, it will be sufficient to say that subjective criticism must in the future be a little more modest in its assertions on this point. Professor Sayce may have failed to establish his conclusions on the subject; but at least he has succeeded in throwing considerable doubt on those of his antagonists.

The dovetailing of ver. 21 between vers. 20 and 22, 23, rests upon the linguistic considerations above referred to, and the dogma that the words "after their families," etc., are characteristic of P. I need not repeat what I have before said in commenting upon chaps. iv. and v. I would only add that the eccentric treatment of his authorities again here, by the redactor, requires explanation. We have a right to ask *why* he has taken ver. 19 from JE, 20 from P, 21 from JE, and 22, 23 again from P.

Lastly, the most careless student can hardly fail to notice his extraordinary mode of dealing with those authorities in vers. 31, 32. Relying on the foregone conclusion that the phraseology of these verses is the phraseology of P, the subjective critics have assigned these verses to him. But in that case "these" are *not* the names of "the sons of Shem, after their families," and some others are. Is it likely that the redactor, after transcribing materials derived from some other source or sources, J, or E, or JE combined, let us suppose, would have summed up in this way from another author altogether—one whom he had not been following? Or, if it be contended that the two sources were here identical, how, it may be asked, have modern critics contrived to dis-

criminate between them?¹ These two last verses might very reasonably be the work of a redactor, and, had the subjective critics asserted that this was so, no one would have been hardy enough to contradict them. But they have insisted that these words are specially characteristic of P. Consequently we are driven to the conclusion that the redactor here is not in his preternaturally acute, but in his normally feeble and inconsequent, mood—a mood in which he saw nothing absurd in taking a considerable amount of his genealogical details from one author, and then adding the summary of details, which he had *not* given, from the pages of another.

J. J. LIAS.



ART. II.—THE HISTORY OF THE WORDS OF ADMINISTRATION IN THE HOLY COMMUNION.

THE history of the words in which the elements in the Holy Communion have from time to time been distributed to the faithful must always have an interest for Christian people. Three of the Evangelists, as well as St. Paul, have been careful to record the sacred words with which our Lord originally blessed and distributed the bread and wine. We can have no doubt that in their writings we possess, at least in substance, the very words used on the occasion. We take St. Paul's language in the First Epistle to the Corinthians as being the fullest, and also as being incorporated into our own Communion Office. "The Lord Jesus the same night on which He was betrayed took bread: and when He had given thanks, He brake it, and said, This is My body, which is [broken] for you: this do in remembrance of Me. In like manner also the cup, after supper, saying, This cup is the new covenant in My blood: this do ye, as oft as ye drink it, in remembrance of Me" (1 Cor. xi. 23-25). As to the question raised by some, Did our Lord repeat these words to each of the Apostles separately? we consider it a profitless inquiry. The probability is in favour of one solemn asseveration and blessing, and then a silent distribution. It is to be remembered that at the moment our Lord was at once Speaker, Giver, and Gift. There needed no repetition of the words; it was all too real and too overwhelming. When we pass on from the upper chamber into history, as given to us in the Acts of the

¹ My meaning is this, if I have not made it sufficiently clear in the text. The words "these are the generations," etc., if taken from P, would naturally follow the genealogy P had given. They would hardly be appended by any editor or redactor in the world, however abnormally eccentric, to a genealogy extracted from another author.