

expositors, Chrysostom, Theodore of Mopsuestia, and Theodoret. The words are a quotation from Job 13¹⁶ (LXX). There it is a judicial process that is in view, and the word means victory in the struggle for right. His contest has begun. He hopes that in nothing will he be put to shame (1²⁰, *αἰσχυνθήσομαι*—probably by denying Christ or failing to set forth His claims in the best light), but that at this very time (*καὶ νῦν*, his trial) Christ may be glorified in his person, whether by life or death (for, of course, he cannot be certain of release). Despotical tribunals were notoriously arbitrary. Still he feels justified in believing that a happy prospect awaits him in this life. It is not going beyond probability to speak (1²⁶) of his *παρουσίας πάλιν πρὸς ὑμᾶς* ('presence again with

you'), or to express his confidence (2²⁴) that he will soon visit them.

There is every reason to believe that Paul's expectation was realized (see Harnack, *Chronologie*, pp. 238–239). If the foregoing brief discussion have any validity, we can the more easily picture the actual facts on which that expectation was based; we are able more clearly to grasp the historical background of the whole situation. Plainly, this favours the hypothesis that 'Philippians' is the latest of the Imprisonment-Epistles.¹

¹ After arriving at the above conclusions, we have been gratified to find that the same general view of the situation, supported by many of the same arguments, has been taken by Zahn, *Einleitung in d. N.T.*, Bd. i. pp. 380–382, 391–392. He rejects, however, Mommsen's explanation of *πραιώριον*.

Recent Foreign Theology.

Sennacherib and Sargon.

DR. LEHMANN'S handsomely printed book,¹ though addressed to the specialist in Assyro-Babylonian history, ought to interest Old Testament scholars as well. The two problems which he sets himself to solve are: (1) the apparent discrepancy between the date given by Sennacherib, at Bavian, for the reign of Tiglath-pileser I. and other chronological records that have come to us; and (2) the vast antiquity assigned by Nabonidos to Sargon of Akkad and his son Naram-Sin. His book deals very exhaustively with these two questions, and brings together all the materials for settling them which were known up to the date of its publication. Among them the so-called Dynastic Tablet naturally occupies a prominent place. This is a tablet discovered by Mr. Pinches, which, though unfortunately mutilated, gives us the names of the Babylonian kings from the 'First Dynasty of Babylon' onward, arranged in dynasties, and with the length of each reign attached. Had the tablet been complete we should have had an exact chronology—at all events, as it was conceived by the native historians—from the foundation of the dynasty to which Khammurabi or Amraphel belonged.

¹ *Zwei Hauptprobleme der altorientalischen Chronologie und ihre Lösung*. By C. F. Lehmann. Leipzig: Pfeiffer, 1898.

The tablet is badly written, and, consequently, difficult of decipherment, even where it has not been injured or destroyed. Dr. Lehmann has made a careful examination of the numerical ciphers contained in it, and has thus been able to correct some of those given in the published copies of the text. In certain cases, however, the actual cipher must remain doubtful until a duplicate of the inscription can be found. But there is one point of chronological importance which may be considered as settled; the fourth dynasty (of Isin) lasted 132 years, and not 62 years as was at first supposed.

But before problems can be solved they must first exist; and that Dr. Lehmann's problems have any real existence seems to me more than questionable. Frankly, I do not believe in them, in spite of all the learning and historical acumen displayed in his book. Let us first take his second problem, that of the antiquity of the date (3800 B.C.) assigned to Sargon of Akkad.

Dr. Lehmann's difficulty here does not lie in the remoteness of the date, but in the fact that between the era of Sargon and that of the second dynasty of Ur, a period of a thousand years according to Nabonidos, no dated Babylonian monuments have been discovered. Hence Dr. Lehmann concludes that the interval in question had no existence. But it is dangerous to argue from the imperfection of our knowledge, more

especially in matters relating to Babylonia, where as yet only two sites have been explored with any approach to scientific completeness. The Egyptologist is familiar with the fact that while monumental remains are abundant in one period of history, they are entirely wanting in another; in spite of the care with which Egypt has been ransacked, there is still a monumental gap between the sixth and eleventh dynasties, and again between the thirteenth and the seventeenth. We have a striking example of the same fact in Babylonian history itself. The Dynastic Tablet tells us that the first dynasty of Babylon was followed by a dynasty of eleven kings, who ruled for 368 years. And yet *not a single monumental trace of any of these kings has thus far been found.* Upon Dr. Lehmann's principles the dynasty and the period of time during which it lasted ought alike to be non-existent. As a matter of fact our monumental knowledge of Babylonian history is still practically confined to three epochs,—that of Sargon of Akkad, that of Khammurabi, and that of Nebuchadnezzar II. and his successors. Even for the period which preceded the Assyrian conquest of Babylonia we have but little monumental evidence, and most of that comes from Assyrian sources. Room, moreover, must be found for the multitudinous kings whose names are given in 'unchronological order' in a tablet, the object of which is to explain the meaning of them in Semitic Babylonian, and it is only natural to suppose that some at least of these names belong to the unknown period between the successor of Naram-Sin and the founder of the second dynasty of Ur. So far as I can see, therefore, there is no need to change the date assigned by Nabonidos to Sargon of Akkad, or to imagine that a king of antiquarian tastes, who had at his disposition far more historical and chronological materials than we possess, was mistaken in his calculations.

Dr. Lehmann has much more to say on behalf of the reality of his first problem. The fragmentary chronological notices that we have of the 'Kassite' period of Babylonian history certainly seem to conflict with one another, and to me at least appear to present an insoluble puzzle. Here again we must wait for more light. But I cannot agree with Dr. Lehmann in thinking that the puzzle would be solved by correcting 'the Bavian date.' His chief argument against the latter is supposed inconsistency with the Dynastic Tablet.

The mutilated condition of the tablet, however, must not be forgotten, nor is the chronology of the tablet itself altogether beyond suspicion. In 1890, in the preface to the 3rd volume of the new series of *Records of the Past*, p. xv, I gave reasons for questioning the accuracy of the compiler of it in the case of the first dynasty of Babylon, and since then Dr. Meissner has discovered the name of a king, Immerum, who is shown by the contracts to have reigned over Babylonia at that very time, and whose name is, nevertheless, omitted in the compiler's list. And since the publication of Dr. Lehmann's book, a text which has just been published by the Trustees of the British Museum throws further doubt on the compiler's accuracy. The text consists of chronological tables which were drawn up in the reign of Ammi-zadok, the fourth successor of Khammurabi, and the number of regnal years assigned by it to the kings of the dynasty differs materially in several instances from that given by the Dynastic Tablet. Sumu-abi, the founder of the dynasty, is made to reign 14 years instead of 15; his successor, Sumu-la-ilu, 36 years instead of 35; Sin-muballidh, the father of Khammurabi, 20 years instead of 30; Khammurabi himself, 43 years instead of 55; and his son, Samsu-iluna, 38 years instead of 35. Some of these differences may be explained by the supposition that the compiler of the Dynastic Tablet included in the reigns of the legitimate kings, the reigns of princes like Pungun-ilu, Immerum, and Eri-Aku, whom he considered illegitimate; he has certainly done this in the case of the Kassite dynasty, where the seven years' rule of the Assyrian conqueror, Tiglath-Bir, at Babylon, is omitted altogether.

But, even apart from the question of the confidence to be placed in the chronological exactitude of the Dynastic Tablet, there is no ground for believing it to be inconsistent with the statement of the Bavian inscription that Merodach-nadin-akhi, the Babylonian opponent of Tiglath-pileser I., reigned about 1100 B.C. On the contrary, the Dynastic Tablet itself, when correctly read, necessitates the 'Bavian chronology.' Mr. Pinches has pointed out to me that the name of the 26th Kassite king, which has been misread Gisammeti, is really Kudhur-[Bel], whose son, Sagarakti-buryas, lived 800 years before Nabonidos. As Kudhur-Bel is placed by Dr. Lehmann in 1330 B.C., the end of the Kassite dynasty will fall in

1233 B.C. instead of 1113 B.C. as he supposes, and all disagreement between the Bavian date and the Dynastic Tablet vanishes at once. The first 'problem' thus shares the fate of the second, and is really non-existent.

But though the two points upon which Dr. Lehmann's book turns are somewhat of the nature of a will-o'-the-wisp, the book itself is a valuable contribution to ancient Babylonian history. All the historical materials at present at our disposal are given in it with a completeness and lucidity which leave nothing to be desired, and the elaborate chronological tables attached to the volume—whatever opinion we may hold of the system of chronology they embody—will be found quite indispensable by the students of early chronology, whether Babylonian, Assyrian, or Biblical. The minute examination, moreover, to which the numerical ciphers of the Dynastic Tablet have been subjected, is a guarantee that in that particular direction no fresh light is to be expected. In his interpretation of one of the numerical summaries, however, Dr. Lehmann, like the other German and French Assyriologists who have discussed the question, involves himself in needless difficulties. The literal translation of the summary at the end of the Nabonassar dynasty is neither 22 (or 31) kings, nor 22 years, but '22 dynasties.' With the epoch of Nabonassar a new chronological era began; the supremacy of Babylon passed away, and the Babylonian throne came to be filled by Assyrian conquerors. The compiler of the tablet, accordingly, pauses to note that up to that point twenty-two dynasties had ruled since the beginning of Babylonian history.

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Budde's Commentary on 'Lamentations.'

THERE is a peculiar fitness in the circumstance that the illustrious investigator of the laws of the *kinah* measure—Professor K. Budde—has been chosen as commentator on the *Klagelieder* in Mohr's *Kurzer Handcommentar*. All Old Testa-

¹ *Die fünf Megillot*. Erklärt von Budde, Bertholet, Wildeboer. Edinburgh and London: Williams & Norgate. Price 4s.

ment scholars are aware that in spite of the prior labours of de Wette, Keil, and Ewald, the Hebrew elegiac measure was practically discovered by Budde and made known to the world in his famous article in the *ZATW* (1882, pp. 1-52). What the same scholar is capable of as a commentator has been shown by his work on *Judges* in the same series as the present, and on *Job* in the *Handcommentar* of Nowack.

The short Book of Lamentations, it is safe to say, is one of the least known in the Old Testament to the average reader of the English Bible. For this the obscurity of language (we mean in the Authorized Version) and a supposed monotony of tone have been hitherto partly responsible. Yet we venture to assert that it is a book which deserves the closest study, alike for the intrinsic value of its contents and for the glimpse it gives us into Jewish modes of thought and aspirations at various periods of the post-exilic history. At various periods, we say, for, as we shall find presently, the little book cannot be regarded as a unity.

In our English Bibles, as in the Septuagint, Lamentations follows immediately after Jeremiah. This position was doubtless originally due to the notion that the latter prophet was its author. Nay, in the LXX the two books are connected by the sentence which introduces Lamentations, 'And it came to pass after Israel was led into captivity and Jerusalem laid waste, that Jeremiah sat weeping, and lamented with this lamentation over Jerusalem, and said.' Until comparatively recently the prevailing tradition in Jewish and (consequently) in Christian circles was to a similar effect. But the tendency is now all the other way, and we should have been exceedingly surprised if Budde had shown himself conservative here (for conservative he can be, as his work on *Job* shows). It may indeed be taken as a final result of criticism that Jeremiah did not write the book or any part of it. Scarcely anyone will be found to maintain the prophet's authorship of the whole, and very few recent critics assign any part of it to him. Budde has an interesting discussion of the passage 2 Ch 35²⁵, 'And Jeremiah lamented for Josiah: and all the singing men and singing women spake of Josiah in their lamentations unto this day, and they made them an ordinance in Israel: and, behold, they are written in the lamentations.' He agrees with W. R. Smith and others (against Thenius)

that these last words refer to the canonical Book of Lamentations, in spite of the strange allusion to Josiah (which, however, is easily explicable, as coming from a writer like the Chronicler). But what is of most importance, Budde points out that this early tradition did not attribute all five poems to Jeremiah, but apparently only one of them, the other four being assigned to the 'singing men and singing women.' Jeremiah being out of the question, how many hands are we to recognize in the book? Chaps. 2 and 4 are generally regarded as the oldest and fundamental part of the whole, and while Budde does not agree with Thénius that they are actually Jeremiah's, he sees no reason for denying that the two proceed from one pen, the pen of an actual eye-witness of the scenes described. Formerly he was willing to assign chaps. 1 and 5 to this same hand, holding that chap. 3 alone was from a different and later hand than the rest of the book. But in the commentary before us he agrees with Löhr that chap. 1 (which is influenced by Deutero-Isaiah, e.g. in vv.^{9f.}) is post-exilic, perhaps c. 430 B.C., or more probably later. In regard to chap. 5 he is inclined to trace its source to the remnant that was left behind in Palestine, c. 550 B.C. Chap. 3 has always been felt to belong to a different category from the rest of the book. Budde would bring it down to the pre-Maccabæan section of the Greek period, the same as that to which Cheyne (*Origin of the Psalter*) assigns Ps 119.

Regarding the singular 'I' of chap. 3, Budde contends (against Cheyne, Smend, etc.) for the individual sense, instead of understanding it of the nation collectively, after the manner of many of the late Psalms. He supposes the 'I' to be intended by the author of the poem to personate an eye-witness (probably Jeremiah) of the destruction of Jerusalem.

As to the peculiarity of the acrostic arrangement, namely, that in chap. 1, it follows the usual order of the Hebrew alphabet, whereas in chaps. 2-4 (chap. 5 is not acrostic) the letter Δ precedes Υ ('as if with us P stood before O,'—Nöldeke), Budde wisely, we think, remarks, 'There is no explanation of this deviation.'

The commentary is remarkably full, considering the limited space, and very informing, while the textual criticism is, as we always expect from Budde, brilliant and frequently convincing. The whole work is worthy of its author, and constitutes

a valuable addition to the series to which it belongs.

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The 'Western' Text of the New Testament.

THE publication of the readings of Codex Bezae in Dr. Nestle's critical edition of the Greek Testament is one of many signs of reawakened interest in the vexed question of the relation of the text represented by this MS. to the text of the oldest Greek uncials. Professor Bousset, the editor of the *Theologische Rundschau*, does not hesitate to say that this is *the* problem of the science of the textual criticism of the New Testament, and in his own article on 'The Text of the New Testament,' which appears in the July number, there is an instructive survey of the history of the discussion, which is of especial value on account of its acute and suggestive criticisms of the theory advocated by Dr. Blass.

At the outset of his inquiry Blass limited his investigations to the Acts, his conclusion being that Luke is himself the author of two editions of this book. The R. text is the rough draft which Luke wrote at Rome, the A. text is Luke's revision of his earlier work; the R. text remained in possession of the church at Rome, the A. text in its original form was the copy sent to Theophilus. In his commentary Blass directs attention to a number of statements found in the R. text which, in his judgment, a later copyist could not have added, but which Luke alone would have ventured to omit as non-essential; hence he argues that the only theory which accounts for all the variations is that which ascribes both the improvements in style and the abbreviations of the A. text to Luke himself.

Bousset is of opinion that the strictures of Corssen and Bernhard Weiss have rendered the theory of Blass quite untenable. Corssen has endeavoured to prove that the language of the R. text often lacks the characteristics of Luke's style, whilst in many places it clearly bears the marks of a later recension. Bernhard Weiss, in undermining the foundations of the hypothesis of Blass, goes further in his depreciation of the R. text than

Bousset approves, for 'he very seldom reckons with the possibility that in the R. text an original reading may be found, and he very often makes use of violent methods in tracing the origin of the R. text to the A. text.'

As examples of passages in which the R. text shows evidences of later intentional revision, Bousset cites Ac 15³²⁻⁴⁰ 18⁴⁻⁷. According to the A. text of 15³³ Judas and Silas 'were dismissed in peace' from Antioch, and yet in v.⁴⁰ Paul is said to have chosen Silas as his companion, when he left Antioch on his missionary journey. The R. text inserts after v.³³: 'But it seemed good unto Silas to abide there, but Judas went forth alone.' The hypothesis of Blass requires us to believe that Luke, having written these words, omitted them from his revision, and so made an awkward gap in his narrative; Bousset contends that they are the intentional correction of an editor who was anxious to obliterate a supposed discrepancy in Luke's history. In 18⁷ the R. text reads: 'And (Paul) departed from Aquila,' the last two words being obviously intended to remove the ambiguity of the A. text: 'He departed thence.' But in this instance the corrector has not carefully read the context; the reference is not to a change in the private residence of Paul, but to his departure from the synagogue (18⁴) to a new place of teaching. This reading is arbitrarily excluded by Blass from his reconstruction of the R. text, hence the statement that 'Corssen is right in describing this procedure as a *petitio principii*.'

On the whole question Bousset contends that the critical study of the two texts of the Acts does not yield results favourable to the theory of Blass, who is nevertheless right in maintaining that it is often impossible to assign any reason why a later editor should have introduced some of the variants which are found in the R. text. On the other hand, it cannot be denied that Bernhard Weiss has overshot the mark in his endeavour to show that in every instance the motive which led to the correction or addition may be discerned. Some place—Bousset rightly urges—must be left for the play of the editor's fancy and for accidental variations; more careful study may explain

changes, the reason for which does not lie upon the surface; the possibility that in the R. text an original reading may sometimes be found must not be excluded; but the peculiarities of the R. text of the Acts are not likely to be accounted for except as the work of an editor who was not the author, but who intentionally revised the original text soon after its publication.

In his later works Blass has extended his investigations to the two texts of the Third Gospel, his conclusion being that whereas in the Acts the R. text preceded the shorter and more elegantly written A. text, in the Gospel the relation of the one text to the other is completely reversed. In the Acts the R. text is characterized by additions to the narrative and by greater detail in the descriptions; in the Gospel the R. text is more concise and pregnant. The explanation which Blass gives of these seemingly contradictory phenomena is ingenious;—he holds that whilst Luke wrote the first draft of the Acts in Rome, he wrote the first draft of the Gospel in Jerusalem, and revised it in Rome. Hence the R. text (*forma Romana*) is in the Acts the rough draft, but in the Gospel the revised edition.

Bousset fully recognizes the splendid service which Blass has rendered to the science of New Testament criticism by his reconstruction of the R. text of the Acts; but he argues with considerable force that the more recent attempt to reconstruct the R. text of the Third Gospel is not equally successful, and must be called premature. The authorities used by Blass are Codex Bezae, with *e* and *k*, two African codices of the Old Latin version, but as witnesses to the R. text of the Gospel the oldest Syriac version and other MSS of the Old Latin version are available; it is therefore doubtful whether the reconstruction gives us the R. text or one of its descendants. For these and other reasons Bousset concludes that whilst the R. text of the Gospel contains more original readings than the R. text of the Acts, it is nevertheless a revision by another hand of the Gospel which Luke wrote.

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