Hesiod's *Works and Days*. Dr Reif was so sure that I "followed" Plöger, that he clearly did not consult the German article. Had he done so, he would have discovered that Plöger does not even mention Koh. iii 1-8!

Further misgivings as to Dr Reif's serious reading of my book arise from his final paragraph. Referring to my statement that Koheleth was acquainted with the Jewish religious climate of his day, he comments: "That this day was before that of Ben Sira, at a time when Mishnaic Hebrew was 'experimental' (p. 183) and not yet able to give adequate expression to ideas from non-Israelite sources and during the earliest years of the Hasmonean dynasty, is the central and most controversial conclusion of the book ...". He has thus completely misunderstood my standpoint, for I have persistently argued that Koheleth was after, not before Ben Sira. Labouring under such misconceptions and confusion, it is not surprising that he should regard my arguments as "tortuous, mutually contradictory and unconvincing" (p. 124). Dr Reif should have read the book carefully, comprehended its thrust and mastered its arguments before attempting a review of it.

Bangor, North Wales

C. F. Whitley

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A REPLY TO DR C. F. WHITLEY

I am grateful to the Editorial Board of *Vetus Testamentum* for allowing me this opportunity of commenting on the points raised above by Dr Whitley in connection with my review of his book. The burden of my detailed and cautious criticism was that Whitley's understanding of the development of the Hebrew language in the period of the Second Temple was methodologically unsound and that his arguments for maintaining the anteriority of Ben Sira to Ecclesiastes were based on faulty premises. It is significant that Whitley makes no response to this important criticism but confines himself to disputing some of the less central elements in my original remarks. Whitley's attempted rejoinder amounts to five minor points on which it will not be necessary to dwell at length:

1. Whitley's failure to see the difficulty to which F. Zimmermann was responding arises out of his over-simplification of the meanings of the roots *gēr* and *jyr*. In connection with my own understanding of the roots *gēr* and *jyr*, Whitley's understanding of the development of the Hebrew language in the period of the Second Temple is methodologically unsound and his arguments for maintaining the anteriority of Ben Sira to Ecclesiastes are based on faulty premises. It is significant that Whitley makes no response to this important criticism but confines himself to disputing some of the less central elements in my original remarks. Whitley's attempted rejoinder amounts to five minor points on which it will not be necessary to dwell at length:

1. Whitley's failure to see the difficulty to which F. Zimmermann was responding arises out of his over-simplification of the meanings of the roots *gēr* and *jyr*. In connection with my own understanding of the development of the Hebrew language in the period of the Second Temple is methodologically unsound and his arguments for maintaining the anteriority of Ben Sira to Ecclesiastes are based on faulty premises. It is significant that Whitley makes no response to this important criticism but confines himself to disputing some of the less central elements in my original remarks. Whitley's attempted rejoinder amounts to five minor points on which it will not be necessary to dwell at length:

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of these roots see, for instance, A. A. Macintosh, "A Consideration of Hebrew \^\text{\textsuperscript{g}}\text{\textdagger}r", VT 19 (1969), pp. 471-9; S. C. Reif, "A Note on \^\text{\textsuperscript{g}}\text{\textdagger}r", VT 21 (1971), pp. 241-4; P. A. H. de Boer, "Cantate Domino: An Erroneous Dative", Oudtestamentische Studiën 21 (1981), pp. 55-67.

2. In response to my invitation to raise the level of his arguments by referring to the etymology of the various roots \^\text{\textdagger}lm in the Hebrew Bible, Whitley simply repeats the comment on the word h\textsuperscript{\textdagger}l\textsuperscript{\textdagger}l\textsuperscript{\textdagger}m on pp. 31-2 of his book. In connection with \^\text{\textdagger}rr he expands a little on what he originally wrote but in an expositional rather than etymological vein and consequently with no effect on his linguistic thesis.

3. I note that Whitley offers no refutation of the important linguistic researches of the distinguished Hebraist Israel Yeivin, to which I referred, or of the widely accepted revisions of P. E. Kahle's theories which motivated my criticism of the history of Ben Sira offered in his book. I therefore find it difficult to understand how he can dismiss these recent developments in so cavalier a fashion, particularly since in doing so he seems first to reject the relevance of detailed linguistic data in favour of a more general historical approach and subsequently to reverse his position completely.

4. Since he devotes over eighty per cent of the text of his book to linguistic matters and defines his work as "a fresh examination of the language of the book" (p. 3), it is clear what Whitley regards as the main import of the volume. With this in mind, I, in common with other reviewers, devoted the substance of my remarks to his linguistic thesis and thought it unfair to be anything but brief and uncontroversial about pp. 149-84 which treat of Koheleth's thought and amount to no more than an appendix to the book. In reply to his comments on one half-sentence of mine, I simply refer the reader to how reviewers in various countries have understood and evaluated the last part of his work. For Professor R. N. Whybray it is "too brief to do justice to the subject" (S. O. T. S. Book List [1980], p. 79); Professor Roland Murphy is surprised that he "does not seriously discuss the evidence that R. Braun has presented", regards some of the similarities as "entirely too general" and is doubtful whether his "analysis of Greek influence will be adopted by most students" (CBQ 42 [1980], p. 256); in the words of Father J.-L. Ska "l'étude comparative pourrait ... se révéler fragile aux