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ARTICLE IV.

"THE EXODUS IN THE LIGHT OF ARCHÆOLOGY."

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THE BIBLIOTHECA SACRA has been waging a campaign against the documentary criticism of the Pentateuch for many years now, and the American representatives of that criticism have sought their only safety in silence. But no sooner is a letter received making it probable that I am well out of the way than a partisan of the theory induces two of the leading critics of the country to speak of a "clear refutation" of my positions and an "irrefutable" answer. Well, the best-laid plans of mice and men gang aft agley, and it is still possible for me to take some steps in the matter.

Before I deal with the points of archæology and criticism raised by Mr. Whatham's article, a few words must be said about the complaint he has thought proper to make regarding his treatment. It is the case that his first attempt to obtain access to the BIBLIOTHECA SACRA was unsuccessful. The reason appears clearly in the sentence he quotes from my letter. He had not troubled to familiarize himself with what I had written. Now, at the third attempt, he has still failed to perform this elementary duty, but his support by Professors Barton and Paton makes publication necessary. If conservatives complain that the documentary theorists ignore their work, they cannot refuse to give the latter a hearing when they claim that they are presenting a complete answer.

Therefore the critical professors must take the responsibility for forcing the publication of Mr. Whatham's article.

The latter has gravely misrepresented me in a way that would have been impossible for anybody who had read my original paper on "The Date of the Exodus"¹ carefully. He writes:—

"The eminent scholar, Mr. S. A. Cook, sees in the fact that Bedouin tribes were being admitted into Egypt to feed their herds on Egyptian soil in the reign of Merneptah reason for assuming that the Exodus must have taken place in the reign of Merneptah's successor. The Rev. James Balkie, referring to the foregoing fact, adds that it appears in the report of an Egyptian official dated in the eighth year of Merneptah—a report showing 'the bringing in of a tribe of Semites to the lakes of Pithom, in the land of Succoth, to feed themselves and their herds.' From this he also, with Cook and many others, puts the Exodus in the reign of Merneptah's successor or after Merneptah's Syrian campaign, which took place not later than the fifth year of Merneptah's reign (*The Story of the Pharaohs*, p. 239). This important evidence is strangely omitted by Mr. Wiener in his original article on 'The Date of the Exodus.'"

The charge of omitting this evidence is untrue; for I had quoted the material portion in full on page 467 of DE, in a footnote, as follows: "It may also be noted that the Exodus explains a fact which appears from an interesting document of the eighth year of Merneptah, viz. that there was then room in Goshen for Edomite Bedouin (Shasu). 'We have finished passing the tribes of the Shasu of Edom through the Fortress of Merneptah-Hotephirma (Life, Prosperity, Health to him) in Theku (? Succoth) to the pools of Pithom, of Merneptah-Hotephirma in Theku, in order to sustain them and their herds in the domain of Pharaoh (Life, Prosperity.

¹This has been republished and may be obtained from the Bibliotheca Sacra Company for 10 cents. I cite it as DE, and the article on "The Date of the Exodus and the Chronology of the Judges" (BS, Oct. 1917) as DECJ.

Health to him), the good sun of every land' (Breasted, *Ancient Records*, vol. iii. p. 273)."

Now it is reasonable to ask Professors Barton and Paton whether this is really the best they can do. Do they think it conducive to the advancement of learning to put forward as "irrefutable" a charge that I have omitted an important piece of evidence which I have quoted? During all these years they have not ventured to meet me in fair argument. How come they now to belittle and misrepresent my work in this way?

Turning now to the rest of Mr. Whatham's article, I find that there is singularly little to answer. In DE I began by laying stress on the fact that the possibility of proving the regnal year of the Exodus was due to the recovery of the original order of the text of Numbers. Mr. Whatham quotes writers who *without that recovery* were unable to ascertain the place of the Exodus in Egyptian history. That, of course, is merely repeating in another way what I had said at the outset. He apparently agrees that Rameses II. was the Pharaoh of the oppression, and he has not a word to say against my discussions of the Numbers text, the chronology of the period of the Judges, the building of Pithom and Raamses, the Hyksos coincidences, or the seat of the Egyptian royal residences, or my refutations of the Asher and Joseph-el mistakes and the Hammurabi and Habiri fallacies. So far as I can discover, the "clear refutation of Mr. Wiener's positions" either ignores or accepts nine tenths of them, while much of it deals with views advanced by other writers, but not by me, and with points that are irrelevant. As I understand him, however, Mr. Whatham does differ from me on certain topics, and these must now be considered.

A. Mr. Whatham thinks that the Exodus took place in

the reign of Seti II. I am glad to have the opportunity of discussing this theory. It is impossible for the following reasons:—

(1) The report already quoted, which I was charged with ignoring, shows that in the eighth year of Mørneptah tribes of Edomites were passed to the pools of Pithom in the land of Succoth. The "eminent scholar" and others who see in this a proof that the Israelites were still in Goshen have entirely overlooked the smallness of the territory and the impossibility of its sustaining any but a very moderate population. Let me cite the first living authority on this point, who, curiously enough, seems himself not to have noticed the bearing of this consideration when framing his own theory of the Exodus.

"The conditions of the sojourn in Egypt should first be grasped. It is expressly stated that the Israelites were settled in the land of Goshen, in order to be out of the way of the Egyptians. But Goshen is not a large tract of country; it is bounded on the north and south by deserts, which are too high to have been cultivated; it dwindles to a mere channel on the east; and on the west it is barred by the great city of Bubastis, which was always an important centre of Egyptian life and worship. A triangle of about ten miles in the side, with perhaps some minor extension, is all that can have been comprised in Goshen. If we make every possible allowance it cannot have covered 100 square miles.

"The population of this district is stated to have been 4,000 Bedawyn a century ago; it is now improved by agriculture to support a farming population of 12,000 persons. As the Israelites were essentially pastoral, probably the Bedawy population shows most nearly what numbers Goshen formerly supported. The Israelites must have been much like the half-settled Bedawyn of the present day, living in tents scattered over the country, with their flocks and herds" (W. M. Flinders Petrie, *Egypt and Israel* [1911], pp. 28-30).

Thus it is abundantly clear that Goshen could not carry a population of more than some 4,000 or 5,000 souls. Yet the passage I have quoted from the report is prefaced by the fol-

lowing words: "Another matter for the satisfaction of my lord's heart," which show clearly that additional population was required in this small region at the time when it was made. The fact that there was such abundant room for the Edomite tribes in Goshen by the pools of Pithom is explicable only if the Israelites who had built Pithom in the preceding reign were no longer there. It was the Exodus that had left Goshen vacant for the purpose to which it was now put.¹

(2) Chronologically it is impossible to interpolate into the Biblical narrative the period of time necessary for the postponement of the Exodus to the reign of Seti II. Rameses II., the Pharaoh of the oppression, died in 1234 B.C., according to Petrie, or 1225 B.C., according to Breasted. Seti II. came to the throne in 1214 B.C. (Petrie) or 1209 B.C. (Breasted). We cannot interpolate twenty or sixteen years into the life of Moses at this period without doing violence to the plain meaning of the Biblical narrative, nor can we reduce the period of the wanderings by this amount, for it will be remembered that the traditional forty years was exactly borne out by Petrie's dates for the second year of Merneptah and the great campaign of Rameses III. against the Northern invasion which was the historic preparation of Canaan for the Israelite conquest.²

(3) Even if it were true that we have evidence of the escape of some Syrian slaves in the reign of Seti II., it is obvious that they could not have been defeated in or near Palestine under the name of the "people of Israel" within

¹ On the numbers of the Israelites, see *Essays in Pentateuchal Criticism*, pp. 155-169; *BS*, Jan. 1917, pp. 107-110. Mr. Whatham appears not to have looked at these discussions, in spite of my letter.

² See *DE*, pp. 467 f.; *DECJ*, pp. 587 f.

five years of the death of Rameses II., whom Mr. Whatham and Professors Paton and Barton agree with me in regarding as the Pharaoh of the oppression. But the Pentateuch introduces us to a people of Israel who were so defeated at that time, and who had recently left Egypt.

(4) The Israelites of the Hebrew narratives were not slaves at all. They were a community enjoying a large measure of local organization, but subjected to *corvée*, or forced labor, under the immediate orders of their own tribal leaders acting under Egyptian control.

I have set these points out because they seem to me to meet the theory which would assign the Exodus to the later years of Merneptah almost as well as they refute the Seti theory. But the matter with which I now have to deal relates only to Seti.

(5) I have often had occasion to speak of the character of higher critical work, and it may be that some of my readers have wondered whether any men who held important professorial positions could be so incompetent as I charged the critics with being. If so, let them examine this instance carefully, and I have no doubt of the result.

"Putting, therefore, this evidence with that giving the escape of Syrian slaves across the eastern border of Egypt, slaves who in the reign of Seti II. made good their escape, although pursued by Egyptian troops (Cook, *Ency. Brit.* [11th ed.], vol. x. p. 78; Paton, *BW*, Aug. 1915), we are justified in seeing in this last incident the historical basis for the Old Testament record of the Israelite Exodus from Egypt."

So writes Mr. Whatham. *The Biblical World* is, unfortunately, not accessible to me, and I cannot therefore discover how the responsibility for this sentence should be apportioned without delaying the appearance of the article (which I am reluctant to do), but I am asking Dr. Wright kindly to ap-

pend the relevant passages of Mr. Cook and Professor Paton, and I invite my readers to act as judges between me and these men.¹

The so-called "evidence" is omitted with good reason from Breasted's "Ancient Records." But here it is as given by Brugsch:—

"I set out from the hall of the royal palace on the 9th day of the month Epiphi, in the evening, after the two servants. I arrived at the fortress of Thuku, on the 10th of Epiphi. I was informed that the men had resolved to take their way towards the south. On the 12th I reached Khetam. There I was informed that grooms, who had come from the neighborhood [of the 'sedge-city,' had reported] that the fugitives had already passed the rampart to the north of the Migdol of king Seti Mineptah" (H. Brugsch, *A. History of Egypt under the Pharaohs* [Eng. Trans., 2d ed., 1881], vol. ii. p. 138, or new edition condensed and thoroughly revised by M. Brodrick [1891], p. 320).

We have no information as to the identity of the writer.

That is the foundation, and the sole foundation, for Mr.

¹"On these grounds the Exodus may have taken place under one of his successors, and since Mineptah or Merneptah (son of Rameses), in relating his successes in Palestine, boasts that *Yisra'el* is desolated, it would seem that the Israelites had already returned. On the other hand, it has been suggested that when Jacob and his family entered Egypt, some Israelite tribes had remained behind and that it is to these that Mineptah's inscription refers. The problem is complicated by the fact that, from the Egyptian evidence, not only was there at this time no remarkable emigration of oppressed Hebrews, but Bedouin tribes were then receiving permission to enter Egypt and to feed their flocks upon Egyptian soil. It might be assumed that the Israelites (or at least those who had not remained behind in Palestine) effected their departure at a somewhat later date, and in the time of Mineptah's successor, Seti II., there is an Egyptian report of the pursuit of some fugitive slaves over the eastern frontier. The value of all such evidence will naturally depend largely upon the estimate formed of the biblical narratives, but it is necessary to observe that these have not yet found Egyptian testimony to support them. Although the information which has been brought to bear upon Egyptian life and customs substantiates the general accuracy of the local colouring in some of the biblical narratives,

Whatham's statements. Two servants — *not* Syrian slaves — take to flight. *One* individual pursues them, and in the hands of these would-be historians this is perverted into an escape of Syrian slaves pursued by Egyptian troops, and solemnly put forward as "the historical basis for the Old Testament record of the Israelite Exodus from Egypt" — "a conclusion as worked out by us which was accepted as irrefutable by the two eminent Semitic scholars, Professors Paton and Barton, to whom we had submitted this present effort for review"! And these are the men who claim for themselves and their fellows a monopoly of scholarship, and affect to despise anybody who happens to possess the very

the latter contain several inherent improbabilities, and whatever future research may yield, no definite trace of Egyptian influence has so far been found in Israelite institutions" (Encyc. Brit., vol. x. p. 78).

". . . The Pharaoh of the exodus was then one of Ramses' successors, either his son Merneptah (1225-1215 B.C.), or a still later monarch. The Hebrews then did not leave Egypt until the Nineteenth or Twentieth Dynasty.

"In a number of Egyptian texts a people called 'pw-r' are mentioned, sometimes with the determinative of the Egyptian verb 'pr, 'work,' sometimes with the determinative for 'foreign people.' Chabas first suggested that this was the phonetic equivalent of 'Ibri,' Hebrew. There has been a tendency of late to return to the view that the 'pw-r' with foreign determinative are Hebrews. These people are mentioned under Ramses II, of the Nineteenth Dynasty, and Ramses III and IV, of the Twentieth Dynasty, as a foreign population that executed forced labor for the Pharaohs on their public works. If the identification with the Hebrews be correct, we have Hebrews in Egypt as late as the Twentieth Dynasty."

"Since it is impossible to hold that all Israel entered Canaan under the Eighteenth Dynasty, or all Israel under the Nineteenth Dynasty, it seems necessary to think that part of the tribes effected a settlement under the Eighteenth Dynasty, and part later, under the Nineteenth or Twentieth Dynasty" (BW, Aug. 1915, p. 87).

scanty modicum of intelligence that is requisite for seeing through them!

B. Mr. Whatham quotes from Professor Barton that quaint product of the arid mythopoetic faculty of modern Western Orientalists, the theory that the "Leah" tribes were in Palestine while the "Rachel" tribes were in Egypt. "The two servants," it will be noted, have now become the "Rachel" tribes. In my first article I had already written: "Then there is the theory of a divided Israel. For this there is not a particle of evidence, and it is contradicted by the whole Pentateuchal record" (DE, p. 474). But of course when I wrote that, I did not know that the higher critics could or would boast of the possession of eminent Semitic professors who were capable of regarding an individual as a body of Egyptian troops and two male servants as the Rachel tribes, and accepting this as the irrefutably demonstrated historical basis of the Exodus. I will therefore now go further and produce positive evidence against the theory. If only the Rachel tribes were in Egypt, what becomes of the parts played by sons of Leah in the Joseph story? And how about the leaders of the people who left Egypt? Did Moses and Aaron belong to Rachel tribes? Or the Levites? Or the minor personages mentioned incidentally, such as Dathan and Abiram? And what of the settlement of Reuben and Gad across the Jordan? I have no hope that such considerations will produce any impression on the minds of men who see the historical basis of the Exodus in the flight of two individuals, but anybody who is capable of weighing evidence will see how impossible a theory of this kind really is.

C. I come now to Merneptah and the Israel stele.

(1) Mr. Whatham has fallen into the error (from which a careful perusal of DE would have saved him) of assuming

that Pekanan in the inscription means Canaan. It is the name of a town near Hebron.

(2) He has misunderstood my remarks about Yenoam. I threw out a suggestion — but only as one of several possibilities — that the place meant here was not necessarily the town in the north of Palestine.¹ So far as my view of the Exodus is concerned, I am absolutely content to adopt Mr. Whatham's own statement of the effect of its mention in the inscription:—

"But are we to locate the Israelites near to this town of Innuamam? Locate them in Palestine possibly, but not necessarily as far north as Innuamam, for there is nothing in the Merneptah stele which indicates that the overthrow of these Israelites is to be placed in the neighborhood of this last-named town."

With this contention I agree. As I had said in a passage Mr. Whatham has overlooked, "As already pointed out, we do not know whether the order in which the places are named is intended to be approximately geographical. If Yenoam is the place near Tyre, it is most certainly not" (DE, pp. 461 f.).

(3) The Amarna correspondence shows clearly how many local troubles there might be in Palestine without involving the presence of the Pharaoh, and it is to small affairs of this kind that I take the allusions of our stanza to be. The onus of proof lies on those who affirm that Merneptah conducted a campaign in Syria. All Mr. Whatham can say is *not* that he *did* do so, but, "Now Merneptah may *not* have personally undertaken the campaign into Syria . . . but then he *may* have done so." That is not evidence. It is gambling on an improbability; for, had Merneptah undertaken a victorious campaign, it is in the last degree unlikely that a boastful hymn written in his honor and mentioning the episodes of that

¹ I had of course examined the other occurrences of the name in the Egyptian inscriptions, and was of opinion that in all those it did mean the town in the north.

campaign should have entirely omitted all reference to his share in it, and all panegyric of his personal conduct. The argument will be found developed on page 462 of DE. Mr. Whatham has been unable to produce any better reply than "Merneptah may *not* have personally undertaken the campaign . . . but then he *may*." Let me quote again the short summary on pages 467 f. of DE: "Such are the coincidences of truth and of nothing else known to the human mind. Consider once more the long chain extending from Joseph to the death of Moses, a period of 470 years. Remember that it begins in strongly vouched coincidences between the Hyksos period, the Raamses excavation, and the 430 years, and that the history of Joseph is minutely true to Egyptian life in all the little touches. Then recall the impossibility of duplicating the building of Pithom and Raamses as store cities under one and the same Pharaoh, the length of his reign, the fact that both the Hebrew and the Egyptian records testify to the defeat of a non-territorial Israel in the early years of his successor, giving Canaan durable security from Israelitish invasion, the coincidence of the forty years with the last of the Egyptian raids, the harmony with Egypt and desert conditions revealed by the narrative of Exodus-Numbers, the unerring certainty with which all our data point to one year and one year only, the ease with which we can trace the history of Israel from season to season till the departure from Kadesh-barnea. Above all do not forget that if the facts come from the Bible on the one side they come from a multitude of different though consentient witnesses on the other, covering documentary and monumental sources, and the testimony of excavations. If that be not historic truth there is no such thing." And Mr. Whatham thinks that he can dispose of such a catena of minutely vouched coincidences, not by proving that

Merneptah was present at the battle, but by saying without a particle of evidence that he *may* have undertaken a personal campaign.

(4) There is absolutely no foundation for the following assertions of Mr. Whatham:—

"The indication is that the mention of three of the most important towns of Canaan as being in a state of revolt against Egypt shows that all Canaan, more or less, is in so great disorder that nothing less than a native Egyptian army could possibly bring peace out of so extensive a military insurrection. Askalon, Gezer, and the towns following until we get to Innuamam, could only have been subdued by an Egyptian home army, and the joy over their defeat manifested in the inscription we are considering must have also included the notice of the desolating of the Israelites by the same military forces."

There is no hint in the inscription of the employment of a native Egyptian army, or of a desolating of the Israelites by any named force, or of any connection between the various events to which allusion is made, except such as is provided by geography and their occurrence in the first few years of the reign. We cannot say whether they happened at about the same time or at considerable intervals. We have no means of judging whether some or all of them were due in whole or in part to Egyptian garrisons, or to troops sent specially from Egypt, or to native vassals or allies. "Carried off is Askalon." To what extent, or by whom, or why, or when, we are not told. Nothing beyond the fact stated can be based on such a notice. The analogy of the Amarna period and of the times of the judges points rather to separate episodes.

(5) The Israel of the inscription is non-territorial. It is determined by the sign for people, not by that for land. This fact is recognized by Professor Barton himself, in a passage quoted by Mr. Whatham, when he says that "the Leah tribes were *roaming*" (my italics). The reason for

his choice of the word "roaming" lies in the language of the inscription (see further, DECJ, pp. 582 f.).

(6) "All this of course is an assumption," writes Mr. Whatham of a large portion of his argument. Precisely: and as assumptions cannot be set against facts, it calls for no further comment.

(7) The defeat of Israel recorded in Numbers and Deuteronomy was also a defeat inflicted on a non-territorial or "roaming" Israel in Palestine so crushing that they were driven to the Hormah, which was obviously considerably nearer the border of the land than the place where battle was first joined.

To sum up: It is common ground that Rameses II was the Pharaoh of the oppression in whose reign Israel built Pithom and Raamses. According to Numbers and Deuteronomy, within five years of his death these Israelites had migrated from Egypt, and, while still on their wanderings, after initial successes against the Canaanites of the Negeb and the Amalekites (Ex. xvii.) separately, had met with a crushing defeat in Canaan at the hands of the combined forces of these peoples. We know, from the political circumstances of the time, that these tribes were under the suzerainty of Merneptah, the immediate successor of the Pharaoh of the oppression. This battle sufficed to protect Canaan from further attack by Israel until some thirty-eight years later. According to Egyptological evidence the people of Israel while roaming (to use Professor Barton's word) met with a crushing defeat in Canaan within five years of the death of Rameses II., and this with other events secured a durable peace for Palestine. Further, three years later, according to another document, the strictly limited territory around Pithom where the Israelites had been settled during

the oppression is no longer in their occupation, for Edomite Bedouin are admitted to it. Naturally I conclude that the Hebrew and Egyptian records relate to one and the same wandering Israel and one and the same defeat in Palestine during the early years of Merneptah's reign.¹ To avoid this conclusion Mr. Whatham writes, “Merneptah may *not* personally have undertaken . . . but then he *may* have done so.” Such virtue resides in his “may” that on its unsupported authority he duplicates a nation, postulating a second Israel composed of two persons. This had not left Egypt during Merneptah's reign and was still in Goshen when the Edomites arrived, though the only document that refers to these persons would lead us to look upon them as palace attendants who had nothing whatever to do with the building of Pithom and Raamses or any of the experiences of the historic Israel. Unlike the first Israel, this second “nation” consisted not of an organized community subjected to forced labor under its own leaders, but of two males of unknown nationality. By some intellectual process which I do not profess to be able to follow, all the tribes and personages mentioned in the Pentateuch in connection with the Exodus and the wanderings—even the women and children—are telescoped into this duovirate, which Mr. Whatham terms Israel, and made to descend from Rachel, though most people would feel some difficulty in disposing thus of Reuben, or Gad, or Moses, or Dathan, or Abiram. All this on the strength of a single “may.” And Professors Barton and Paton are so impressed

¹ Doubtless if Amalekite and Canaanite versions of this occurrence had been preserved, they would differ from the Hebrew and Egyptian records and diverge from each other. The credit would be ascribed to neither the God of Israel nor the Pharaoh of Egypt, but to native deities or leaders, in the one case Amalekite, in the other Canaanite.

by this magic monosyllable that they proclaim Mr. Whatham's article "irrefutable"!

Now for some of the matters on which Mr. Whatham is significantly silent. On page 463 of DE I wrote: "A careful examination of the phenomena of Numbers some years ago showed that on geographical, historical, chronological, and literary grounds the present arrangement of the text is impossible. That investigation appeared in the BIBLIOTHECA SACRA for April, 1909, and is reprinted on pages 114-138 of my 'Essays in Pentateuchal Criticism.' I cannot repeat it here, and I must content myself with saying: first, that its results are assumed in what follows; and, secondly, that nobody can form a just estimate of the immense strength of my position without studying that discussion carefully."¹ When Mr. Whatham's first reply reached me in July, 1917, I saw at once that he had not looked at that discussion, in spite of the emphatic language I had used. Consequently I again insisted on its importance in the letter to which he refers. But in spite of his professed anxiety "as a higher critic" "to give every point its due weight," there is still not a word about it. As to Professors Barton and Paton they said nothing at all of this discussion from its first appearance in 1909 till some time in 1917. Then, after some eight and a half years of consideration, they put forward Mr. Whatham's article as a clear refutation of my positions. I therefore publicly challenge these three men to publish their replies to the questions that follow. In this way all men will have an opportunity of judging whether or not the documentary theorists can meet my contention.

¹ In line 9 of p. 121 of EPC, "followed" should be read for "preceded."

For the sake of clearness the actual questions may be prefaced by a few preliminary observations.

Since the first publication of my study of the material chapters of Numbers, nine years ago, my conclusions have been reinforced and developed in three ways.

(a) The extraordinarily wide and minute testimony of archæology extended by evidence that has come to light since first I wrote has immensely fortified my positions. On this Mr. Whatham and Professors Barton and Paton may be referred to DE, DECJ, the earlier portions of the present paper, and my article in the BIBLIOTHECA SACRA for January, 1917. If they will devote to the whole of these and the material passages of my "Essays in Pentateuchal Criticism" the most intensive and thorough study of which they are capable, they will be in a better position to appreciate my meaning.

(b) I had pointed out that the death of Aaron was wrongly located on the western border of Edom. The Sahidic reading which places it in Moab (see *ante*, pp. 257 f.) has since come to my knowledge, confirming my view from another quarter.

(c) The progress of textual criticism and other studies has led to the formulation, as yet incomplete, of a library theory of the composition of the Pentateuch which provides an adequate setting for the phenomena revealed by my earlier studies of these chapters. On this I commend my papers in the BIBLIOTHECA SACRA for January and April, 1918 (with the various discussions to which incidental reference is made therein), to the most searching examination of my three opponents. It may be added that I have clues which will, I hope, enable me ultimately to throw further light on the problems of Num. xxxiii. (as well as of other passages which have no close bearing on this inquiry); but, owing to my mobilization,

this part of the investigation must be deferred. For the same reason I reserve the question whether the words "king of Arad" in xxi. 1 are a gloss (see Kittel *ad loc.*). In any case the campaign was waged in the Negeb.

Three theories as to the relevant portions of the Pentateuchal narrative now stand before the inquirer:—

I. It should be accepted in the present order and form of the Massoretic text. The advocates of this view have produced no sufficient explanation of the abnormal difficulties with which it teems.

II. The documentary critics claim that it has been produced by an intentional editorial combination of portions of older works—mainly P and JE—each of which, though at variance with the other and with D, was intelligible and self-consistent.

III. In contrast to the foregoing views, both of which agree in regarding the Pentateuch as having been written in the form of a long continuous scroll from its first composition, the library theory points to the mass of evidence which shows that at an early period it consisted of a series or library of very short writings; such as, the books of origin of the heavens and man, the books of the covenant and the song, etc. It claims that accidental damage to this library gave rise to difficulties, and that the efforts to repair these made by the men who incorporated the library in scroll form (with frequent mistakes as to the order of the writings) and their successors often served only to aggravate the confusion. Later corruptions and glossing completed the tale.

I accept the following versional readings as better than the Massoretic:—

(a) In Num. xiv. 33 I prefer Jerome's "wanderers" to

"shepherds," a difference of a single letter in Hebrew (most of the documentary theorists agree).

(β) In xx. 23 I adopt the Sahidic "Moab" for "Edom." The latter appears to me to have arisen *after* the passage had come into its present position. The inconsistency of a narrative relating to Moab with the context in which it now stands attracted notice and led to the emendation.

(γ) I regard xxxiii. 40, which is omitted by the Greek MSS. bw, as a late gloss based on the present order of the earlier narrative.

(δ) In xxxiii. 38 I follow the Syriac in reading "first" for "fifth" as the month of Aaron's death.

On the evidence of Numbers and Deuteronomy I claim that the original order of the narrative was somewhat as follows: Num. xii.; xxi. 14-21; xxi. 1-3; xiii.; xiv.; xvi.-xviii.; xx. 2-13, 22a; xxi. 4b-9. There is a lacuna here. Originally the history related the journey to the gulf of Akabah and the turn northwards. Aaron's death on Mount Hor in the border of Moab belongs to the Moabite scenes later on.

The following questions which I address to Mr. Whatham and Professors Barton and Paton will serve to focus attention on the impossibility of maintaining the documentary theory in these chapters:—

(i) How comes it that JE speaks of *the* Hormah as a place already known in Num. xiv. 45, while a subsequent passage (xxi. 3) first explains the giving of the name?

(ii) Do you believe that either JE or any other Hebrew historian in the original order of his work told that Moses after receiving a divine command to turn *to-morrow* (Num. xiv. 25b) proceeded to ignore it without rebuke or punishment for the period of time required for all the transactions narrated in the portions of the history assigned to JE which

at present lie before the narrative of the execution of the command in Num. xxi. 4b?

(iii) Do you believe that in the form of the Numbers narrative known to the author of Deut. i. 40; ii. 1, 14, the order was as at present? If so, why did the Deuteronomic writer gratuitously assert that the thirty-eight years which, according to that narrative, were spent at Kadesh, were really occupied in wanderings after the departure from Kadesh?

(iv) Why did Moses after receiving a command to turn southwards immediately (xiv. 25) endeavor to obviate the necessity for this march by seeking permission to cut across Edom? And how comes it that the historian recorded this conduct without any hint that it was a defiance of an earlier divine command or other sign of disapproval?

(v) How do you explain the extraordinary geographical eccentricity of the wanderings of the Israelites in the present order with the cut across Edom from Kadesh to Mount Hor (on either of the two views of its position) and the doubling back to the neighborhood of Hormah?

(vi) How came the Israelites, after receiving the command to turn southwards from Kadesh and suffering a grievous defeat in the Negeb to the north, to wage a successful campaign in that same northern district (embracing the very scene of their defeat) as a preliminary to turning south?

(vii) Why did they, immediately after winning a signal victory in the Negeb (xxi. 1-3), with no other reason than the divine command which they had ignored with impunity for thirty-eight years, suddenly evacuate the conquered territory and turn southwards to the gulf of Akabah?

(viii) How do you explain the Sahidic variant "Moab" in xx. 23?

(ix) Do you hold that Num. xxxiii. 40 is an original part of the text? If so, what does it mean?

It may be that one or more of my opponents, when confronted with these questions, will agree as to the historical questions involved, and accept my views, that the Negeb campaign and the negotiations with Edom originally preceded the command to turn *to-morrow*, that the thirty-eight years were spent away from Kadesh, which was finally evacuated in obedience to the divine command at a date in the third year after the Hormah defeat, and that the death of Aaron on Mount Hor belongs to the Moab scenes. These admissions would remove a number of the arguments on which the documentary theory was based. A further question, however, is even more crucial.

(x) How, on any theory of intelligent compilation from complete and orderly documents (as opposed to my hypothesis of accidental damage to a library of short writings, and consequent attempts to improve matters), do you explain the conduct of the editor in turning the consistent and intelligible narratives of JE and P into the present chaos?

These questions may, I think, suffice to bring matters to a head by concentrating attention on this portion of my case and compelling the documentary theorists to show whether or not they can meet it.