

A Reply to Dr. Baxter.

BY PROFESSOR A. S. PEAKE, M.A., FELLOW OF MERTON COLLEGE, OXFORD.

IT may surprise some readers of THE EXPOSITORY TIMES to find that I am writing this reply. After I have been utterly discomfited by Robertson Smith, slain by him by anticipation, and my argument expressly annihilated by Wellhausen, they may think that in all decency I should 'blush unseen' for the rest of my life. And no doubt those who accept Dr. Baxter's criticisms on Wellhausen will agree with him that my article is full of 'evasions and suppressions and inconsequences,' with notable misrepresentations of himself and, 'in a most superlative degree,' of Wellhausen, and that they never have 'met in with such a glaring misleader of the public.' But they will no doubt be properly grateful that Dr. Baxter has resisted the temptation to 'turn in absolute contempt from such a controversialist' as myself, since he has given them so crushing a demonstration of my incompetence. If I must plead, as indeed I must, that I remain wholly unrepentant, perhaps they will put down my contumacious obstinacy to a double dose of original sin. To those who are prepared to listen I give my reasons for my position.

First, then, I have not read his book, and again and again this is flung at me, as if I were reviewing his book. I explicitly state that I am not dealing with the book as a whole; I do not review it, as he asserts. My reasons are stated, but I will say this further. Dr. Baxter complains in his Preface that the articles in *The Thinker* were left unanswered, and I thought that it would be nice on my part if I obliged him. In the next place, he has himself told us at the outset of his book that Wellhausen's first chapter 'lends itself very readily to exclusive treatment.' Accordingly, his chapters appeared as an independent work in *The Thinker*, and as sufficiently complete and self-contained for him to publish testimonials to its merits. If so, I am within my rights in publishing my reasons for regarding it as unsatisfactory. Thirdly, I was able to work at this section with much greater care than would have been possible if I had reviewed the whole book. It was with great difficulty that I made time to go twice through these chapters, and

twice to turn up every reference, and then a third time read, as I wrote my article, the passages on which I touch. If I had reviewed the whole book, I must have relied on a single reading and much less careful testing of references. I venture to think that no one of those, whose eulogies on his First Part Dr. Baxter has quoted, took anything like the trouble before they wrote their opinions that I took before I wrote mine.

Again, I am charged with misrepresenting him. I say that 'he tells us again and again that Wellhausen's whole position is overturned in these chapters.' It is certainly the fact that the phrase 'whole position' is borrowed from Wellhausen, and in a quotation from Dr. Baxter the inverted commas are retained, so that my paper is not wholly without indication of this. But most people will suppose that Dr. Baxter intended to endorse this, as if he had said, 'By his own confession Wellhausen's whole position is hereby overturned.' (As a matter of fact he does not quite understand what Wellhausen means by his 'whole position,' but this by the way.) In truth, if Dr. Baxter were right as to the Sanctuary, I should be disposed to agree that the case was made out, and the testimonials to Part I., to which he invites our attention, seem to support the same view. He says, further, that he repudiates the opinion I ascribe to him, and says 'the opposite.' As a matter of fact, he says in the Preface substantially what he quotes to prove this statement, so that I *had* read his opinion that Part II. provides more illustration than Part I. of the inaccuracy of Wellhausen's assertions. As to the statement itself, I am afraid I do not follow him. 'The opposite' of what I attribute to him would be either that he claimed that Part I. did not overturn Wellhausen's position, or that he did not claim that Part I. overturned it. But his point is that Part II. overturns it much more effectually, which is not 'the opposite' of what I attribute to him. Part I. is to Part II. as water is to wine. If I vary the metaphor, and say Part I. is to Part II. as gunpowder is to dynamite, am I to be

blamed for saying Dr. Baxter claims to have blown up the position with gunpowder, because he tells us that he will use dynamite, which will be still more effective? Is the latter 'the opposite' of the former?

A much more important charge follows. I have misrepresented Wellhausen, it seems, 'in a most superlative degree.' If this were really made out, I should hide my diminished head without more ado. I propose to rebut this charge in two ways—by actual quotation from Wellhausen, and by examining Dr. Baxter's proofs for his assertions. I will first quote Dr. Baxter's account of my contention. 'He says, Wellhausen virtually takes the whole mass of recent (so-called) critical conclusions for granted, as needing no proof, and that he has only to settle whether P comes before or after D.' For the sake of precision, I quote my actual words as to what he takes for granted: 'Criticism had achieved several definite results, the analysis of the Hexateuch into the four main documents, now commonly known as J, E, D, and P, the dating of the Deuteronomistic Code in or shortly before the reign of Josiah, and of J and E, including the Book of the Covenant, in the earlier period prior to Josiah. All this is assumed by Wellhausen as common ground, and he never intended to prove any of these points.' I reiterate this, and proceed to establish it by quotations from Wellhausen. On page 6 he says, with reference to the literary analysis: 'At present there are a number of results that can be regarded as settled.' He then states what some of them are, namely, that the following documents have been discovered in the Hexateuch—Deuteronomy, the Grundschrift (=P), the Jehovistic history book, which has been analysed into J and E. He accepts this analysis, and refers to papers of his own in which it is discussed. Next as to the date, he says, on page 9: 'With regard to the Jehovistic document, all are happily agreed that, substantially at all events, in language, horizon, and other features, it dates from the golden age of Hebrew literature, to which the finest parts of Judges, Samuel, and Kings, and the oldest extant prophetic writings also belong,—the period of the kings and prophets which preceded the dissolution of the two Israelite kingdoms by the Assyrians. About the origin of Deuteronomy there is still less dispute; in all circles where appreciation of scientific results can be looked for at all, it is recognised that it was com-

posed in the same age as that in which it was discovered, and that it was made the rule of Josiah's reformation, which took place about a generation before the destruction of Jerusalem by the Chaldeans.' This is not merely a sketch of critical opinion at the time, nor have I mixed up this sketch with the statement of the book's objects. It is Wellhausen's endorsement of these critical results. But Dr. Baxter may urge, 'True, Wellhausen endorses these results, and regards them as settled, but he does not assume them.' Now this is a vital point, for my main criticism of Dr. Baxter rests precisely here, that Wellhausen does assume them, and does not intend to prove them. Accordingly, we find him on p. 13 saying what his two chief assumptions are: 'The assumptions I make will find an ever-recurring justification in the course of the investigation; the two principal are, that the work of the Jehovist, so far as the nucleus of it is concerned, belongs to the course of the Assyrian period, and that Deuteronomy belongs to its close.' These are precisely the assumptions that I said he made.

But I am charged with misstating, not simply the assumptions, but also the aim of the *Prolegomena*. I will quote my description of this, and then justify it by quotations from Wellhausen: 'The main question that he had to discuss was the date of the Priestly Code. His book was not directed against the traditional view at all, but against the prevailing critical view that P was earlier than Deuteronomy.' On p. 8 Wellhausen says: 'Now the Law, whose historical position we have to determine, is the so-called "main stock," which, both by its contents and by its origin, is entitled to be called the Priestly Code, and will accordingly be so designated.' Again, on p. 9: 'It is precisely this Law, so called *par excellence*, that creates the difficulties out of which our problem rises, and it is only in connection with it that the great difference of opinion exists as to date.' And he concludes that paragraph with the words, 'It is the Priestly Code, then, that presents us with our problem.' In fact, so much is it the main object of the *Prolegomena* to prove the Grafian view that P is later than Deuteronomy and Ezekiel, that Wellhausen has to warn his readers that 'not everything we have hitherto discussed proves, or is meant to prove, Graf's hypothesis' (p. 368). And two pages earlier, dealing with 'the real point at

issue,' he says: 'There are in the Pentateuch three strata of law and three strata of tradition, and the problem is to place them in their true historical order. So far as the Jehovist and Deuteronomy are concerned, the problem has found a solution which may be said to be accepted universally, and all that remains is to apply to the Priestly Code also the procedure by which the succession and the date of these two works has been determined—that procedure consisting in the comparison of them with the ascertained facts of Israelite history' (p. 366).

I think these quotations warrant my statement that 'the main question he had to discuss was the date of the Priestly Code.' But I have not done with this point yet, for I have still to deal with Dr. Baxter's quotations. He says, not only that I have turned the *Prolegomena* into an utter farce, but that Wellhausen 'actually proclaims that, if he had written a book on the lines on which Mr. Peake says he has written, he would have been producing a book "of no value."' Here Dr. Baxter has confused the *aim* with the *method* of the book. I say, the chief aim of the book was to prove the late date of P. What Wellhausen says would have been 'of no value,' is to have adopted a particular method of proving it. Let me quote the passage to which Dr. Baxter refers: 'Now it is admitted that the three constituent elements are separated from each other by wide intervals; the question then arises, In what order? Deuteronomy stands in a relation of comparative nearness both to the Jehovist and to the Priestly Code; the distance between the last two is by far the greatest—so great that on this ground alone Ewald, as early as the year 1831 (*Stud. u. Krit.* p. 604), declared it impossible that one could have been written to supplement the other. Combining this observation with the undisputed priority of the Jehovist over Deuteronomy, it will follow that the Priestly Code stands last in the series. But such a consideration, although, so far as I know, proceeding upon admitted data, has no value as long as it confines itself to such mere generalities.' In other words, he will not adopt this particular method of proof, not because the data are wrong, but because the conclusion he wishes to reach would not necessarily follow. Nothing I have said is in conflict with this, and I entirely agree that such a method would not have been satisfactory. But this does not bar him from using these admitted data in his investi-

gation. It simply means that he cannot, without more ado, found his conclusion on these alone. I ought also to point out that the 'admitted data' referred to are not these that I speak of, hence what Wellhausen says would have been 'of no value' has no reference to the lines on which I said his book was written.

Next, there is the quotation he gives as expressly annihilating my position with reference to Deuteronomy. Once again I submit, that Dr. Baxter has not correctly apprehended Wellhausen's point. Wellhausen says that, though convinced that D must be dated in accordance with 2 Kings xxii., he does not, like Graf, use this as the fulcrum for his lever. What does this mean? It means, as the context shows, that the investigation 'proceeds on a broader basis than that of Graf, and comes nearer to that of Vatke.' When in turn we ask in what the difference consists, we find our answer on p. 368. Graf 'brought forward his arguments somewhat unconnectedly, not seeking to change the general view which prevailed of the history of Israel.' Wellhausen, on the contrary, brings forward a connected argument, in which the centralisation of the cultus is referred to as the origin of the particular divergences treated in his later chapters. Thus Deuteronomy becomes a turning-point in the history of the religion, and leads up directly to the provisions of the Priestly Code, and that is what he means by the assertion, 'my whole position is contained in my first chapter.' But the very fact that the priestly developments are to be explained by Deuteronomy makes it desirable that the *fact* of the developments itself should be independently proved, and this is done in the succeeding chapters. The words, 'is to be dated in accordance with 2 Kings xxii.,' are those on which, I suppose, Dr. Baxter chiefly lays stress, and it might be supposed that Wellhausen was really refusing to assume what I say he assumes. That can hardly be so, however, for in this very paragraph he asserts that he assumes that Deuteronomy belongs to the close of the Assyrian period. There is a distinction between this, however, and dating it in accordance with 2 Kings xxii. It is not a matter on which critics are agreed whether Deuteronomy was composed in the reign of Josiah or a little earlier. Wellhausen held the former, but other distinguished critics (*e.g.* Robertson Smith) held the latter. While, then, it was safe to assume that it was composed at one or other of these dates, it

was not safe to assume that it must be dated in accordance with 2 Kings xxii. And I carefully guarded this very point when I spoke of 'the dating of the Deuteronomic Code in or shortly before the reign of Josiah.' And the very context which Dr. Baxter urges against me, tells to my mind the other way. If the date of Deuteronomy has first to be ascertained, Deuteronomy cannot be the starting-point, but only if the date of Deuteronomy is fixed. However, I quote the actual words, which will be better than Dr. Baxter's vague reference to the context: 'Deuteronomy is the starting-point, not in the sense that without it it would be impossible to accomplish anything, but only because, when its position has been historically ascertained, we cannot decline to go on, but must demand that the position of the Priestly Code should also be fixed by reference to history.' That my interpretation is correct is, I think, shown by the passage from p. 366 already quoted, especially when taken in connexion with Wellhausen's complaint on the next page, that the procedure which when applied to Deuteronomy was called historico-critical method, should be called, when applied to the Priestly Code, construction of history. His point is, treat one as you have treated the other. The position of Deuteronomy has been historically ascertained, and it is only fair to apply the same methods to the Priestly Code.

With reference to the quotations he gives from Wellhausen (p. 506*b*), I cannot grant that he has accurately explained them. The following words I think cannot be justified as a correct account of Wellhausen's method: 'Then, he will take the legal enactments and demonstrate that they consist of three codes of divers authorship and widely sundered dates.' This is read into what Wellhausen says, and it is part of my contention that Wellhausen does not undertake the literary analysis. And this leads on to the point on which he lays such stress, that the book professes 'to hang on nothing but begin *ab ovo*.' Against this the passages I have already quoted, as to the assumptions of the book, may be urged. But I may point out further that Wellhausen confesses obligations in particular to Kuenen and Vatke. Now what is the 'new and characteristic inquiry' to which Dr. Baxter refers? As a matter of fact, it is not altogether new, though it was unusual among critics. It is the method of archæological investigation.

Wellhausen's point is that the older, anti-Grafian criticism, while it was successful in the literary analysis, and the dating of J, E, and D, when it came to date the Priestly Code, failed through its application of wrong methods. Thus, speaking of the polemic against the Grafian theory, he says: 'The firemen never came near the spot where the conflagration raged: for it is only within the region of religious antiquities and dominant religious ideas—the region which Vatke in his *Biblische Theologie* had occupied in its full breadth, and where the real battle first kindled—that the controversy can be brought to a definite issue.' Wellhausen's method then is not wholly new and independent, he had been anticipated in it by Vatke, 'from whom indeed,' he says, 'I gratefully acknowledge myself to have learnt best and most' (p. 13). 'Where is dependence then? It is excluded,' says Dr. Baxter, parodying the Epistle to the Romans. On the contrary, Wellhausen expressly declares his dependence on his predecessors. (Cf. also his reference, p. 369, to the papers on the composition of the Hexateuch where he speaks of himself as 'following in the steps of other scholars.') But when I too assert this, I am assured that I cast a great slur on Robertson Smith. As I have learnt more from him on the Old Testament than from anyone else, and as I have the highest respect for his opinion, I should feel that I was probably wrong if I had done so. But I think the 'withering plainness' is on my side. I so entirely agree with what he says in the preface that I refer to it in my article. But fortunately I can quote a passage from Robertson Smith which is decisive in my favour: In the *Old Testament in the Jewish Church* (2nd ed. pp. 226, 227), he has a note from which I will quote: 'The view set forth in this volume, which makes the priestly legislation the latest stage in the development of the law, is often called Wellhausenianism, but this designation is illegitimate, and conveys the false impression that the account of the Pentateuch with which Wellhausen's name is associated, is a revolutionary novelty which casts aside all the labours of earlier critics. In point of fact, Wellhausen had many forerunners even in Germany (George, Vatke, Reuss, Graf, etc.); while in Holland the lines of a sound historical criticism of the Pentateuch had been firmly traced by the master hand of Kuenen, and the results for the history of Israel had been set forth in his *Godsdienst van Israel* (Haarlem,

1869-70). But it was reserved for Wellhausen to develop the whole argument with such a combination of critical power and historical insight as bore down all opposition . . . It ought to be added that the new criticism does not reject the work that had been done by older scholars, but completes it. Those scholars were mainly busied in separating, by linguistic and literary criteria, the several sources of the Pentateuch; and this work retains its full value. The weak point in the old criticism was that it failed to give the results of literary analysis their proper historical setting.'

In view of this mass of evidence, I ask whether I can be justly stigmatised as a 'glaring misleader of the public,' or whether Dr. Baxter's claim can be justified that he thoroughly understands his Wellhausen?

Not content with supplementing his arguments by personalities, he has gone out of his way to make an offensive insinuation, and though it is confessedly 'in no way material,' he cannot rest content without repeating it. It is that I have borrowed my main contention from an article by Mr. Benn in the *Academy*. And now let us see how a plain tale will put him down. I have not even seen Mr. Benn's article to this day (Aug. 1st), and my own article was in print before I knew that he had written it. Further, in the April number of the *Primitive Methodist Quarterly Review*, I had included in the Current Literature section a four-page review, in which I dealt with the first part of Dr. Baxter's book. And a quotation from it will be useful here. After some detailed criticisms, I say it is unnecessary to dwell further on these, 'for there is a preliminary criticism which would justify a reviewer in refusing to trouble himself with any detailed criticism at all. . . . He displays a strange ignorance of the state of things. Else he could never have begun his attack on the critical view with the *Prolegomena* to the *History of Israel*. He would have known that this work was not written for those who objected to the critical view altogether. It has a definite place in the history of criticism, and is relevant to that stage. Hence it takes much for granted, because there was no dispute about it in the critical camp. Such a fundamental question as the approximate date of Deuteronomy was settled by common consent, hence it obviously did not lie within the province of the *Prolegomena* to discuss this question at length. So with other critical questions. The author is

perhaps unaware that Wellhausen had himself discussed the literary analysis of the Hexateuch in his work, *The Composition of the Hexateuch*. He was therefore able to dispense with the detailed proof of many positions assumed in the *Prolegomena*. There is small reason, then, in the author's frequent assertion that Wellhausen gives not the faintest trace of evidence for some of his statements. Of course he doesn't in the *Prolegomena*; he assumes the proof as well known. Nor can the author expect to be taken seriously till he consents to begin at the beginning, and to deal with the literary, which underlies all the historical, criticism. Let him examine and refute such a work as Kuenen's *Hexateuch*. Let him show that the analysis is a vain dream, as he calls the critical construction of the history, by refuting the arguments of the analysts.' I also speak of 'a fundamental vice of this book in method, the absence of any adequate literary criticism.' It may be obvious by this time who discovers mare's nests. I am not in the least surprised that Mr. Benn and I took the same line. I should have been much more surprised if we had not. One has not to read many pages to see how the land lies, and I believe ninety-nine critics out of a hundred would have hit independently on the same criticism. The coincidence might have suggested to Dr. Baxter that he had really misrepresented the critical view, but such a suggestion would perhaps be unlikely to occur to him. In any case I commend to his attention those works in which the case for the literary analysis and the dates of J, E, and D is stated. He will perhaps understand why I do not follow him in detail in his discussion about Shiloh and other matters. It is not the fact that I take no notice of it. My fundamental objection to his method covers that part in particular. While he insists that the arguments for the analysis into documents and the dating of J, E, and D are to be found in the *Prolegomena*, while he refuses to examine the literary and historical criticism of the authorities he uses for his case, discussion on this point is of little purpose. It is Dr. Baxter's failure to realise what Wellhausen assumes and what he tries to prove that vitiates a very large part of his criticism. Hence I leave many things 'ungrappled with' in my article, partly because we have no common ground for discussion, partly because in my space I could only deal with a small selection.

As to his criticisms under III., I cannot accept any of them. I am far from acknowledging that Dr. Baxter is right as to the evolution or anything else where he says I agree with him. If he misunderstands Wellhausen, I do not wonder that he misunderstands me. I stand by everything I said, and simply ask those who read Dr. Baxter's article kindly to read mine again, and if possible by all means to look up the passages in Wellhausen. I have already, in dealing with the fundamental error of Dr. Baxter's criticism, occupied so much space that I cannot claim more for these details.

The testimonials which Dr. Baxter quotes on p. 511 are of not the slightest interest to me, except as illustrating the need that we on our side should do what we can to spread the light. But one thing I must protest against warmly. 'Mr. Peake,' he says, 'winds up by giving great and generous prominence to what he calls two Scottish "puffs" of my book. For a reason which I shall state, let me give him one or two English "puffs."' This is what Dr. Baxter, who is liberal in the use of superlatives, would call a misrepresentation 'in a most superlative degree.' If I had said anything like this I should have been guilty of bad taste almost

as great as characterises Dr. Baxter's reference to "babes" at Oxford.' Considering that the two theologians from whom I count myself to have learnt most are Scotsmen, I am not likely to have fixed on the nationality of the writers in this offensive way, especially as this kind of reference seems to me to be almost criminal. Needless to say, I never call them Scottish 'puffs.' If Dr. Baxter were in my place, he would no doubt have pilloried me as 'a glaring misleader of the public,' a controversialist from whom he might turn with contempt. I content myself with a protest. Finally, I by no means think that Dr. Baxter is concerned with Wellhausen's consistency alone. I recognise that he believes himself to be fighting the good fight of faith. I wish, indeed, that some of his weapons were not quite so carnal, yet I do full justice to his good intentions. But, unfortunately, he has left so many uncaptured fortresses in his rear, that I see no prospect of victory whatever. May I make one suggestion to him? Controversy with him would be much pleasanter if he were a little less arrogant and self-confident, and if his language did not too often pass all the bounds that literary courtesy has marked out for us.

Contributions and Comments.

The Ecclesiasticus Discovery.

IN your able remarks on the leaf of Ecclesiasticus, identified and edited by Mr. Schechter, you have made a little slip of the pen about which I should not have cared to trouble you were it not that there is a moral as well as a literary interest involved in it. You quote correctly Mrs. Lewis's letter to the *Academy*, which states that we purchased the said leaf in Palestine, yet over the page you say that it 'was found on Mount Sinai.' There is very little Hebrew indeed at Sinai; it is highly improbable that this leaf ever was there; and we could not have brought it thence by honest means, as the convent authorities will not sell any of their treasures.

MARGARET D. GIBSON.

Cambridge.

That Apollon knew an Early Written Gospel.

I HAVE read with great pleasure the kindly notes on my Acts, in No. 6 of your most instructive journal. But permit me to say that with regard to Acts xviii. 25, Dr. Salmon's case and mine is not so hopeless but that we may undertake our defence. In my opinion, the verb *κατηχῆσθαι* has not a very strict meaning as to where the instruction comes from, whether from a book directly or from a person; but since men were at that time generally instructed orally, not in a literary way, it is used in the majority of cases of oral teaching. I should like to except the passage in Rom. ii. 18, *κατηχούμενος ἐκ τοῦ νόμου*. This is explained (for instance, by Wilke-Grimm):