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DR. DRIVER'S INTRODUCTION TO THE OLD TESTAMENT LITERATURE.

PART III.

I SAID in Part II. that Dr. Driver would have done well to make his *non liquet* refer, not to Davidic, but to pre-Exilic psalms. There are in fact, as it appears to me, two tenable (though not two equally tenable) views. According to one, we may still have some pre-Exilic psalms (including those which refer to a king, and some at least of the persecution-Psalms), a few Exilic (*e.g.* Pss. xxii., li., cii.), and also a considerable number of post-Exilic Psalms (including a few Maccabean Psalms, and at any rate Pss. xliv., lxxiv., lxxix).[1] This was the view which I adopted not as critical truth but as a working hypothesis, when preparing that commentary on the Psalms (1888) which has been so strangely overlooked by nearly all the reviewers of my *Bampton Lectures*. It is the very view now independently adopted by Dr. Driver, which indicates that in his more special study of the Psalms he has now reached the point which I had reached in 1888. At this I rejoice, for I am confident that the view which was only a working hypothesis to me in 1886 is no more than this to Dr. Driver in 1891. He cannot go backward —this were to deny facts; he can only go on to the second of the two views mentioned, viz. that the whole of the

[1] Some of those who have reviewed my *Bampton Lectures* have accused me of having treated the external evidence which has been thought to be adverse to the theory of Maccabean psalms and the objections drawn from the Septuagint Psalter too slightly. The view which these scholars take of the present position of Psalm criticism is however entirely different from my own and from that taken by competent scholars abroad (see Mühlmann, *Zur Frage der makk. Psalmen*, 1891, p. 3). Nor, so far as I can judge, is it that of Prof. Driver.

Psalter, in its present form, with the possible exception of Ps. xviii.; is post-Exilic. Just as Cornill thought in 1881 that the 24th and probably other Psalms were Davidic, and that Psalms lxxxiv., lxxxv., xlii., xliii., were of the reign of Jehoiakim, but by 1891 had come to see that the whole Psalter (except perhaps Psalm lxxxix.) was post-Exilic,[1] so it will probably be with Dr. Driver, however much he may modify his view by qualifications.[2] It is the latter theory of which I have myself for the first time offered a comprehensive justification. Caution and sobriety were as much needed for this as for any other critical task, nor would the want of ability to enter into the feelings of a psalmist (*nachempfinden*) and to realize his historical situation have been at all a helpful qualification. The result is doubtless capable of large improvement in detail, but in the fundamental points can hardly be modified.[3]

Does this latter theory differ essentially, or only in secondary points, from that of Dr. Driver? Only in secondary points. I made no leap in the dark when I

[1] Cf. his essay in Luthardt's *Zeitschrift*, 1881, pp. 337–343 with § 36 of his *Einleitung* (1891).

[2] I do not think that he will find that much is gained by insisting on an ancient basis which has been obscured by editors. If it helps any one to believe in such a basis, by all means let him do so; it is more harmless than in the case of the Book of Daniel. But the chief object of the criticism of the psalms is to determine the date when they became known in substantially their present form. It appears to me that in all probability the editors mainly concerned themselves with the omission of passages which had too temporary a reference. In two (presumably) Maccabean psalms—lxxiv. and cx.—there certainly seem to be some omissions; in Psalm lxxiv. there may also be a fresh insertion (*vv.* 12–17).

[3] It is difficult to reply as one would wish to a series of criticisms made from a different and perhaps a narrower point of view, especially when such criticisms deal largely with subordinate points which are not essential to the main theory. When the next English dissertation on the origin of the Psalter appears, it will at any rate be compelled to make considerable use of hypothesis, or it will be a failure. Prof. Davison (in the *Thinker*, Feb., 1892) does not seem to recognise this. To him and to Prof. Kennedy (two of the most courteous of my critics) I have given an imperfect reply in the *Thinker* for April; to Prof. Kennedy also in the *Expository Times* for the same month.

prepared my *Lectures*, nor will Dr. Driver be conscious of any abrupt transition, when he finds opportunity to advance further. The essential of both views is the recognition of the impossibility of proving that any psalm in its present form is pre-Exilic. "Of many Psalms," adds Dr. Driver, "the Exilic or post-Exilic date is manifest, and is not disputed; of others it is difficult to say whether they are pre- or post-Exilic" (p. 362). Whichever view be adopted, it must be allowed that even Books I. and II. were put forth *after the Return*. This is not expressly mentioned by Dr. Driver, and, as I have said, it seems to me a regrettable omission. But though not mentioned, it is not, nor can it be, denied. I venture to put this before those theological reviewers who, in their needless anxiety for the ark of God, have hurried to the conclusion that the author has "rejected Dr. Cheyne's sweeping criticism of the Psalms," and that the "net result" set forth by the author on pp. 362, 363 is "very different from that which Dr. Cheyne has given us,"[1] and to express the hope that they may perceive the error into which they have fallen, and begin to suspect that it is not the only one.

We are now come to Proverbs and Job, and nowhere perhaps does one feel more strongly the imperfection of Dr. Driver's plan. It is true, what was most desirable was not yet feasible—a thorough and *comprehensive* study of the contents and origin of the Wisdom-literature, which would furnish results at once surer and more definite than the old-fashioned Introductions can give. But I think that more might have been done than has been done to show the threads which connect the products of this style of writing, and to anticipate the results which a critic of insight and courage could not fail to reach. But alas! Dr. Driver has not thrown off that spirit of deference to conservatism which,

[1] See *Church Quarterly Review*, Jan., 1892, p. 343; *Guardian*, Dec. 2nd, 1891, p. 1953.

if I am not mistaken, injures his work elsewhere. At the very outset the tradition respecting Solomon in 1 Kings iv. 29–34 receives no critical examination, and though the headings in Proverbs x. 1, xxv. 1 [1] are not unconditionally accepted, Dr. Driver speaks notwithstanding as if some of the Proverbs in two of the greater collections might possibly be the work of Solomon. This is hardly the way to cultivate the critical spirit in young students, and (against the author's will) may foster an unjust prejudice against critics not less careful, but perhaps less compromising than the author. As to the conclusions here offered, I feel that while censure would be impertinent, praise would be misleading. The "present condition of investigation" is only indicated in a few lines of a footnote (p. 381), and the "way for future progress" is not even allusively mentioned. It appears to me that criticism ought to start not from the worthless tradition of Solomonic authorship, but from the fact that the other proverbial books in the Old Testament are with increasing certainty seen to be later than 538 B.C. Now what does Ben Sira tell us about his own work?

> "I too, as the last, bestowed zeal,
> And as one who gleaneth after the vintage;
> By the blessing of the Lord I was the foremost,
> And as a grape-gatherer did I fill my winepress."
> —(Ecclus. xxxiii. 16.)

Who were Ben Sira's predecessors, and when did they live? The writers of Proverbs xxx. and xxxi. 1–9 and 10–31, and of the gnomic sayings (or some of them) in Koheleth may be among them; but surely there were more productive writers or editors than these (so far as we know them from their writings). The force of the arguments against a post-Exilic date for the final arrangement of our composite Book of Proverbs seems to me to be constantly

[1] Note that Sept. does not give the former heading at all, and has no "also" in the latter.

increasing, and were I to resume the work laid aside in 1887, I feel that my results would be nearer to those of Reuss and Stade (adopted by Mr. Montefiore) than to those of Delitzsch.[1] I am not indeed prepared to give up a large antique basis [2] for chaps. xxv.–xxvii., the proverbs in which, as Prof. Davidson has pointed out, differ on the whole considerably in style from those in x. 1–xxii. 16. But not only chaps. xxx. and xxxi., but the passages forming the "Praise of Wisdom," and the introductory verses of the redactor (i. 1–6), are altogether post-Exilic (not of course contemporary), and so too, probably, is much of the rest of the book. Indeed however much allowance is made for the tenacity of the life of proverbs, and for the tendency to recast old gnomic material, one must maintain that in its present form the Book of Proverbs is a a source of information, not for the pre-Exilic, but for various parts of the post-Exilic period.[3] I will only add that Dr. Driver may perhaps modify his view of the gradual formation of Proverbs in deference to recent researches of Gustav Bickell.[4]

The chapter on Job is a skilful exhibition of views which are well deserving of careful study. It is evidently much influenced by a book of which I too have the highest appreciation—Prof. Davidson's volume on Job in the Cambridge series (comp. his article "Job" in the *Encycl. Brit.*). If

[1] In my article "Isaiah" (*Ency. Brit.*, 1889) I expressed the view that the "Praise of Wisdom" is either Exilic or post-Exilic; in my *Job and Solomon* (1887) I dated it earlier. But, as *Bampt. Lect.*, p. 365, shows, I have been coming back to my former view of Prov. i.–ix., and taking a survey of Proverbs from this fixed point, I see that the difficulties of Reuss's and Stade's view (when duly qualified) are less than those of my own former and of Dr. Driver's present theory. Comp. Mr. Montefiore's thorough and interesting article on Proverbs, *Jewish Quarterly Review*, 1890, pp. 430–453.

[2] The heading in xxv. 1 reminds one of Assyrian library notes. Isa. xxxviii. 9 may rest on a tradition of Hezekiah's interest in books.

[3] In this connection I may refer to my notes on the Persian affinities of the "Wisdom" of Prov. viii., EXPOSITOR, Jan., 1892, p. 79.

[4] See the *Wiener Zeitschr. f. d. Kunde des Morgenlandes*, 1891–1892 (chiefly important for the metrical study of Job, Proverbs, and Ecclesiasticus).

therefore I object to it, it can only be in the most friendly manner, and on the same grounds on which I have already criticised that beautiful textbook.[1] I must however add that I think Dr. Driver should have taken some steps in advance of a book published in 1884. Both he and Dr. Davidson have a way of stopping short in the most provoking manner. At the very outset, for instance, they compromise rather more than is strictly critical on the subject of the historical existence of Job.[2] It is true, we ought not, without strong grounds to presume that the plot of the poem is purely romantic, Semitic writers preferring to build on tradition as far as they can. But to use the words "*history*" and "*historical* tradition" of the main features of the Job story is misleading, unless we are also bold enough to apply these terms to the pathetic Indian story of Harischandra in vol. i. of Muir's *Sanskrit Texts*. No doubt there were current stories, native or borrowed, of the sudden ruin of a righteous man's fortunes; but if we had them, we should see that they were not historical, but simple folk-tales, which, to a student of natural psychologies, are surely better than what we call history. On this however I have said enough elsewhere;[3] so I will pass on to one of the great critical questions—that of the integrity of the Book.

Here Dr. Driver is not very satisfactory. It is true, he thinks it "all but certain" (why this hesitation?) that the Elihu-speeches are a later insertion, which, considering his conservatism on Isaiah xl.–lxvi., is a concession of much value. But he unfortunately ignores even the mildest of

[1] *Academy*, Nov. 1, 1884.

[2] Among minor matters connected with the Prologue, these may be noted. I see no explanation of the name of Job, and for the meaning of the "land of Uz" miss a reference to W. R. Smith, *Kinship in Arabia*, p. 261. A hint might also have been given of the appearance of a legend of "three *kings*" from the East (Job ii. 11, Sept.).

[3] *Job and Solomon*, pp. 62, 290.

those critical theories, of which a wiser critic (in my opinion) speaks thus in an American review[1]:—

"If we are not mistaken, a much better case could be made out for a theory of many authors than for the theory of one [or of two]. As the name of David attracted successive collections of Psalms, and the name of Solomon successive collections of Proverbs, why may not the name of Job have attracted various treatments of the problems of suffering righteousness?"

Why not, indeed, if the evidence points, as it does, in this direction? And my complaint is not that Dr. Driver does not adopt this or that particular theory, but that he fails to recognise a number of exegetical facts. He approaches the Book of Job, as it seems to me, with the preconceived idea that it left the author's hand as a finished and well-rounded composition. This idea is no doubt natural enough, but is hardly consistent with the results of criticism in other parts of the Old Testament and in other literatures. As has been well said by the authors of the *Corpus Poeticum Boreale*, "The great books of old time are accretions; our Psalter is such a one, Homer is such a one, the Sagas are such a one." Ewald, who began by believing in the unity of Genesis, found out that this unity was factitious; may it not very naturally be so with a poem, which, like the dialogues in Job, prompted to imitation and to contradiction? Dr. Driver's able forerunner has indeed justified his own reluctance to disintegrate by his desire to enjoy the poem as much as he can. He can sympathize, he tells us, with those persons who are "so intoxicated with the beauty of a great creation, that they do not care a whit how it arose."[2] But he forgets that the true writer is not a mere dissector, but analyzes in order to reconstruct. Nor can it be said that the Book of Job as it stands is a great work of art. I know all that can be said on the difference between Eastern and Western art,

[1] Review of Genung's *Epic of the Inner Life* in *The Nation*, Aug. 27th, 1891.
[2] Davidson, EXPOSITOR, 1883, p. 88.

and between Eastern and Western psychology; but the difference must not be pressed to an extreme. I am willing to admit—indeed, I did in 1887 expressly admit—that the six accretions indicated in my *Job and Solomon* (pp. 67–69), need not have come from as many different writers. The Elihu-speeches, however, which are the most obvious of the accretions, cannot have come from the writer of the Dialogues (though Kamphausen once thought so). Nor, as it would seem, can the Epilogue. I grant that the author of the Dialogues prefixed to his work not only chap. iii., but also chaps. i. and ii. But I cannot believe that he meant xlii. 7–17 to be the *dénoûment* of the story;—that hypothesis at least no ingenuity can render plausible. "The only possible close of the poem, if the writer is not untrue to his deepest convictions, is that the Satan should confess before Jehovah and the court of heaven that there are 'perfect and upright' men who serve God without interested motives."[1] Such at least is still my own opinion. That we do not now find such a close, only proves either (what we knew before) that the original poem has not come down to us intact, or that the Book of Job, like that of Koheleth, was left in an unfinished state by the author.

Whether the other passages were, or were not, added by the author is to some extent an open question. It seems to me extremely hazardous to suppose that the writer went on retouching his own work, but this is the only possible course for those who hold out against the view, which for some at least of the added passages I cannot help advocating. But at any rate one thing is certain, viz. that even after removing the speeches of Elihu, the Book of Job does not form a genuine whole—that some of the original passages have been retouched and new ones added. That eminent critic Dillmann, who in spite of himself continually

[1] *Critical Review*, May, 1891, p. 253 (the present writer's review of Hoffmann's *Hiob*).

makes such gratifying concessions to younger scholars, is in the main point on my side,[1] and so are all the chief workers in this department. Against me, as I have good cause to know, there stands arrayed the host of English theological reviewers. But how many of these have made a serious critical study of the Book of Job? How many have even read carefully—much less worked at—any critical work in which the unity of Job is denied, and have assimilated the *positive* side of a disintegrating theory? I complain of my friend Dr. Driver because, with the best intentions, he has made it *more difficult* for ordinary students to come to the knowledge of important facts, and made it possible for a thoroughly representative, and in some respects not illiberal, writer in a leading Anglican review to use language which must, I fear, be qualified as both unseemly and misleading.[2]

And what has the author to say on the date of the poem, or rather since the poem has, by his own admission, been added to, on the date of the original work and of the Elihu-speeches? To answer that the latter were added by "a somewhat later writer" is, I think, only defensible if the original poem be made post-Exilic. For surely, if anything has grown clearer of late years, it is that the language and ideas of "Elihu" are those of some part of the post-Exilic period.

The new edition of Dillmann's *Hiob* may be taken as evidence of this. He still makes the original poem pre-Exilic (though nearer to B.C. 586 than formerly), but whereas in 1869 he thought that the Elihu-speeches "might have been written in the course of the sixth century" (*i.e.* possibly before the Return), in 1891 he tells us that they are probably to be assigned to the fifth century. As to the

[1] See Dillmann, *Hiob* (1891), *Einl.*, p. xxviii., and cf. his remarks on th controverted passages in the course of the book.

[2] *Guardian*, Dec. 2, 1891.

original poem, our author states (as I did myself in 1887) that—

"It will scarcely be earlier than the age of Jeremiah, and belongs most probably to the period of the Babylonian captivity." [1]

Both Dillmann and Dr. Briggs favour the former date; Umbreit, Knobel, Grätz, and Prof. Davidson the latter. Gesenius also prefers an Exilic date, but will not deny the possibility of a still later one. And it is a post-Exilic date which many critics (*e.g.* Kuenen, Wellhausen, Stade, Hoffmann,[2] Cornill) are in our day inclined to accept. Ought not this to have been mentioned? I feel myself that in the present position of the criticism of the Hagiographa a post-Exilic date has acquired a greater degree of plausibility.[3] If, for instance, the Book of Proverbs is in the main a composite post-Exilic work, it becomes at once in a higher degree probable that the Book of Job is so too.

[1] Prof. Bissell, I observe, hopes to prove a considerably earlier date *by the help of Glaser's discoveries in Arabia* (*Presbyterian and Reformed Review*, Oct., 1891). He refers to Prof. Sayce. I trust that Prof. Whitehouse will be more cautious (see *Critical Review*, Jan., 1892, p. 12).

[2] Prof. G. Hoffmann's arguments (*Hiob*, 1891) do not perhaps materially advance the discussion, though his book ought to have been referred to by our author. His linguistic proposals are too violent, and his references to Zoroastrianism do not show enough study. Nor am I sure that he has added much of value to the argument from parallel passages. On the latter I venture to add these remarks for comparison with Dr. Driver's valuable section (p. 408). On the parallels between Job and the probably or certainly Exilic parts of ii. Isaiah it is difficult to speak confidently. Nor need we perhaps consider the Prologue of Job to be indebted to Zech. iii.; the modes of representation used were "in the air" in the post-Exilic period. And as to the parallel adduced by Cornill (*Einl.*, p. 234) between Job xlii. 17 and Gen. xxxv. 29, xxv. 8 (both P), this, if admitted as important, will only affect the date of the Epilogue. Then we turn to the Psalms, the Song of Hezekiah, and the Lamentations. It would be difficult indeed to say that Isa. xxxviii. 10–20, or that Ps. xxxix. and lxxxviii. were not written in the same period as Job, and these works can, I believe, be shown to be post-Exilic. If this seems doubtful to any one, yet Ps. viii. 5 "is no doubt parodied in Job vii. 17" (Driver), and there is no reason for not grouping Ps. viii. with the Priestly Code. I admit that Lam. iii. is, by the same right as Ps. lxxxviii., to be viewed as in a large sense contemporary with Job (see Delitzsch, *Hiob*, p. 24). But what is the date of the Lamentations? See farther on.

[3] Comp. *Bampt. Lect.*, p. 202.

It is still of course a question to be argued out in detail; there is no escaping from the discipline of hard and minute investigation. But, so far as I can see, the evidence collected, when viewed in the light of general probabilities, and of the results attained and being attained elsewhere, justifies us in asserting that the whole of the Book of Job belongs most probably to the Persian period. On linguistic grounds[1] I should like to put the main part of the Book in the first half of this period, and the Elihu-speeches in the second, but these grounds are not by themselves decisive.

A word must here be said on a subject which will be in the mind of many readers. These critical results must have some bearing on theories of inspiration. But what bearing? I have an uneasy feeling that the remark on page 405—that "precisely the same inspiration attaches to [the Elihu-speeches] which attaches to the poem generally"—is hardly penetrating enough, and that by such a half-truth Dr. Driver has unwisely blunted the edge of his critical decision. Of course, the Elihu-speeches *are* inspired; they are touched by the same religious influences which pervades all the genuine Church records of the Exilic or post-Exilic period which are contained in the Hagiographa. But it can hardly be said that these speeches have the same *degree* of inspiration as the rest of the Book of Job, at least if the general impression of discriminating readers may be trusted. The creator of "Elihu" may have some deeper ideas, but he has not as capacious a vessel to receive them as the older poet.[2] And though it may be true that he had a good motive, and that the course which he took was sanctioned by the religious authorities

[1] These grounds are briefly indicated by Dr. Driver on p. 404 (sect. 8) and p. 406 (top); cf. my *Job and Solomon*, pp. 291–295. Besides Budde's *Beiträge*, Stickel (*Hiob*, 1842, pp. 248–262) still deserves to be consulted on the Elihu-portion.

[2] See *Job and Solomon*, pp. 42–44.

of the day, yet it is certain both that he has defects from which the earlier writer is free, and that he has for modern readers greatly hindered the beneficial effect of the rest of the poem. We must not, in short, force ourselves to reverence these two poets in an equal degree.

I admit that the difficulties which theories of inspiration have to encounter in the Song of Songs, Ecclesiastes, and Esther are still greater, and I think that Dr. Driver would have facilitated the reception of his critical results on these books if he had at once taken up a strong position with reference to those difficulties. It might even have been enough to quote a luminous passage from a lecture by Prof. Robertson Smith,[1] the upshot of which is that these three books "which were still disputed among the orthodox Jews in the apostolic age, and to which the New Testament never makes reference,"[2] and, let me add, which do not seem to be touched by the special religious influences referred to above, are not for us Christians in the truest sense of the word canonical.[3] These books however are intensely interesting, and a "frank and reverent study of the texts" shows that they "have their use and value even for us," and my only regret is that in Esther and Ecclesiastes, at any rate, Dr. Driver is slightly more "moderate" than was necessary, and that he does not make it quite as easy as it might have been for some of his readers to agree with him.

I pass to a book in which I have long had so special an interest that it will require an effort to be brief—the glorious Song of Songs. Our author rejects the old alle-

[1] *The Old Testament in the Jewish Church*, pp. 174, 175; cf. Wildeboer, *Die Entstehung des alttest. Kanons* (1891), pp. 150, 152.

[2] See however Trench, *Seven Churches of Asia*, pp. 225, 226.

[3] Of the Song of Songs, Lowth, writing to Warburton in 1756, says: "If you deny that it is an allegory, you must exclude it from the Canon of Holy Scripture; for it holds its place there by no other tenure" (Warburton's *Works*, by Hurd, xii. 458).

gorical interpretation as artificial and extravagant (p. 423), but does not regard Delitzsch's modification of it as untenable, provided it be admitted that there is nothing in the poem itself to suggest it. His meaning, I presume, is this —that the Song is only allegorical in so far as all true marriage to a religious mind is allegorical,[1] but that we cannot suppose the poet to have thought of this allegory when he wrote, and that, his own meaning being so beautiful, it is almost a pity to look beyond it. Dr. Driver's treatment of the Song is marked by much reserve. He does indeed commit himself to the lyrical drama theory, without considering whether the poet may not to some extent have worked up current popular songs (just as Poliziano did in Medicæan Florence); and though he puts two forms of this theory (Delitzsch's and Ewald's) very thoroughly before the reader, he evidently prefers the latter, with some modifications from Oettli. Still one feels after all that he has not given us a thorough explanation of the Song. This was perhaps justifiable in the present state of exegesis. For though the poem has not been altogether neglected by recent scholars, with the exception of Grätz and Stickel none of them has seriously grappled afresh with the problem of its origin. To Grätz (in spite of his many faults as a scholar) and Stickel the student should have been expressly referred;[2] the mention of the former on p. 423 seems to me far from sufficient. Help may also be got from Prof. Robertson Smith's able article in the *Encyclopædia Britannica* (1876), and by the section relative to the Song in Reuss' French edition of the Bible.

For determining the date of the Song the linguistic argument is of more than common importance. Here I must complain that such a thorough Hebraist as Dr. Driver

[1] Cf. Julia Wedgewood, *The Moral Ideal* (1888), pp. 269, 270.

[2] Stickel's book appeared in 1888, and was ably reviewed by Prof. Budde (*Theol. Lit.-ztg.*, 1888, no. 6).

hesitates so much. The only fresh ground for uncertainty is the discovery of a weight on the site of Samaria, ascribed to the eighth century, with שׁל as in Song i. 6 (viii. 12), iii. 7. Apart from this, a linguist would certainly say that this pleonastic periphrasis *proved* the late date of the poem as it stands, but now it seems permissible to Dr. Driver to doubt. That I reluctantly call an unwise compromising with tradition. In 1876 (the date of Prof. Robertson Smith's article) we did not see our way in the post-Exilic period as we do now. If there is anything in the contents of the Song which express a pre-Exilic date, let it be pointed out. Meantime all the facts as yet elicited by exegesis can be explained quite as well on the assumption of a late date as of an early one. Let us then (failing any fresh exegetical evidence) hear no more of the Song of Deborah and the early north-Israelitish dialect. It is certain that the use of ש for אשׁר is specially characteristic of late writings; certain, that שַׁלְמָה Song i. 7 is analogous to שֶׁלְמִי Jon. i. 7, and also to בְּשֶׁל אֲשֶׁר Eccles. viii. 17, and אֲשֶׁר לְמָה Dan. i. 10 (the fuller relative used as in Jon. i. 8[1] [contrast ver. 7], in a carefully expressed speech); certain, too, that some at least of the loan-words mentioned on pp. 422, 423 (note [3]) point definitely to the post-Exilic period (even one or two Greek words seem highly probable). Kuenen in 1865, in spite of his preconceived theory of an early date, admitted that "the language seemed, at first sight, to plead for the Persian period"; Gesenius and M. Sachs—a great Christian and a great Jewish Hebraist—have expressed themselves still more strongly on the "modern Hebrew" of the Song of Songs. It is also highly probable that a careful study of the names of plants in the Song would favour a post-Exilic date. Nor can the parallelisms between this book and that "song of loves (or, love)," the

[1] I do not take the fuller phrase in ver. 8 to be a gloss (cf. the four lines added by Dr. Driver on p. 301 in 2nd edition).

45th Psalm, be ignored. If that psalm is post-Exilic, so also presumably is the Song of Songs.[1] But Dr. Driver's researches on the Psalms have not yet perhaps led him to see what to me is now so clear, and I am therefore content to have shown that, quite apart from this, the facts admitted by Dr. Driver point rather to a late than to an early date, and that we cannot therefore safely assume, with our author, that the poem has a basis of fact. Readers of Delitzsch's delightful essay on "Dancing, and Pentateuch-Criticism"[2] do not need to be assured that the post-Exilic period was not without the enlivenment of secular dancing and song.

And now comes another little disappointment—another little compromise with conservatism, which I should prefer to glide gently over, but for the illusion which is growing up among us that paring down the results of criticism is necessary for a truly Christian teaching. The Book of Ruth, according to our author, is a prose idyll, similar, I presume, to that which may have lain in the mind of the author of that idyllic group of quasi-dramatic *tableaux*—the Song of Songs, and based, like the Song (according to Dr. Driver), on tradition. We are told that,—

"The basis of the narrative consists, it may reasonably be supposed, of the family traditions respecting Ruth and her marriage with Boaz. These have been cast into a literary form by the [pre-Exilic] author, who has, no doubt, to a certain extent idealized both the characters and the scenes. Distance seems to have mellowed the rude, unsettled age of the Judges" (pp. 427, 428).

This description seems to soften the facts a little too much. It is not merely a "mellowed" picture that we

[1] See *Bampton Lectures*, pp. 167, 179 (cf. p. 298). On p. 167 (foot), read "*can be better accounted for.*" I do not see where to find a situation for either of these poems before the Greek period. One of the early and fortunate reigns must of course be selected. But I hold myself open to correction.

[2] Delitzsch, *Iris* (E. T.), pp. 189-204). The Mishna (*Taanith*, iv. 8; see Wünsche, *Talm.*, i. 473) tells how Song iii. 11 was sung in the vineyard dances.

have before us, but, as Mr. Cobb has remarked,[1] complete "*contrariety* of spirit, style, social life, and public affairs." Nor is anything gained by postulating an uncertain amount of traditional material; the story of Ruth is practically as imaginative as that of Tobit, and is none the less edifying on this account. But let us see how the acute and learned author endeavours to prove a pre-Exilic date. The genealogy, as he admits, "appears to suggest an Exilic or post-Exilic date," but this "forms no integral part of the book," while, in spite of many isolated expressions[2] which, taken together, seem at first sight to point to the post-Exilic period, the "general beauty and purity of the style of Ruth point decidedly to the pre-Exilic period." We are not told whether the book was written before or after Deuteronomy (which is referred on p. 82 to the reign of Manasseh), but it is pointed out that the peculiar kind of marriage referred to in chapters iii. and iv. is not strictly that of levirate (Deut. xxv. 5), and that the reception of Ruth into an Israelitish family "appears to conflict with Deuteronomy xxiii. 2." In reply, it may be said (1) that in order to give the "present condition of investigation" it was important to give a much fuller statement of the grounds on which "most modern critics consider Ruth to be Exilic (Ewald) or post-Exilic (Bertheau, Wellhausen, Kuenen, etc.)"; (2) that by Dr. Driver's very candid admission "the style of the prose-parts of Job ['most probably' Exilic, p. 405] is not less pure"; (3) that the religious liberality of the writer and the family relations which he describes in the Book are perfectly intelligible in the post-

[1] *Bibliotheca Sacra*, Oct., 1891, p. 662.

[2] לָהֶן, שָׁבַר, קָיַם are, I think, decisive. I incline to add שַׁדַּי, which before the Exile is poetical (see *Bampton Lectures*, p. 84). Dr. Driver regards Ruth iv. 7 (קים) as a gloss, cf. 1 Sam. ix. 9. But the latter passage is embedded in a *pre-Exilic* section, whereas Ruth iv. 7 occurs *ex hyp.* in a post-Exilic narrative. The narrator tries to throw himself back into early times, but *has to explain* a custom unknown to his post-Exilic readers. Nor is there any special reason to regard לָהֶן as a word of the early northern dialect (p. 427).

Exilic period (cf. on the one hand the Book of Jonah, and on the other Kuenen's remark on Leviticus xviii. and xx., *Hexateuch*, p. 268); and (4) there is clearly no *necessity* to suppose the genealogy to have been added in a later age. In fact the one excuse for giving this Book an earlier date than that of Jonah is the greater flavour of antiquity which it possesses (notice the points of contact with Samuel given by Bertheau in the *Kurzgef. Handbuch*, p. 286).[1] Its real design is, not to glorify the Davidic house, but to show the universality of God's love. Just as our Lord exhibits a Samaritan as the model of practical piety, so the unknown writer of this beautiful little book brings before us a Moabitish woman as the model of an affectionate daughter who receives the highest earthly reward.[2]

The five Lamentations deserve attention, not only for some classic beauties of expression which have endeared them to the Christian heart, but as (perhaps) the earliest monuments of the piety of regenerate Israel, and as (perhaps) supplying presumptive evidence of the cultivation of religious lyric poetry long before the Exile. Nowhere perhaps does Dr. Driver's individuality show itself more strikingly than here. What pains he takes to soften the prejudices of old-fashioned readers, and give the principal result of criticism in its most moderate form! To unprejudiced students, however, he may seem timid, and it is certainly strange to hear that "even though the poems be not the work of Jeremiah, there is no question that they are the work of a contemporary (or contemporaries)." Nägelsbach long ago saw that at any rate Lamentations ii. implies an acquaintance with the Book of Ezekiel, and, to Dr. Driver, the affinities between all the Lamentations and the prophecies of Jeremiah ought surely to suggest that the author (or

[1] See Dr. Driver, p. 302, and cf. *Bampton Lectures*, p. 306.

[2] Comp. Talm. Bab., *Sanhedrin*, 96 *b* (Wünsche, iii. 188), where still bolder flights are taken.

authors) had made a literary study of that Book. A considerable interval must therefore have elapsed between B.C. 586 and the writing of the Lamentations,[1] and the language used in Lamentations v. 20 (comp. Isa. xlii. 14, lvii. 11) points rather to the *end* than to the beginning of the Exile. This period is, moreover, the earliest which will suit the parallelisms between Lamentations iii. and the Book of Job (referred in this work to the Exile), which are more easily explained on the supposition that the elegy is dependent on Job than on the opposite theory.[2] It ought however to be mentioned that there are plausible grounds for giving a still later date to the third elegy, in which Jerusalem is not once mentioned, and which it is difficult not to associate with the Jeremianic psalms. If Psalm xxxi. is post-Exilic (and any other theory seems to me extremely improbable), so also is Lamentations iii., and of course we must add, If the poem of Job (as a whole) is post-Exilic, so also is Lamentations iii. And though I do not for a moment deny that lamentations were indited during the Exile (the Books of Ezekiel and of ii. Isaiah sufficiently prove this), yet the mere fact that the authors of Lamentations i., ii., iv., and v. refer so prominently to the fall of Jerusalem, is no conclusive proof that these lamentations too were not written in Judah after the return. The dramatic imaginativeness of the psalmists has, I believe, been proved,[3] and the peculiar rhythm called "elegiac" has been traced by Budde in many productions of the post-Exilic age. It seems to me far from impossible that, just as the Church of the Second Temple composed its own psalms, so it preferred to indite fresh elegies for use on the old fast-days.

[1] See Prof. W. R. Smith's excellent article in *Encyclopædia Britannica*.

[2] See my *Lamentations* (Pulpit Comm.), Introd., p. iii.

[3] Cf. my commentary on Pss. lxxiv. and cxxxvii. The Second Isaiah, too, describes imaginatively in "elegiac rhythm" the state of captured Jerusalem Isa. li. 17–20).

The next section is one of the very best in this part of the volume—it is on Ecclesiastes. I will not occupy space with summarizing it, but urge the student to master its contents. I quite agree with Dr. Driver that the work may possibly be a work of the Greek period. The language, as I remarked in 1887, favours (though it does not absolutely require) a later date than that suggested by Ewald (close of the Persian period). The objection that if the book be of the Greek period, we have a right to expect definite traces of Greek influence, I now see to be inconclusive; the Wisdom of Jesus the Son of Sirach contains none, and yet belongs to the Greek period.[1] Moreover, Hellenism must have influenced very many who did not definitely adopt Greek theories. Certainly the work is very un-Jewish. Very probably Kuenen is correct in dating it about 200 B.C., *i.e.*, about forty years before the great Maccabæan rising (so too Mr. Tyler). Dr. Driver admits the force of his reasoning, though he still not unreasonably hesitates. He is himself strongest on the linguistic side of the argument; see especially his note on the bearings of Prof. Margoliouth's attempted restorations of Ben Sira (p. 447). I cannot equally follow him in his argument against a theory which I myself hold, viz. that the text of Ecclesiastes has been manipulated in the interests of orthodoxy. As was remarked above, the book is not in the strictest sense canonical, and we have therefore no interest in creating or magnifying difficulties in a theory which is intrinsically probable, and is supported by numerous phenomena in the later period.

The section on Esther is also in the main very satisfactory. But why are we told that this narrative (which was not canonical according to St. Athanasius, and which, fascinating as it is, we can hardly venture to call inspired)

[1] On supposed Greek influences, see, besides Menzel, *Qohelet und die nacharistotelische Philosophie*, von August Palm (1885).

cannot reasonably be doubted to have a historical basis? Is it because of the appeal to Persian chronicles (Esth. ii. 23; x. 2; cf. ix. 32)? But it is of the essence of the art of romance not to shrink from appeals to fictitious authorities. One may however admit that a story like Esther, which professed to account for the origin of a popular festival, probably had a traditional, though not a historical, basis. On this point reference may be made to Kuenen's *Onderzoek* (ed. 2), p. 551, and Zimmern in Stade's *Zeitschrift*, 1891, p. 168. The latter thinks (and both Jensen and Lagarde agree) that the Feast of Purim may be derived ultimately from a Babylonian New Year's Feast, and that the story of the struggle between *Mordecai* and Haman was suggested by a Babylonian New Year's legend of the struggle between *Marduk* and Tiâmat. This coincides curiously with the views proposed above to explain the origin of the Jonah-narrative. Of course, the story may have been enriched with Persian elements (on which see Lagarde and Kuenen[1]) before it was Hebraized by a Jewish story-teller.

Dr. Driver's lingustic argument for placing Esther in the 4th or 3rd century B.C. is excellent. But there is one important omission in his brief discussion. If the date is so early, how is it that the earliest independent evidence for the observance of Purim in Judæa is in 2 Maccabees (see p. 452)? Moreover, there is no mention of Mordecai and Esther[2] in Ben Sira's "praise of famous men" (Eccles. xliv.–xlix), which would be strange if Purim and its story were well known in Judæa in B.C. 180. May not the festival have been introduced into Judæa, and the Book of Esther have been written some time after the Maccabæan

[1] Lagarde's treatise *Purim* (1887) is important; Dr. Driver's reference gives no idea of this. See also his *Mittheilungen*, ii. 378–381, iv. 347. On Persian legendary elements, see also Kuenen, *Ond.*, ed. 2, ii. 551, and cf. Cornill, *Einl.*, p. 253.

[2] Cf. Ben Sira's silence as to Daniel (see *Job and Solomon*, p. 194).

War (so Reuss, Kuenen, and Cornill)? Or, though this seems less probable, the book may have been written by a Persian Jew in the third century, but not brought to Palestine till later. Dr. Driver ought perhaps to have mentioned this theory (Mr. Bevan, *Daniel*, p. 29, notes two significant words which Esther has in common with Daniel). He might also have added to his "literature" my article "Esther" in *Enc. Brit.* (1878); Cassel's *Esther* (1888); and Dieulafoy, "Le livre d'Esther et le palais d'Assuérus" in *Revue des études juives*, 1888 (Actes et Conférences).

Nor can I help giving hearty praise to the sections on Chronicles, Ezra, and Nehemiah. The details, especially on style, are worked out with great care. The only objection that I shall raise relates to the sketch of the method and spirit of the Chronicler, which I could have wished not less reverent, but bolder and more distinct in expression. We are all familiar with the attacks to which writers like Dr. Driver are exposed; some of the most vigorous passages of Bishop Ellicott's recent charge are directed against that strangest of all theories—"an inspiration of repainting history"—to which these reverent-minded writers are supposed to have committed themselves. If Dr. Driver had only been a little clearer on the subjects of inspiration and of the growth of the Canon, how much simpler would have been his task, especially in dealing with the Hagiographa! Of course, the Chronicles are inspired, not as the prophecies of Isaiah and Jeremiah, but as even a sermon might be called inspired, *i.e.* touched in a high degree with the best spiritual influences of the time. Dr. Driver says (Preface, p. xvi.):—

> "It was the function of inspiration to guide the individual [historian] in the choice and disposition of his material, and in his use of it for the inculcation of special lessons."

But clearly this can be true of the Chronicler only with

those limitations, subject to which the same thing could be said of any conscientious and humble-minded preacher of the Christian Church. And if these limitations cannot be borne in mind, it is better to drop the word altogether, and express what we mean by some other term. That there are some passages in Chronicles which have a specially inspiring quality, and may *therefore* be called inspired, is not of course to be denied. But upon the whole, as Prof. Robertson Smith truly says,[1] the Chronicler "is not so much a historian as a Levitical preacher on the old history." The spirit of the Deuteronomistic editor of the earlier narrative books has found in him its most consistent representative. He omits some facts and colours others in perfect good faith according to a preconceived religious theory, to edify himself and his readers. He also adds some new facts, not on his own authority, but on that of earlier records, but we dare not say that he had any greater skill than his neighbours in sifting the contents of these records, if indeed he had any desire to do so. Dr. Driver's language (p. 501) respecting the "traditional element" used by the Chronicler seems therefore somewhat liable to misunderstanding.[2]

The only remaining section of the book relates to the Book of Daniel, and upon this, as might be expected, Dr. Driver's individuality has left a strong impress. It is needless to say that the student can fully trust the facts which are here stored up in abundance, also that the conclusions arrived at are in the main judicious, and the mode of their presentation considerate. And yet helpful, very helpful, as this section is, it does not fully satisfy a severely critical standard. Far be it from me to blame the author for this; I sympathize too deeply with the conflict of feel-

[1] *The Old Test. in the Jewish Church*, p. 420.

[2] To the "literature" of Ezra I should add Nestle, "Zur Frage nach der ursprünglichen Einheit der Bücher Chronik, Esra, Neh.," in *Studien u. Kritiken*, 1879, pp. 517–520; van Hoonacker, "Néhémie et Esdras; nouvelle hypothèse," in *Le Muséon*, 1890.

ings amid which he must have written. I would speak frankly, but (on the grounds already mentioned) without assumption of superiority. First of all, I think it a misfortune that the sketch of the contents of the Book could not have been shortened. I know the excuse; there existed in English no commentary on Daniel sufficiently critical to be referred to. But on the other hand, there was the most urgent need for more preliminary matter, especially on the characteristics of this Book. Ordinary readers simply *cannot* understand Daniel. Modern culture supplies no key to it, as Mr. Gilbert's interesting paper in the EXPOSITOR for June, 1889, conclusively shows. I do not undervalue the judicious remarks on pp. 480–482, but on "apocalyptic" literature something more was wanted than bare references to various German authors, one of whom (Smend) ought, as I think, to have been made much more prominent.[1] Secondly, I think that a freer use should have been made of the cuneiform inscriptions, especially considering the unfriendly criticisms of Professor Sayce. In this respect I believe myself to have long ago set a good example, though my article on Daniel (*Enc. Brit.*, 1876) of course requires much modification and expansion.[2] And here let me repair an omission in Part I. of this review. Dr. Driver should, I think, in dealing with Hexateuch criticism, have taken some account of Assyrian and Egyptian investigations. Even if he thought it safer not to speak too positively on the bearings of these researches on the question of the dates of documents, he ought, I think, to have "indicated the way for future progress" (editor's preface).[3] But

[1] Dr. Wright's work on Daniel in the *Pulpit Commentary* will, I am sure, be full of learned and honest discussion. But when will it appear? Mr. Bevan's *Short Commentary on Daniel* (1892) is so good that we may even ask him for something more complete, though not more careful and critical.

[2] See also *Bampt. Lect.*, pp. 105–107 (cf. 94, 296).

[3] I referred to this at the Church Congress in 1883 (*Job and Solomon*, p. 6), and Prof. Robertson Smith wrote an acute paper on "Archæology and the Date of the Pentateuch" in the *Contemp. Rev.* for October, 1887. Against the

on the relation of cuneiform research to the criticism of Daniel no reserve was called for. It would have been quite right to say that the statement respecting Belteshazzar in Daniel iv. was erroneous, and that the names Ashpenaz, Shadrach, and Meshach could not have been put forward as Babylonian in Exilic times;[1] also that Hamelsar (probably) and Abed-nego (certainly) are ignorant deformations of Babylonian names, and that though Arioch is doubtless Eri-aku, yet this name was probably obtained from Genesis xiv. 1.[2] And much more might, I think, have been made of the writer's slight acquaintance with Babylonian ideas and customs. Above all, while on "the Chaldæans" and on Belshazzar very just remarks are made, on "Darius the Mede" we get this unfortunate compromise between criticism and conservatism (p. 469; cf. p. 479, note [2]):—

"Still the circumstances are not perhaps such as to be altogether inconsistent with either the existence or the office of "Darius the Mede"; and a cautious criticism will not build too much on the silence of the inscriptions, when many certainly remain yet to be brought to light."

Now it is quite true that in the addenda to the second edition it is stated, in accordance with the contract-tablets published by Strassmaier, that neither "Darius the Mede" nor even Belshazzar bore the title of king between Nabû-na'id and Cyrus. But it is not the very venial error in

coloured statements of Prof. Sayce's interesting paper in the *Expository Times* for December, 1881, I have already protested (p. 93). The Tell-el-Amarna tablets introduce a fresh element, not of simplicity, but of complication ("development" is, alas! not such a simple matter as theorists used to suppose). But E. Meyer's critical inference from Egyptian history in Stade's *Zt.*, 1888, pp. 47–49 (cf. his *Gesch. des Alt.*, I. 202) appears to be worth a corner even of Dr. Driver's limited space.

[1] Few probably will accept Kohler's suggestions on "the Chaldean names of Daniel and his three friends," in the *Zt. für Assyriologie*, 1889, pp. 46–51.

[2] The reported "discovery of transcendent importance" relative to Gen. xiv. 18, sinks upon examination into an interesting and valuable fact about Jerusalem, which is of no direct importance for Genesis-criticism. See my *Bamp. Lect.*, p. 45, and cf. Zimmern, *Zt. f. Assyriologie*, Sept. 1891, p. 263. *Let popular apologetic writers be more on their guard!*

the original statement on which I lay stress, but the attitude of the writer. Out of excessive sympathy with old-fashioned readers, he seems to forget the claims of criticism. The words of Daniel v. 31 should be in themselves sufficient to prove the narrative in which they occur to have been written long after B.C. 536.[1]

Thirdly, against the view that chap. xi. contains true predictions, the author should, I think, have urged Nestle's *certain* explanation of the so-called "abomination of desolation" in Stade's *Zeitschrift* for 1883[2] (see *Bampt. Lect.*, p. 105). That an Exilic prophet should have used the phrase explained by Nestle, Bishop Ellicott himself will admit to be inconceivable. I will not blame Dr. Driver for his remark on p. 477 (line 28, etc.), but I believe that it is not quite critical, and that Nestle's discovery supplies the last fact that was wanted to *prove* to the general satisfaction that Daniel xi., xii. (and all that belongs to it) was written in the reign of Antiochus Epiphanes. I say "the last fact," because a faithful historical explanation of Daniel xi., xii. such as is given by the great Church-Father Hippolytus in the lately discovered fourth book of his commentary[3] *forces* on the unprejudiced mind the conclusion that this section was written during the Syrian

[1] That Mr. Pinches should have come forward on the side of conservatism at the Church Congress in 1891, is, I presume, of no significance. He is far too modest to claim to have studied the Book of Daniel critically. The same remark probably applies to Mr. Flinders Petrie (see *Bampt. Lect.*, pp. 9, 10). On "Darius the Mede," compare Meinhold (*Beiträge*, 1888), and Sayce, *Fresh Light*, etc. (1884), p. 181, who however unduly blunts the edge of his critical decision. See also my own article "Daniel," for an incidental evidence of the confusion between Cyrus and Darius Hystaspis from 1 Kings x. 18, Sept.

[2] Dr. Driver mentions this explanation in the addenda to ed. 2. But, like Mr. Bevan (*Daniel*, p. 193, who also refers to Nestle), he thinks the "abomination" was an altar. Surely, as Bleek saw, it was (primarily at least) a statue. The statue of Olympian Zeus bore the Divine name, and the altar was presumably erected before it.

[3] Fragments of the Syriac version of this fourth book were given by Lagarde, *Analecta Syriaca* (1838), pp. 79-91. Georgiades discovered, and Dr. E. Bratke edited the complete work in Greek in 1891.

persecution. Hippolytus, it is true, did not draw this conclusion, but who can wonder that the Neoplatonic philosopher Porphyry did? And should we not be ready to learn even from our foes?

Fourthly. (The reader will pardon this dry arrangement under heads with a view to brevity.) I notice on p. 479 the same confusion which occurs elsewhere between "tradition" and history. I do not think that any critic who agrees on the main point with Dr. Driver would maintain that "Daniel, it cannot be doubted, was a historical person" except the newly converted Delitzsch, who, as his article in the second edition of Herzog's *Encyclopædia* shows, had not worked his way to perfect clearness. Listen to the late Prof. Riehm, who is now just obtaining recognition among us.

"The material of his narratives the author may partly have taken from folk-tales (*aus der Volkssage*), though at any rate in part he invented it himself. . . . And even if there was a folk-tale (*Volkssage*), according to which Daniel was a prophet living during the Exile and distinguished for his piety, yet the historical existence of an Exilic prophet Daniel is more than doubtful."[1]

One must, I fear, add that the two statements mentioned in note[2] as resting possibly or probably on a basis of fact are, the one very doubtful, the other now admitted to be without foundation.

Fifthly, as to the date of the composition of the book. Dr. Driver states this to be at earliest about B.C. 300, but more probably B.C. 168 or 167 (p. 467). Delitzsch is bolder and more critical; he says about B.C. 168. But to be true to all the facts, we ought rather to say that, while some evidence points to a date not earlier than B.C. 300, other facts point to the reign of Antiochus Epiphanes, and perhaps more definitely still to the period between the end of Dec. 165 (the dedication of the temple, which is mentioned

[1] *Einleitung in das A.T.*, ii. 309.

in Daniel viii. 14) and June 164 (the end of the seventieth year-week, when the writer of Daniel expected the tyrant Antiochus to "come to his end.").[1]

It was a pity that so little could be said on the composition of the book. Reuss and Lagarde both held that the book was made up of a number of separate "fly-sheets," and Dr. C. H. H. Wright maintains that it is but an abridgment of a larger work. The theories of Lenormant, Zöckler, and Strack also deserved a mention. On Meinhold's theory a somewhat too hesitating judgment is expressed (p. 483), which should be compared with Mr. Bevan's more decided view in his *Daniel*. From the form of the opening sentence of par. 3 on page 482, I conjecture that something on this subject may have been omitted. But if by so doing the author obtained more room for his *linguistic* arguments, I can but rejoice. Gladly do I call attention to the soundness of the facts on which these are based and the truly critical character of his judgments, and more particularly to what is said on the Aramaic of the Book of Daniel, and the eminently fair references to Prof. Margoliouth.[2]

But the treatment of the language of Daniel is but the climax of a series of linguistic contributions. To any one who has eyes to see, the special value of the book consists in its presentation of the linguistic evidence of the date of the documents (cf. p. 106). I do not say that I am not sometimes disappointed. No wonder; did not a good scholar like Budde, in 1876, claim the Elihu-speeches for the original Book of Job on grounds of language? Often I could have wished both that more evidence were given

[1] The fullest justification of this is given by Cornill, *Die siebzig Jahrwochen Daniels* (Königsberg, 1889); cf. *Einleitung*, p. 258. This little treatise deserves a fuller criticism than it has yet received.

[2] Mr. Bevan's mainly linguistic commentary on Daniel and Mr. Brasted's study on the order of the sentences in the Hebrew portions of Daniel (*Hebraica*, July, 1891, p. 244, etc.) appeared after the completion of Dr. Driver's work.

and a more definite conclusion reached (*e.g.* on Joel); but I recognise the difficulties with which Dr. Driver had to contend, arising partly from his limited space, partly from the unfamiliarity of the reader with this style of argument. With Dr. Driver's remark in the *Journal of Philosophy*, xi. 133 (note [1]) I agree, and when Dr. Briggs suggests that in my researches on the Psalms "the argument from language is not employed with much effect," [1] I feel that if not quite as firm as I might have been, I have been at least as bold as Dr. Driver would have been; indeed, I am indebted to my colleague for criticisms of my "Linguistic Affinities of the Psalms," which tended rather to the limiting than to the heightening of their "effect." I think that I should now be able to put forward a few somewhat more definite conclusions (positive and negative), but Dr. Driver's self-restraint on p. 361 will perhaps show Dr. Briggs that if I erred, it was in good company. Let me add that the author himself has not lost the opportunity of giving some sufficiently definite conclusions on the development of Hebrew style. It is on a paragraph which begins by stating that "the great turning-point in Hebrew style falls in the age of Nehemiah" (p. 473). The result thus indicated is based upon much careful observation. It agrees substantially with the view of H. Ewald (*Lehrbuch*, p. 24), which is a decided improvement upon Gesenius's (*Gesch. der. hebr. Spr.*), but must however, as I believe, be qualified, in accordance with the great variety of Hebrew composition.[2]

In bringing this review to an end, let me say once more how much more gladly I would have echoed the words of that generous-minded eulogist of this book—Prof. Herbert

[1] In a very generous notice of *Bampt. Lect.*, *North American Review*, January, 1892, p. 106.

[2] Cf. *Bampt. Lect.*, pp. 460-463; Geiger, *Urschrift*, pp. 40, 41. I need not say that I am by no means a disciple of this brilliant but too hasty critic.

E. Ryle.[1] I have written because of the illusions which seem gathering fresh strength or assuming new forms among us, and if I have shown some eagerness, I trust that it has been a chastened eagerness. The work before us is a contribution of value to a great subject, and if the facts and theories which it so ably presents should influence the higher religious teaching, no one would rejoice more than myself. But solid, judicious, and in one place brilliant as it is, it requires much supplementing as a sketch of the present state of criticism—not merely in the sense in which this must be true of even the best handbooks, but for reasons which have, as I hope, been courteously stated. The author appears to have thought that criticism of the Bible was one of those shy Alpine plants of which it has been well said that "we can easily give our plants the soil they require, but we cannot give them the climate and atmosphere; the climate and atmosphere are of as much importance to their well-being as carefully selected soil." I venture, however, to hope that he is unduly fearful, and that the mental climate and atmosphere of England is no longer so adverse as formerly to a free but reverent Biblical criticism. Indeed, one of my chief grounds for advocating such a criticism is that it appears to me to be becoming more and more necessary for the maintenance of true evangelical religion. It is, therefore, in the name of the Apostle of Faith that one of the weakest of his followers advocates a firmer treatment of all parts of the grave historical problem of the origin of our religion.

T. K. CHEYNE.

[1] See *Critical Review*, Jan., 1892.