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Editorial: History, faith and ethics

Incoming governments characteristically blame their problems on the preceding administration. As incoming editor of *Themelios* my feelings are the reverse. Readers will doubtless join in a warm expression of thanks to Rev. Dr David Wenham who has edited the journal so competently for eight years. We are delighted, however, that Dr Wenham's services to *Themelios* will not be lost. He joins the ranks of Associate Editors, with particular responsibility for the NT field. We are also pleased to welcome Dr David Lim to the team of International Editors. Dr Lim is Academic Dean at the Asia Theological Seminary, Manila, Philippines.

After such a lengthy period in the hands of a NT scholar, readers may be tempted to see in the decidedly OT flavour of this first issue under new management a reflection of the new editor's field of interest. They would not be entirely mistaken, but the truth is that on this point at least I can lay most of the responsibility on the last 'government' for the excellent contributions I inherited.

John Bimson's survey of the present state of historical research on the emergence of Israel in Canaan and the review article on yet another History of Israel focus our thoughts on what for many theological students is one of the most difficult issues that confronts them — the historicity of the biblical narrative. What the articles show clearly is that even in the ranks of evangelical scholarship there is no standard or unanimous 'position', either on the level or nature of historicity in accounts which all accept as the word of God, or in the reconstruction of Israel's history from all the available evidence. Bimson most helpfully classifies a bewildering range of theories on the origins of Israel and in the process critiques the views of evangelical scholars who themselves critique others. Students would do well to consult fuller developments of his own position referred to in the bibliography. It is clear that a commitment to the truthfulness of the biblical narrative does not prohibit us from acknowledging that the historical processes involved were much more complex and included many more events than the biblical story has chosen to incorporate. To say that biblical history is theological history is by no means to denigrate its value as history. All history writing proceeds from some interpretative framework which governs the selection of recorded events and processes. But it does mean that we need not adopt a suspicious stance towards all 'scientific' historical research on biblical times just because it comes up with theories about 'what actually happened' which are not in the Bible. We do need to cultivate humility, both before the biblical historical text in the same way and for the same reasons as humility should characterize our approach to any part of the Bible, and before the extra-biblical evidence presented (not invented!) by historical researchers.

Robert Jackson's article may at first sight seem unrelated to the dominant theme in this issue. But in fact it represents a

very healthy counterbalance inasmuch as it tackles a brand of the theology which makes heavy use of the OT but does so within a seriously distorted interpretative framework. And it is to the addressing of this kind of practical issue that all our study of the Scriptures must ultimately lead if it is not to remain finally antiquarian and irrelevant. For just supposing John Bimson's dream were to be realized and he were to put together a reconstruction of Israel's origins upon which all scholars unanimously agreed (I said it was a dream!), taking into account the biblical narrative and squaring with all the archaeological findings. . . . We should still be left with the question, So what? Why Israel? What is the meaning and purpose of this people? What is the significance of their history being so painstakingly recorded in our Scriptures?

To answer those questions takes us beyond history alone into the area of faith and ethics. Faith, because it was Israel's religious belief (shared by Christians) that their history was actually God at work for the redemption of humanity and creation. Ethics, because their perception of that history was articulated in a covenant commitment which demanded a distinctive ethical response from them. Whatever our final (or provisional) viewpoint on the historical problems of Israel's emergence, emerge they certainly did. And from our earliest records of them, we find this insistence that to be Israel was to be something different. They were the people of Yahweh, and were called to demonstrate it by living as the people of Yahweh in the midst of the nations. Israel's history took place on a very public stage, for good (Dt. 4:5-8) or ill (Dt. 29:22-28). Israel was thus called to a mission and an ethic (*cf.* Gn. 18:18-19; Ex. 19:4-6) and that fundamentally is the heart of the relevance of their history to us, their spiritual heirs.

It is within this context that as Christians we value the work of the historians and learn from them even while reserving the right to disagree even radically with their presuppositions or conclusions. Bimson exposes some of the critical weaknesses in Gottwald's reconstruction of Israel's origins, for example, and others have rejected his ideological tailoring of all the OT material to his own sociological dogma. Nevertheless, one unquestionable service Gottwald has rendered is to demonstrate the link (he would call it a structural-functional mutual dependency!) between Israel's religious faith, especially as regards the portrayal of Yahweh, and their social, economic and political structures and ideals. To him, Yahweh was cast in a certain form as a 'feedback servo-mechanism' in support of their social struggle and thrust to egalitarianism. To us, it was the other way round. Israel's remarkable economic and political transformation of Canaan, and their insatiable drive for justice and the protection of the weak, vulnerable and marginalized, were reflections of the character and demands of the God they worshipped. This was what it meant to 'walk in the way of the LORD' (Dt. 10:12-19). To be an Israelite, to worship Yahweh, was to be committed to certain social and personal ethical

ideals. That he has shown to be a matter of historical fact in itself.

So our study of OT history cannot stop with a mere recounting of rearranged facts. For us, as for them, history demands response. But our response must match the paradigm of Israel's own response, in the covenant law and the prophetic demand based on it. Failure to do so is what makes the 'prosperity gospel', so carefully exposed by

Jackson, so insidious. It has the appearance of biblical form, but denies the content of a major biblical ethical thrust. As students of the Bible, then, let us present ourselves as workmen with no need to be ashamed of the thoroughness of our critical research and historical carefulness. But let us not lose sight of the point of the exercise, which is that the Scriptures are given to be profitable for salvation and for 'training in justice' (2 Tim. 2:16; 3:15-17).

The origins of Israel in Canaan: an examination of recent theories

John J. Bimson

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Introduction

'There have been only two important views of the conquest of Palestine by ancient Israel', wrote G. E. Mendenhall in 1962, in an article in which he offered a third.¹ Since then hypotheses have proliferated, and the question of Israel's origins has become a vastly complex one. The purpose of this article is twofold: to provide some account of the development and current standing of the main theories on offer, and to assess their relative merits. There are, of course, more theories than can be discussed even in this overlong article, but most of the omitted ones are variants of those included, so that many of my comments will be applicable beyond the scope of this discussion.

The theories discussed here fall into two main groups: those which assume that Israel entered the land of Canaan from outside, and those (now the majority) which assume that Israel was to a great extent indigenous to Canaan. Those in the latter category all post-date 1960. I will give a brief account of each before offering an assessment of it.

1. Hypotheses in which Israel enters Canaan from outside

1a. A 13th-century conquest

We begin with the view which adheres most closely to the biblical traditions: that the Israelite tribes entered Canaan by force, acting more or less in concert. There are, of course, a number of variants on this view in terms of the *date* of Israel's entry into the land. The most noteworthy alternative to the view outlined here is that the exodus and conquest occurred in the 15th century BC, as implied by a straightforward reading of 1 Kings 6:1. Although this continues to be favoured by a number of American evangelical scholars,² and by the present writer,³ it has not been influential in recent decades and will not be treated in detail here. We will see below, however, that it deserves renewed attention.

The majority of those scholars who wish to retain the biblical picture of a more or less unified and violent conquest have long favoured the theory that this event occurred in the

13th century BC. Its classic form took shape at the hands of the so-called Baltimore School, consisting primarily of W. F. Albright and his pupils J. Bright and G. E. Wright. Albright's own excavations in the 1920s and 1930s at Beitin and Tell Beit Mirsim (which he believed to be the sites of biblical Bethel and Debir respectively) unearthed destruction levels which he associated with the traditions of the conquest. A date at the end of the LBA (Late Bronze Age), in the second half of the 13th century BC, seemed to be indicated by the pottery evidence.⁴ British excavations (1932-8) at the site of Lachish (Tell ed-Duweir), under the direction of J. L. Starkey, also produced a destruction layer, which Albright dated c. 1230/1220 BC⁵ (though Olga Tufnell, one of the British excavators, preferred a date in the 12th century BC⁶). Y. Yadin's excavations at Hazor (1955-8) also produced evidence of a violent destruction, in this case clearly datable to c. 1220 BC, thus adding a fourth major city to the list.⁷ Hence Bright was able to write in his *A History of Israel*: '... It may be regarded as certain that a violent irruption into the land took place late in the thirteenth century!'⁸

These destruction levels were merely part of an impressive web of evidence which seemed to point to a 13th-century setting for the exodus and conquest. With the conquest set at around 1220 BC, the exodus would have occurred (according to Nu. 14:33, *etc.*) forty years earlier, around 1260 BC. This places it in the reign of the pharaoh Ramesses II, in keeping with the reference in Exodus 1:11 to the enslaved Israelites building the store-cities Pithom and Raamses. Excavations at the likely site of Raamses revealed no evidence of occupation during Egypt's Eighteenth Dynasty, effectively excluding a date before the 13th century BC for the exodus.⁹ N. Glueck's surface-surveys of Transjordan, undertaken chiefly in the 1930s, led to the conclusion that the region lacked any settled population between the 19th and 13th centuries BC, which ruled out a date before the 13th century for the events of Numbers 20:14-21 and 21:21 - 24:25.¹⁰

This 13th-century scenario has been adopted by several evangelical scholars. Among British evangelicals K. A. Kitchen has long been its foremost exponent.¹¹ Some American evangelicals have also adopted it,¹² though others have balked at a non-literal treatment of 1 Kings 6:1 (discussed below) and have continued to argue for a 15th-century date.¹³

Problems and methodology

The 13th-century date has never been without its problems. To set against Debir, Bethel, Lachish and Hazor, with their clear destruction levels, there are the troublesome cases of Jericho, Ai and Gibeon.

Since Kathleen Kenyon's excavations at **Jericho** (1952-8) it has been widely accepted that no traces of the city attacked by Joshua are to be found. Kenyon's own view was that, following the destruction of MBA (Middle Bronze Age) Jericho, around 1560 BC, the mound was deserted until a LBA town began to grow up around 1400 BC. This occupation lasted only about a century, and was followed by another long period of abandonment.¹⁴ No evidence was found which could support the existence of a town in the second half of the 13th century BC. The site of **Ai** (Khirbet et-Tell, or simply Et-Tell) is another parade example of the failure of excavations to support the biblical tradition. Here the gap in occupation lasts at least a thousand years, c. 2200-1200 BC. Once again there is no city for Joshua's forces to attack in the late 13th century. This problem was already known after the excavations by Mme. J. Marquet-Krause in 1933-5,¹⁵ and it was hoped that the renewed excavations of 1964-72, led by J. A. Callaway, would produce some kind of solution, either by uncovering hitherto neglected evidence from the *tell* itself, or by finding an alternative site for the biblical city nearby. These hopes came to nothing; the gap at Et-Tell was confirmed and the search for an alternative site proved fruitless.¹⁶ J. B. Pritchard's excavations (1956-62) at El-Jib, the site of **Gibeon**, produced no LBA material except some burials from the 14th century BC, leading Pritchard to the conclusion that no town had existed there during the LBA.¹⁷ As early as 1965 Pritchard remarked that the problems encountered at Jericho, Ai and Gibeon (ironically, the three cities to which Joshua 2-9 give extended treatment) 'suggest that we have reached an impasse on the question of supporting the traditional view of the conquest with archaeological undergirding'.¹⁸

Supporters of the 13th-century conquest have not accepted this verdict, emphasizing the positive evidence and offering explanations for the negative finds from Jericho, Ai and Gibeon. For example, Bright has stressed that extensive erosion makes the situation at Jericho unclear,¹⁹ Wright and Yadin, following the same line, have explained the lack of LBA fortifications there in terms of the re-use of the MBA walls — though Wright admitted that there was no shred of evidence to support this theory.²⁰ Albright explained the problem of Ai by suggesting that the narrative in Joshua 8 originally referred to the taking of Bethel²¹ (though it bears no similarity to the account of Bethel's capture in Jdg. 1, beyond the fact that in both cases the Israelites take a Canaanite city!). He assumed there had been a very small settlement at Gibeon at the time of Joshua, thus explaining the virtual absence of LBA material there, and suggested that Joshua 10:2 (which speaks of Gibeon as a major city in that period) is an erroneous scribal gloss.²²

There are patent weaknesses in such explanations, especially in the latter two, where the biblical tradition has to be adapted to some extent before the archaeological evidence can be said to support it (and in the case of Gibeon there has to be a further assumption that some archaeological evidence has been missed). Criticisms recently levelled at the

methodology of the Baltimore School, that it involves inconsistencies, circular arguments and overinterpretations of the evidence,²³ have been justified more often than not.

Those evangelical scholars who have adopted the Baltimore School's scenario have tried to be more rigorously scientific in their defence of its weak points. Kitchen has emphasized that evidence of occupation can be eroded away during periods of abandonment, or simply missed by the excavator when (as is usually the case) limited areas of a mound are explored. He illustrates these points with examples from the archaeology of Egypt, in which excavation has failed to produce remains from sites which are known, on the basis of textual evidence, to have been occupied at the relevant time. Hence he has suggested that 13th-century BC Jericho has been entirely eroded away, that the LBA burials at Gibeon may indicate an occupation which has been missed by the excavator, and that the site of LBA Ai still awaits discovery.²⁴ Indeed, Kitchen has repeatedly stressed that 'absence of evidence is not evidence of absence', *i.e.* that lack of occupation cannot be assumed simply because no trace of it is found. Is this approach on firmer ground than that of the Baltimore School?

Kitchen is certainly right to caution against arguments from silence. However, we must ask how profound the silence is allowed to become before some notice has to be taken of it. To the old chestnuts of Jericho, Ai and Gibeon, we must also add Hebron, Arad and Zephath/Hormah, where the gaps are equally problematical.²⁵ Has the evidence been eroded or missed in every one of these six cases? Perhaps it has, but as the argument is extended to include more sites it inevitably becomes less convincing. It has also been noted that Kitchen does not consistently apply his own dictum.²⁶ He has used the lack of appropriately-dated evidence for activity at the site of Raamses, and for occupation in Transjordan, in order to argue against dating the exodus before the 13th century BC.²⁷ This line of argument has proved distinctly unwise in the case of Transjordan, where more recent surveys and excavations have overturned Glueck's theory of a gap in occupation;²⁸ and it may yet prove erroneous in the case of Raamses as well.²⁹ That, however, is not my main point here; the point is that 'absence of evidence' has not been allowed any significance in the cases of Jericho, Ai and Gibeon, but *has* been treated as 'evidence of absence' in the other cases mentioned above.

This raises serious methodological issues, especially when placed alongside Kitchen's treatment of 1 Kings 6:1. He correctly stresses that the OT's chronological statements must be treated in accord with 'Ancient Oriental principles', but this does not ultimately help his suggestion that the figure of 480 years in that verse results from a totalling of selected figures which actually represented overlapping periods.³⁰ As I have pointed out elsewhere, an alleged Egyptian parallel to which Kitchen appeals does not actually illustrate the process which he envisages, and 1 Kings 6:1 itself contains no hint that the figure results from totalling lesser periods.³¹ Furthermore, the 15th-century date produced by this verse is supported by the figure of 300 years in Judges 11:26, and to dispense with this fact by treating the latter figure in the same way³² is completely unconvincing. What has happened is that Kitchen and others have found the archaeological evidence for a 13th-century date so compelling that they have sought to re-interpret 1 Kings 6:1 and Judges 11:26 in the light of it. But at

the same time they have employed the archaeological evidence inconsistently in support of the 13th-century date. In other words, the standard evangelical argument for the 13th-century date has been no less flawed with circularity than the methodology of the Baltimore School.

To be fair, however, it must be mentioned that Kitchen has offered other evidence in favour of the 13th-century date, namely the clearly attested parallels between the form of the Sinai covenant and treaty texts of the late second millennium BC.³³ But this alone is not a sufficient counterbalance to the problems facing the 13th-century date.³⁴ These have recently multiplied, as we will now see.

Further difficulties raised by recent archaeology

Three recent developments now make any defence of a 13th-century BC conquest extremely difficult. One concerns the nature of the early Iron Age (Iron I) settlements which sprang up after the collapse of the LBA cities. Supporters of the 13th-century conquest have interpreted these as the work of the newly-arrived Israelites, but it is now acknowledged that their remains attest considerable continuity with the material culture of the LBA. If (as is virtually certain) these settlements are indeed the work of the Israelites, it seems unlikely that they were newly arrived in the late 13th century BC. This matter will be discussed more fully below in considering the infiltration theory of Israel's origins.

A second development has been the growing realization that very few cities of the LBA had any defensive walls. In the light of a thorough study of the evidence by R. Gonen, it is now clear that 'Only a handful of settlements were surrounded by a wall during the entire period, or even during part of it'.³⁵ The theory that LBA inhabitants reused the old MBA defences is disproved in a number of cases by the fact that LBA houses were built across the remains of the MBA walls. The biblical tradition that the Israelites were faced by towns which were 'fortified and very large', 'great and fortified up to heaven' (Nu. 13:28; Dt. 1:28; see Jos. 2:15; 6:1, 5; 7:5; 8:29; 10:20; 14:12; *etc.* for further references to walls and gates) simply does not fit the picture of relatively small, unwallled towns which has now emerged for the 13th century BC. Shortly before his death in 1985, Yadin tried to incorporate this discovery into his (essentially Albrightian) defence of the biblical conquest. In common with other supporters of the 13th-century date, Yadin had previously assumed that the culture of LBA Canaan was based on fortified city-states. Compelled by Gonen's evidence to abandon this view, he subsequently suggested that the weakness of Canaan's unwallled cities lent plausibility to the OT's picture of a swift conquest of the land: 'Decaying Canaan of the Late Bronze Age . . . was relatively easy prey for the highly motivated and daring tribes penetrating the country'.³⁶ As a method of salvaging the 13th-century conquest scenario this is not likely to be congenial to evangelical scholars, since it supports one aspect of the biblical tradition by surrendering another, namely that the Israelites encountered towns with walls and gates.

The third discovery concerns those towns which previously seemed to provide evidence for a 'violent irruption into the land' around 1230/1220 BC. B. G. Wood's thorough comparative study of the pottery from a wide range of sites reveals that the destructions of Bethel, Debir, Lachish and

Hazor all require redating to different degrees.³⁷ They did not all fall at the same time, but have to be distributed among no less than *three* waves of destruction spanning roughly a century. The first wave occurred at the end of the subdivision of the LBA known as LB IIB1, and should now be dated c. 1210 BC. Of the places mentioned in the Bible as taken by Israel, it included only Hazor. The second wave occurred c. 1170/1160 BC, at the end of LB IIB2 (the final phase of the LBA). This included Tell Beit Mirsim (Albright's candidate for Debir) and Beitin (traditionally Bethel). However, it is now very widely agreed that the true site for Debir is Khirbet Rabūd,³⁸ which was not destroyed in any of these three waves of destruction. The number of biblical sites involved in this second wave is therefore no more than one (Bethel), and even this should probably be excluded, as there are now serious problems involved in locating Bethel at Beitin.³⁹ The third wave of destruction fell early in the Iron Age, at the end of Iron IA1, c. 1125 BC. Of the places Israel is said to have taken, this included only Lachish.

It is clear, therefore, that either Israel's conquest of Canaan is not to be linked with these destructions (for which there are, in any case, alternative explanations), or it was a long, drawn-out affair, spanning about a century. The latter view has actually been proposed by D. Ussishkin, the current excavator of Lachish.⁴⁰ Again, this latter approach to the problem is not likely to appeal to evangelical scholars, surrendering as it does important aspects of the biblical tradition.

In the light of these serious difficulties it is very unlikely that the theory of a 13th-century conquest can be rescued. Since the theory has for a long time been the favourite defence of the historicity of the biblical traditions among evangelicals, its demise, once accepted, will require a major rethinking exercise. We will return to this in the final section of this article.

1b. The infiltration theory

That Israel's constituent tribes came into Canaan from elsewhere is a presupposition shared by the infiltration and conquest theories of Israel's origins. In their classic forms both theories have also focused on the 13th century BC as the time when Israel's entry took place.⁴¹ But they differ in two important respects.

While the 13th-century conquest theory, at least in its most widely held form, posits a more or less united movement by all or most of the Israelite tribes, the infiltration theory envisages groups with diverse origins settling at different times in different areas. Only after the settlement of these disparate groups did they coalesce into the entity Israel. Secondly, while the conquest theory involves the violent overthrow of Canaanite cities as an initial move by the newcomers, the infiltration theory relegates clashes with the Canaanites to a later stage in the process of Israel's formation. In the view of A. Alt, who originated the theory in 1925,⁴² the first phase was totally peaceful. It involved semi-nomadic pastoralists, who spent their winters in the desert fringes beyond Canaan, gradually making the transition to a settled agricultural existence in the hills where they were accustomed to graze their flocks each summer. The central highlands of Canaan were thinly populated, so their settlement involved little or no conflict with the existing inhabitants of

the land. Only when these settlers had become somewhat established, united and more numerous, did they attempt to wrest new land from the Canaanites of the plains and valleys. Thus began the phase of territorial expansion, which involved extensive armed conflict, but this did not come until the early days of the monarchy.

Alt's initial theory was supported and developed by M. Noth, who used literary-critical approaches to the text in an attempt to reconstruct the complex process of tribal settlement.⁴³ More recently M. Weippert has championed the theory and offered some refinements of it,⁴⁴ and J. M. Miller's reconstruction of the occupation of the land leans heavily on the Alt-Noth model.⁴⁵ Among Israeli archaeologists, the late Y. Aharoni was a staunch proponent of the theory,⁴⁶ and it is currently supported by M. Kochavi, A. Zertal and others.⁴⁷

Presuppositions and methodology

Although the infiltration theory does make use of specific biblical traditions, it clearly rejects the overall picture of Israel's origins found in the Pentateuch and the book of Joshua. It replaces a united, military conquest with a peaceful and piecemeal process. Any theory which proposes a picture so different from the biblical one must provide a plausible explanation of how the biblical picture arose. Does the infiltration theory succeed in this respect?

The stories in Joshua 2-9 of the destruction of Canaanite cities by the Israelites are explained by Alt and Noth as folk aetiologies (identified as such by the formula 'to this day', e.g. Jos. 8:28-29). The purpose of such stories was to explain contemporary phenomena in terms of past events. Specifically, the conquest narratives explain the existence in the land of ruined ancient cities. These aetiologies were originally Benjamite traditions, later given a pan-Israelite colouring through the role of Gilgal (in Benjamin) as the central sanctuary of an amphictyony of twelve tribes. The war stories of Joshua 10:1-15 and 11:1-15 are viewed as belonging originally to the tribes of Benjamin and Naphtali respectively, likewise acquiring their 'all Israel' flavour through the formation of the amphictyony focused on a central shrine. Similarly Joshua, whose historical role was supposedly limited to the traditions of Joshua 17:14-18 and 24:1-28, acquired the status of leader of all Israel.⁴⁸

This reconstruction of the formation of the tradition obviously depends heavily on the theory (developed chiefly by Noth) of a twelve-tribe Israelite amphictyony, and this has come under heavy criticism in recent years.⁴⁹ Without it the 'all Israel' traditions (and, indeed, the fact of Israelite unity itself) are difficult to account for if the tribes had diverse origins.⁵⁰

Secondly, Alt and Noth have been accused of assuming too readily that a narrative is unhistorical simply because it contains an aetiological element. Bright argued long ago that literary form alone cannot determine the historicity or otherwise of a story.⁵¹ B. S. Childs, C. Westermann and others have argued for a tighter definition of aetiology than the one Alt and Noth operated with, and Childs has shown that aetiological elements are secondary in the conquest narratives.⁵² In the light of such objections, even Weippert admits: 'Any solution of the problems raised by Alt and Noth would then be possible only on the basis of external evidence.'⁵³ That is,

he admits that archaeology could theoretically arbitrate in matters of historicity. Noth himself was, in fact, more prepared to listen to archaeological evidence than his critics have sometimes allowed. Although he originally classified the story of the destruction of Hazor in Joshua 11:1-15 as an aetiology, he revised his opinion in the light of Yadin's excavations and admitted that some historical reminiscence lay behind it.⁵⁴ However, since archaeological evidence has generally been ambiguous at best, Weippert affirms that ultimately the question of the nature of Israel's arrival in Canaan 'still depends on the evaluation of the literary character of the texts'.⁵⁵ Nevertheless, he (and implicitly Noth) has made an important methodological concession, for if another chronological setting could be found for the conquest, in which the relevant cities were destroyed, the aetiological explanation for the narratives would presumably be abandoned.

Optimism is not to be recommended, however, for Weippert's use of archaeological evidence is subjective and inconsistent. Thus he is happy to state that Et-Tell is 'definitely to be identified' with Ai⁵⁶ while rejecting the identification of El-Jib with Gibeon on the grounds that '... the results of Pritchard's excavations ... do not agree with the history of the town of Gibeon as this can be deduced from the written sources'⁵⁷ — a reference to the lack of LBA occupation there. This statement is truly astonishing, because the case for identifying El-Jib with Gibeon is actually much stronger (involving inscriptional evidence) than that for identifying Et-Tell with Ai. What lies behind Weippert's inconsistency is the fact that the gap in occupation at Et-Tell is in keeping with the supposed aetiological nature of Joshua 8:1-19, whereas the gap at El-Jib is inconvenient because Weippert wants to make historical use of the tradition in Joshua 9; the narrative of Israel's treaty with the Gibeonites in that chapter points, in his view, 'to peaceful relations between the Canaanite cities and the "Israelite" groups which came into the country'.⁵⁸ A more rigorous approach would demand either that Joshua 9 be divested of historical significance, or that *Et-Tell be rejected as the site of Ai*, because the results of excavations 'do not agree with the history of the town ... as this can be deduced from the written sources'!

In this instance (as, unfortunately, in others) the presuppositions of the infiltration theory are seen to be paramount. External evidence in practice has little or no chance of influencing 'the evaluation of the literary character of the texts'; that evaluation, and the evaluation of the external evidence itself, are both actually determined in advance by the theory.

Archaeology and the nature of the Israelite settlement

In the 1950s, during a survey of southern Upper Galilee, Y. Aharoni discovered 'many small Iron Age settlements in close proximity ... sometimes only one or two kilometres apart'.⁵⁹ The density of Iron Age settlement in this region exceeded that of any later period. On the basis of a pottery assemblage excavated during a trial dig at Khirbet et-Tuleil (Horvat Harashim), Aharoni assigned these settlements to the beginning of the Iron Age and dated them to the 13th-12th centuries BC. He stated: 'There is no doubt, in my opinion, that this wave of settlement from the beginning of the Iron Age is Israelite. ...'⁶⁰

Subsequent surveys conducted in other parts of the hill country by a younger generation of Israeli archaeologists have produced similar results. It is now widely recognized that the early Iron Age saw a proliferation of small settlements in the highland regions. The total number discovered now exceeds 300. Dozens more occur on the Transjordanian plateau.

The majority of these sites are villages of only 1-2 acres, though a few are considerably larger. Most are unwallled, though some (e.g. Giloh, south of Jerusalem, and several Upper Galilee sites) had fortifications. The majority of the early Iron Age villages were new foundations; for example, in the centre of the country 97 of 114 Iron I sites showed no trace of Late Bronze occupation.⁶¹ The material culture of the Iron I villages is uniformly poor. They display a limited variety of pottery types, indicative of a subsistence economy.

Because the central highlands were the region initially colonized by the Israelite tribes, and because the highland settlements display cultural continuity with the Iron II period, which is the time of the Israelite monarchy, the majority of archaeologists have agreed with Aharoni that the Iron I settlements should be associated with the Israelites. For this reason the Iron I settlements have recently assumed a centre-stage position in the debate over the nature of Israel's emergence in Canaan. There have (predictably) been major differences of opinion over the interpretation of these settlements.

Aharoni held firmly to the view that the Galilee settlements predated the fall of Hazor and thus supported the infiltration theory; they showed, in his view, that the conquest of Canaanite cities had been preceded by a time of peaceful settlement. Yadin consistently opposed him,⁶² and recent studies have shown conclusively that Aharoni was wrong; the destruction of LBA Hazor preceded by some decades the earliest Iron I villages.⁶³ However, these studies cannot be said to have vindicated the conquest theory, since (as we have seen above) it no longer seems likely that the fall of LBA Hazor can be associated with the conquest.

Another aspect of the Iron I settlements which has emerged through more recent research is the high degree to which their material culture is in continuity with that of the preceding LBA, notwithstanding its relative poverty. In fact, continuity with the LBA has been observed in every individual aspect of Iron I culture: pottery, axes, daggers, knives, clothing-pins, artistic traditions, cultic objects, scripts and architecture.⁶⁴ It is also evident (as will be discussed below) that the Iron I people were familiar with aspects of settled life and agriculture. These facts are difficult to square with the idea that they were pastoral nomads from the desert fringes. (As noted above, they are equally difficult to square with the notion that the settlers were Israelites newly arrived.)

The evidence indicates that the Iron I settlers were people who had had prolonged contact with the urban culture of LBA Canaan, not people who had initially settled well away from the city-states and later had only hostile relations with them. In short, the most recent assessment of the Iron I settlers does not sit comfortably with the infiltration theory, especially as formulated by Noth.

It is appropriate to mention here another objection which has been raised to the theory: that it depends on an outmoded view of nomadism.⁶⁵ Noth characterized the ancestors of Israel as 'land-hungry semi-nomads' hankering after 'a more settled life in the coveted agricultural countryside'.⁶⁶ Recent studies of nomadism have made clear that the pastoral nomads of the ancient Near East were not 'land-hungry', nor did they live isolated from or hostile to settled societies. Their relationship with urban culture was generally close and symbiotic; the economic and social relationships between the semi-nomadic and sedentary populations were reciprocal, pastoral nomadism complementing an agricultural economy to maximize the potential of the land's resources.⁶⁷

These findings do not totally rule out the infiltration theory, though they do require its drastic modification. This has been recognized by its most recent supporters, who have begun the modification process.⁶⁸ The scholar who has carried this furthest is V. Fritz, who has proposed what he calls a 'symbiosis hypothesis'.⁶⁹ This takes account of the latest nomadism studies, and of the continuity between LBA material culture and that of the Iron I settlers. Whereas Noth favoured the 13th century BC as the time when Israel's ancestors entered the land, Fritz pushes their arrival back to the 14th or even the 15th century BC.⁷⁰ This allows for a long period of contact between Israel's ancestors and Canaanite society of the LBA before the semi-nomadic groups eventually became sedentary at the beginning of Iron I. Only then did they become 'visible' archaeologically, since semi-nomads leave few traces of their existence. Their sedentarization, according to Fritz, was a response to changed economic conditions which affected the whole of Canaanite society at the end of the LBA.

While Fritz's symbiosis hypothesis saves the infiltration theory with respect to the archaeological evidence and nomadism studies, it does not, of course, avoid the problems raised above with respect to the biblical traditions.

2. Hypotheses in which Israel is indigenous to Canaan

2a. The Peasant Revolt theory

G. E. Mendenhall's attempt to construct a third alternative to the military conquest and peaceful infiltration theories emerged from a sense of frustration with current methodologies. The fact that such irreconcilable accounts of Israel's origins could be developed from the same evidence led him to question fundamental assumptions underlying them both. He listed these assumptions as follows: 'a. That the Twelve Tribes entered Palestine from some other area just prior to or simultaneously with the "conquest". b. That the Israelite tribes were . . . "semi-nomads" who seized land and settled upon it during and after the conquest. c. That the solidarity of the Twelve Tribes was an "ethnic" one, and that kinship was the basis of the contrast between Israelite and Canaanite.'⁷¹

After debunking widely-held misconceptions concerning semi-nomads and their relationship with settled agriculturalists (as mentioned above), and the belief that tribal organization indicated a (semi-)nomadic background, Mendenhall discussed the relationship between the terms 'Hebrew' and 'Apiru' (the latter occurring most notably in the 'Amarna letters', correspondence between Canaanite city-state rulers and their Egyptian overlords in the 14th century

BC). By assuming these terms to be synonyms, and defining them to mean someone who 'has renounced any obligation to the society in which he formerly had some standing . . . and has in turn deprived himself of its protection'.⁷² Mendenhall produced a radically new insight into Israelite origins: 'For if the early Israelites were called "Hebrews", they could be termed so only from the point of view of some existing, legitimate political society from which they had withdrawn.'⁷³ The early Israelites were therefore to be seen as the native peasantry of Canaan withdrawing from the oppressive regime of the city-state system. 'There was no radical displacement of population, there was no genocide, there was no large-scale driving out of population, only of royal administrators (of necessity!).'⁷⁴

Mendenhall did not, however, reject entirely the historicity of the Exodus-Sinai traditions. Indeed, they play a vital role in his hypothesis. In Mendenhall's view, the Yahwistic faith, and the concept of a community related to Yahweh by covenant, were brought to Canaan by a group of former slave-labour captives who had escaped from oppression in Egypt. When this group reached Canaan, the loyalty and obligations of the covenant community proved 'attractive to all persons suffering under the burden of subjection to a monopoly of power . . .', who therefore identified themselves with the former slaves and rapidly swelled their community.⁷⁵ But the original nucleus of Israel was 'only a small group'; 'there was no statistically important invasion of Palestine'.⁷⁶

Like the theories which it was intended to replace, Mendenhall's assumed that Israel's origins were to be sought in the late 13th century BC. The reasons for fixing on this date are not at all clear, and Mendenhall seems to have inherited it somewhat uncritically from the older theories.

Mendenhall's thesis initially had a cool reception. Its unfamiliar sociological approach made it less attractive than the older theories, which still retained their popularity. (The current fascination of biblical scholars with matters sociological was relatively undeveloped in 1962.) Also, the thesis was baldly stated and inadequately documented, which left it wide open to criticism. For example, Mendenhall had assumed, rather than argued, that 'Hebrew' was synonymous with '*Apiru*, that the latter had the meaning he attributed to it, and that the political upheaval of the Amarna period provided a model for what happened in Canaan in the late 13th century BC. Critics raised objections to all these points, and for at least a decade after its publication Mendenhall's thesis was widely rejected, often after only the briefest discussion.⁷⁷ A notable exception was provided by the second edition of Bright's *A History of Israel*,⁷⁸ which made significant concessions in Mendenhall's direction. However, during the 1970s, as criticism of the methodology of the Baltimore School mounted, Mendenhall's thesis received increasing attention in scholarly debate.⁷⁹ It also attracted proponents who gave it a much stronger basis than Mendenhall had achieved for it in his original article.

The most important new proponent of the theory is N. K. Gottwald, who has developed it along distinctive lines.⁸⁰ He departs from Mendenhall by rejecting the Mosaic covenant and its religious ideology as a significant factor in Israel's emergence from Canaanite society, and, indeed, has expressed strong doubts about the historical value of the

Exodus-Sinai traditions.⁸¹ Also, while Mendenhall depicts the polarization of Canaanite society and the emergence of Israel as happening virtually overnight, Gottwald extends the process, supplementing the 'peasant rebellion' model with one of 'social revolution', the latter 'ebbing and flowing over two centuries'.⁸² Thirdly, Gottwald regards the Iron I settlements in the hill country as the work of rebel groups withdrawing to regions of lowest resistance;⁸³ Mendenhall, on the other hand, believes that the rebels withdrew 'not physically and geographically, but politically and subjectively' from the existing regimes.⁸⁴

Assessment

As noted above, it has been easy for critics to pick holes in Mendenhall's original statement of the peasant revolt theory. The defences and modifications produced by newer proponents such as Gottwald and M. L. Chaney⁸⁵ have removed some of its serious weaknesses, so that criticisms which could be levelled at Mendenhall do not apply to more recent versions of the theory. In the interests of economy, I will mention only those weaknesses which the main versions have in common.

The objection raised earlier to the infiltration theory, that it fails to account for the biblical traditions, is applicable also to the peasant revolt theory. Mendenhall offers the best explanation of how the biblical tradition arose, since he is able to point to a group which actually did come out of Egypt, become a covenant community at Sinai and subsequently migrate to Canaan. In his view, the larger groups which later joined these arrivals identified themselves so fully with the deliverance from Egypt that 'the original historic events with which all groups identified themselves took precedence over and eventually excluded the detailed historical traditions of particular groups who had joined later'.⁸⁶ It must remain a matter of opinion whether Israel's Canaanite origins could have been so totally eclipsed by the notion that it was foreign to the land, but it seems highly unlikely. We must remember that this idea is found not only in the exodus tradition, but also in the stories of the patriarchs.⁸⁷

Gottwald has tried to discover clues within the biblical tradition which indicate that Israel's origins were at least in part indigenous, and he makes ingenious use of such snippets as Joshua 6:25 and 9:3-15.⁸⁸ But this kind of evidence cannot bear the weight which he tries to place on it. All it shows is that, once it had arrived in the land, Israel assimilated certain Canaanite groups. It may, indeed, have absorbed more Canaanite groups than the traditions imply. But that is still a long way short of saying that Israel was in essence autochthonous and originated in a peasant revolt.⁸⁹

The traditions concerning conquered cities have been explained as arising from the rebels' victory over the city-state rulers. For example, Joshua 12 is viewed as a list of those kings whose holdings were successfully wrested from them.⁹⁰ But the theory does not adequately account for those narratives in which entire cities are *destroyed*. It would clearly have been against the rebels' own interests to destroy their own homes once they had liberated them from the city-state rulers.⁹¹

One fact which is extremely damaging for the theory is that there is no scrap of external evidence that a peasants' revolt

took place in the late 13th century BC. There are no texts such as the Amarna letters of the previous century which refer to any such event, and none is attested archaeologically. Indeed, the archaeology of 13th-century Canaan does not support the distinction between a rural peasantry and an urban aristocracy which supporters of the theory presuppose.⁹² The collapse of LBA cities and growth of Iron I villages have been interpreted as evidence of the revolt, but this view faces several serious problems.

First, L. E. Stager has noted that population figures deduced from LBA and Iron I settlement patterns seem to tell against the theory: 'Given the low aggregate of the Late Bronze Age population throughout Canaan, it appears unlikely that the peasantry, even if they had all "revolted", could have been large enough to account for the total Iron Age I village population . . .'.⁹³ Secondly, if the Iron I highland societies were born out of conflict, it is surprising that so many of them were unfortified. This implies that prevailing conditions were peaceful and that no-one contested the occupation of the highlands.⁹⁴ Thirdly, if the Iron I villages mark an attempt to break away from the city-state system, they should all lie some distance from the surviving centres of power. In fact this is not the case. Izbet Sartah lay close to the coastal plain,⁹⁵ and a number of sites (Giloh, Tell et-Ful and Mevasseret Yerushalayim) clustered near Jerusalem, which, according to biblical tradition, remained outside Israelite hands until the time of David (*i.e.* the start of Iron IIA). Fourthly, as we will see below, there are strong reasons to believe that the Iron I villagers had formerly been pastoral nomads rather than farmers from the plains and valleys.

In short, the archaeological evidence suggests that the peasants' revolt theory is not an accurate account of the processes which were taking place in Canaan at the end of the LBA and the start of the Iron Age. Indeed, one of the strongest criticisms levelled at proponents of the theory is that they ignore or misapply archaeological evidence.⁹⁶

2b. Other theories in which Israel is indigenous to Canaan

While Mendenhall's theory, even as modified by others, has not won majority support, his criticisms of traditional assumptions (especially those concerning nomadism) have proved extremely influential. The peasant revolt theory has spawned several other views in which Israel originated within the land of Canaan, so that there is now a general consensus that Israel was autochthonous.⁹⁷

An objection which can be made to all these views is that they share with the peasant revolt theory the same difficulties with respect to the biblical traditions. To avoid becoming repetitious, this will not be pointed out in each of the following cases. They also have another major difficulty in common, which will be discussed after the various views have been outlined.

2bi. Finkelstein's view of Israel's nomadic origins

I. Finkelstein has recently developed a theory,⁹⁸ on the basis of archaeological studies of the Iron I settlement process, which shares certain features with Fritz's 'symbiosis hypothesis' discussed above. Finkelstein agrees with Fritz

that the Iron I settlers were formerly pastoral nomads who had had prolonged contact with LBA Canaanite culture. However, while Fritz retains the notion that these semi-nomads had originally entered the land from elsewhere, Finkelstein rejects it. He admits that early Israel may have had diverse origins, and concedes that there is probably a historical kernel to the tradition of an exodus from Egypt, but he believes that the vast majority of Iron I settlers were indigenous.

His argument in favour of their semi-nomadic status before the beginning of the Iron Age is much more rigorously developed than Fritz's and is worth summarizing briefly. Essentially Finkelstein makes four points in favour of viewing the Iron I settlements as evidence of a sedentarization process.

i. The earliest Iron I villages were established on the central ridge and in the small intermontane valleys – the best areas for grazing and dry farming. Orchard agriculture only came later with the settlement of the western slopes of the Ephraimite hill country. This pattern of settlement indicates that the settlers had a pastoral rather than an urban or rural background.

ii. The layout of many Iron I sites – typically an elliptical plan in which dwellings surround a central space – resembles the layout of the *duwwar* tent encampments of nomadic groups, and is to be seen as a development from them.

iii. Extensive use of subterranean storage silos by the Iron I villagers is a characteristic of nomadic societies settling down, because the most urgent requirement of such societies is storage space for silage.

iv. Simple broadroom houses and pillared, four-room houses are developments from the bedouin-style tent, and thus indicate the nomadic antecedents of the Iron I settlers.

These arguments are not all of equal value, and the last two are particularly open to criticism.⁹⁹ Nevertheless Finkelstein has succeeded in greatly strengthening the case for viewing the Iron I settlements as evidence for the sedentarization of pastoral nomads. He has also made an important contribution by presenting archaeological evidence for the existence of a significant population of pastoral nomads in LBA Palestine. Nomadic groups are notoriously difficult to detect archaeologically, but Finkelstein argues that sanctuaries and cemeteries away from the centres of settled population point to the existence of such groups during the LBA, and he tentatively identifies them with the *shasu/sutu* referred to in LBA texts.¹⁰⁰

Like Fritz, Finkelstein explains points of continuity between the material cultures of LB and Iron I in terms of close contact between these pastoral nomads and Canaanite society during the LBA.¹⁰¹ Also like Fritz, he interprets the sedentarization of pastoral nomads at the start of Iron I as a response to changed economic conditions which attended the collapse of the LBA city-states. He departs from Fritz, however, in arguing that the semi-nomadic groups were indigenous to Canaan.

Finkelstein suggests that their origin should be sought in the deterioration of Palestine's rural and urban systems at the

end of the Middle Bronze Age, *i.e.* in the 16th century BC. As permanent settlements were destroyed or abandoned, a large proportion of the sedentary population became nomadized, and remained so until the process was reversed at the beginning of the Iron Age. While proof of this thesis is lacking, it does explain what became of Palestine's population during the LBA; the sedentary population was drastically reduced at the end of the MBA, but increased again at the start of Iron I, and the suggestion that the 'missing' proportion lived as semi-nomads, sparsely attested by archaeological evidence, makes sense of this situation.¹⁰² It is not, however, the only possible explanation, as will be pointed out below.

Although Finkelstein uses the term 'Israelite' to describe the Iron I settlers, he stresses that in doing so he is simply using it as 'a *terminus technicus* for "hill country people in a process of settling down"'.¹⁰³ In fact he does not believe there was such a thing as Israelite identity as early as the sedentarization stage: 'The formation of the Israelite identity was a long, intricate and complex process which, in our opinion, was completed only at the beginning of the monarchy'.¹⁰⁴ We will see below that this evaluation is unacceptable, even if one shares Finkelstein's disregard for the biblical traditions as a historical source.

2bii. Callaway's theory of displaced populations from the coastal plain

The late J. A. Callaway¹⁰⁵ has put forward arguments *against* semi-nomadic origins for the Iron I settlers, preferring to view them as Canaanite villagers displaced from the coastal plain and the Shephelah.

Referring to the use of household cisterns at highland sites such as Tell en-Nasbeh and Khirbet et-Tell (as many as three to each house at the latter), he points out that their location beneath walls and floors suggests that they were excavated *before* the houses were built, which in turn suggests that the settlers already possessed a working knowledge of household cisterns, and the technology to create them, when they arrived in the highlands. He also points to the ability of the Iron I settlers to create agricultural terraces, making possible the cultivation of the steeply sloping hillsides. He sees this ability as a prerequisite for the colonization of the highlands, and evidence for an agricultural background for the settlers. He also argues that the material remains of the Iron I villages point to a cultural background in the Shephelah and the coastal plain. Ceramic and metal artefacts and inscribed objects are cited in support of these cultural links. In Callaway's view the Iron I settlers in the hills were refugees from population pressure and conflict in the coastal plain and Shephelah, caused by the arrival of the Philistines and other 'Sea Peoples'. These highland settlers eventually emerged as Israel, so that Israel's origins must ultimately be sought in the Canaanite villages of the plains and lowlands. Callaway's view therefore has something in common with the peasant revolt theory, in that the Iron I settlers are presented by both theories as refugees from the lowland districts. But whereas the peasant revolt theory depicts them as escaping from oppressive economic exploitation, Callaway would see them as refugees from aggressive newcomers.

Quite apart from the fact that Callaway's theory runs into many of the same difficulties as the peasant revolt theory, he

has not succeeded in establishing an agricultural background for the Iron I settlers. He has not produced any evidence which cannot be accounted for in terms of close contact over a long period between semi-nomads and the settled Canaanites of the LBA. Cistern technology could have been acquired through such contact; cisterns (even waterproofed with plaster) were used at, *e.g.*, Hazor and Taanach in the LBA. The artefacts and inscribed objects which Callaway cites as evidence of cultural roots in the lowlands could indicate continuing contact with the surviving cities of the coastal plain (through limited trade or transhumance) after the semi-nomads had begun to settle down. The implications of terracing technology for the origin of the highland settlers are at best uncertain. Callaway assumes that the settlers brought the technology with them ready-made,¹⁰⁶ but D. C. Hopkins, in a major study of highland agriculture, takes a different view. He argues that terracing techniques were not an essential prerequisite for cultivation in the highlands, and that it is 'much more cogent' to assume that the techniques were developed by the settlers as they sought to cope with the exigencies of their highland environment.¹⁰⁷

The economy of the Iron I villages at Khirbet et-Tell and Khirbet Raddana, to which Callaway chiefly refers, does not help his case. Animal enclosures and domestic stables clearly indicate the importance of sheep and goats in that economy, while the areas available for cultivation were very limited. Callaway admits this, and estimates that perhaps 'up to one-half of a family's subsistence came from its animals'.¹⁰⁸ Without wishing to play down the importance of dry farming in the economy, we must question Callaway's insistence that these villages 'were established from the beginning on a subsistence base of agriculture primarily and small cattle secondarily'.¹⁰⁹ In short, his view that the settlers were originally agriculturalists rather than shepherds is an assumption, not a conclusion forced upon us by the evidence.

Finally, if the Iron I villagers were refugees from the coastal plain and the Shephelah, moving inland under pressure from the Philistine invasion of those regions, we would expect them to have primarily colonized the highlands of Judah, immediately to the east. In fact, however, early Iron Age settlements were very sparse in that area; only about ten are known, compared with 115 in Ephraim and ninety-five in Manasseh (*i.e.* between Jerusalem and the Jezreel Valley).¹¹⁰ In short, Callaway's theory accounts for only a small part of the archaeological data (which is just as well accounted for in other ways), and is contradicted by the bulk of it.

2biii. Lemche's 'evolutionary Israel'

In the context of a major critique of the peasant revolt theory, N. P. Lemche has put forward his own alternative view of Israelite origins. Rejecting Israel's revolutionary beginnings, he prefers to speak of an 'evolutionary Israel'.¹¹¹

Lemche is even more radical than Mendenhall and Gottwald in breaking with the biblical traditions. He considers the traditions of Israel's early history to be so late in origin as to be useless for historical reconstruction: '... I propose that we decline to be led by the Biblical account and instead regard it, like other legendary materials, as essentially ahistorical, that is, as a source which only exceptionally can be verified by other information.' His alternative reconstruc-

tion is based entirely on what we can deduce from archaeological materials 'of the social, economic, cultural and political developments in Palestine towards the close of the second millennium'.¹¹² Lemche does not wish, however, to limit his investigations to the late 13th century BC, for the Israelite state may have been formed 'as the result of a development which may actually have covered the whole of the Late Bronze Age (or, for that matter, an even longer period of time)'.¹¹³ After surveying the conditions and institutions of the LBA, and examining possible reasons for the economic and cultural decline which marked the end of the period, Lemche offers a 'working hypothesis', which may be summarized as follows:

From at least as early as the first half of the 14th century BC the central highlands were the habitation of the 'Apiru, whom he defines as 'a para-social element . . . [consisting] of runaway former non-free peasants or copyholders from the small city-states in the plains and valleys of Palestine'.¹¹⁴ In other words, they had once been settled agriculturalists. These groups are attested by the Amarna correspondence, but do not otherwise feature in archaeological evidence for that period, because they were not sedentary at that time; they lived as 'outlaw groups of freebooters'.¹¹⁵ However, when new settlements appear in the highlands over a century later, at the start of the Iron Age, they are evidence of new political structures emerging among those same groups. The Iron I settlements attest a return by those groups to a settled, agricultural lifestyle, and the beginning of a (re)tribalization process. Israel was the end-product of that process.

Lemche's view has much in common with Finkelstein's. Both see Israel developing from non-sedentary groups in the highlands, and both believe those groups had formerly been settled. However, they clearly hold different views on the origins and lifestyles of those groups. Lemche also offers a different explanation for their sedentarization. He believes that their return to an agricultural existence was not possible before the late 13th century BC, because agriculture could not be carried out in the highlands without terracing techniques and lined water-cisterns, neither of which, according to Lemche, was developed until that time.¹¹⁶ This argument is completely fallacious, since (in common with Callaway's argument discussed above) it makes wrong assumptions about both these technological innovations. However, the major weaknesses of Lemche's view will be brought out below, after I have outlined one more theory of indigenous origins.

2biv. The synthesis of Coote and Whitelam

The reconstruction put forward by R. B. Coote and K. W. Whitelam is more complex and nuanced than that of either Finkelstein or Lemche. Nevertheless, it has some features in common with both.

Like Lemche, Coote and Whitelam explicitly reject the biblical narratives as a source for the reconstruction of Israel's early history. Rather, the historian's task is 'to explain the archaeological record in the context of comparative history and anthropology'.¹¹⁷ They review an impressive range of evidence in an attempt to fulfil this task, and are to be commended for producing a challenging new synthesis, only one small part of which can be commented on here – that which bears most directly on the nature of Israel's origins.¹¹⁸

After examining the nature and location of the Iron I settlements, they conclude that the archaeological evidence is not compatible with the conquest, infiltration or revolt theories. Rather, the origin of those settlements is to be set in the context of an economic decline which occurred at the end of the LBA, probably resulting from a breakdown of the inter-regional trade on which Canaan's urban economy ultimately depended. This urban economic collapse triggered a number of processes.

As the lowland urban centres declined in material prosperity, a settlement shift occurred among rural groups, including agriculturalists, pastoral nomads and even bandits, all of whom were economically dependent on the lowland cities to some extent. The development of agriculture in the highlands, which had not been economically viable during the LBA, now became an attractive option for such groups. Settlements were founded, based on a mixture of agriculture and pastoralism. Subsequently 'The settlement into villages in the hinterland was given political and incipient ethnic form in the loosely federated people calling themselves Israel'.¹¹⁹

Thus in the reconstruction offered by Coote and Whitelam the ancestors of Israel were not exclusively peasants, bandits or pastoral nomads, but a mixture of all three, thrown together by the seismic effect of the decline in inter-regional trade. The attractiveness of such a broad synthesis is that it avoids the weaknesses of narrower approaches. It seems able to accommodate virtually all the available data (except, of course, for the biblical traditions, which are explicitly rejected on methodological grounds). We will now see, however, that there is at least one piece of evidence which overturns all four of the theories reviewed in this section.

A major criticism of the above theories

All four of the theories outlined above, either explicitly or implicitly, give a relatively late date to the formation of an entity called Israel. Israel did not even *begin* to take shape until a group or groups embarked on the process of settlement in the highlands, as evidenced by archaeology. In other words, there was no 'Israel' before the beginning of Iron I, and perhaps not for some time afterwards.

It is well known that the earliest reference to Israel outside the Bible comes from the late 13th century BC. This occurs in the final stanza of a hymn celebrating the victories of the pharaoh Merenptah (or Merneptah), inscribed on what has become known as the 'Israel stela', dating from Merenptah's fifth year, *i.e.* 1207 BC in the low chronology now preferred for Egypt's 19th Dynasty. It is the relative dating of this reference to Israel and the earliest Iron I settlements which vitiates the above theories of Israel's origins.

Until recently the beginning of the Iron Age has been very loosely defined. It has commonly been assigned a date of around 1200 BC, in recognition that this is merely a handy, round number, and that some Iron I settlements may have appeared earlier and some later. Hence Lemche, discussing the appearance of Iron I villages, is able to say: 'The date of all this activity perhaps falls towards the close of the 13th century; many are in any case from the beginning of the 12th century'.¹²⁰ Coote and Whitelam remark: 'Israel originated during the third and fourth quarters of the thirteenth century

with the shift in land use and settlement patterns of the Palestinian highland and dry land margin.¹²¹ Finkelstein also assigns the beginning of sedentarization in the highlands to the late 13th century BC, though he admits that the data for this are 'few and inconclusive'.¹²² However, it has emerged from B. G. Wood's recent, magisterial study of the LBA/Iron I transition that the beginning of Iron I should actually be dated close to 1170 BC, and not loosely to around 1200 BC as previously supposed.¹²³ This means that we have irrefutable evidence for the existence of Israel some 30-40 years *before* the earliest Iron I settlements which supposedly mark the *beginning* of Israel's formation. Coote and Whitelam may be hoping to head off such a criticism when they suggest that Merenptah's inscription 'may not refer to . . . any social group directly ancestral to monarchic Israel'.¹²⁴ This fantastic statement is clearly special pleading. The evidence of Merenptah's stela cannot be disposed of simply because it does not fit a particular theory of Israelite origins; instead, the theory must be adapted to fit the evidence.¹²⁵

In summary, it is clear from Merenptah's inscription that Israel's origins must be sought before the beginning of Iron I. Other evidence from within the OT itself points to the same conclusion. Kitchen has repeatedly put forward arguments demonstrating that the form of the Sinai covenant must go back to the second millennium BC,¹²⁶ and several scholars have argued for the pre-monarchic origins of much early Hebrew poetry, effectively demonstrating the great antiquity of the historical traditions which it embodies.¹²⁷ The traditions of Israel's early history cannot be cavalierly relegated to an exilic or other late date in the way that Lemche, Coote and Whitelam and others have stated.

Conclusion: scope for an alternative view

Both internal and external evidence requires that we treat with greater respect Israel's traditions concerning her origins and early history. This in turn requires that we look for a historical, archaeological and cultural context in which the traditions concerning the exodus, wilderness wanderings and conquest can retain their integrity. Many evangelical scholars have long believed that the 13th century BC provides such a context. As we noted earlier in this article, it now seems very unlikely that it does. Is there an alternative?

I have argued in detail elsewhere¹²⁸ for a return to the 15th-century date implied by the OT's internal chronology (1 Ki. 6:1; Jdg. 11:26). Some of the reasons for rejecting this date (such as the alleged gap in occupation in Transjordan) have disappeared and others are not so strong as has sometimes been supposed. One major difficulty for the 15th-century date has been the apparent absence of evidence for a violent conquest at the end of that century. In response, I have tried to show that the missing evidence is probably provided by the fall of Canaan's fortified cities at the close of the MBA.¹²⁹ This event has traditionally been dated between 1550 and 1500 BC and attributed to Egyptian campaigns. Arguments against the traditional view are now emerging, lending plausibility to my suggestion that these cities were actually destroyed about a hundred years later, and that their destroyers were the incoming Israelites.¹³⁰ It will be a good while before enough evidence is available for a final verdict (insofar as final verdicts are ever reached in such matters!), but this approach currently seems to be a promising one.

Several of the arguments put forward by the proponents of alternative views actually complement this theory. As noted above, Finkelstein draws attention to the collapse of Canaan's urban culture at the end of the MBA, and to evidence for a significant semi-nomadic population during the LBA. He suggests that the change should be explained in terms of the nomadization of a large proportion of the previously urban population. An alternative hypothesis would be that invaders slaughtered or dispersed a large part of the MBA urban population, and subsequently lived in the land as semi-nomads. For a variety of political and economic reasons, settlement may not have been an attractive option for the newcomers, until, that is, the socio-economic complexion of the country was changed once again by the decline of Canaan's remaining city-states at the end of the LBA. I am suggesting, of course, that the invaders were the Israelites, who certainly arrived in Canaan as tent-dwellers and remained such for an uncertain period after entering the land.¹³¹ This view allows them to be a well-established part of the Canaanite scene by the time of Merenptah, though not archaeologically 'visible' until their sedentarization a few decades later, at the beginning of Iron I.

Obviously, much more would need to be said to fill out and defend this picture, and this is not the place to do it. I am simply trying to show that the biblical traditions are not incompatible with some of the new understandings of Canaan's archaeological, social and economic history.

Whether more evangelical scholars will find this approach attractive remains to be seen. What is certain is that a major rethink is required among defenders of the biblical traditions concerning Israel's origins. The old synthesis of the Baltimore School cannot be kept alive by tinkering with a few details. The real need is for a completely new synthesis which takes account of the best of recent analyses of archaeological and other evidence while refuting the worst.

¹ G. E. Mendenhall, 'The Hebrew Conquest of Palestine', *BA* XXV/3 (1962), pp. 66-87.

² E.g. L. T. Wood, 'The Date of the Exodus', in J. Barton Payne (ed.), *New Perspectives on the Old Testament* (Waco: Word Books, 1970), pp. 67-86; B. K. Waltke, 'Palestinian Artifactual Evidence Supporting the Early Date for the Exodus', *Bibliotheca Sacra* 129 (1972), pp. 33-47; C. F. Aling, *Egypt and Bible History from Earliest Times to 1000 BC* (Grand Rapids: Baker Book House, 1981), pp. 77-96; C. H. Dyer, 'The Date of the Exodus Reexamined', *Bibliotheca Sacra* 140 (1983), pp. 225-243; E. H. Merrill, *Kingdom of Priests* (Grand Rapids: Baker Book House, 1987), pp. 57-75. It has to be said that a number of these defences of the 15th-century date have been seriously flawed (see Bimson, as in following note, pp. 114-119, 250, and my review of Merrill forthcoming in *Christian Arena*).

³ J. J. Bimson, *Redating the Exodus and Conquest* (Sheffield: Almond Press, 1981).

⁴ For detailed references see *ibid.*, pp. 44-46.

⁵ *Ibid.*

⁶ O. Tufnell, 'Lachish', in D. Winton Thomas (ed.), *Archaeology and Old Testament Study* (Oxford: OUP, 1967), p. 302. (Tufnell's view has been vindicated by more recent excavations; see D. Ussishkin cited in n. 40.)

⁷ See conveniently Y. Yadin, 'Hazor', in *ibid.*, p. 260.

⁸ J. Bright, *A History of Israel* (London: SCM, 1960), p. 120.

⁹ G. E. Wright, *Biblical Archaeology* (London: Duckworth, 1962), p. 60. The fact that a different site is now favoured for Raamses has not changed this conclusion; see Kitchen as cited in n. 27.

- ¹⁰ See conveniently N. Glueck, *The Other Side of the Jordan* (Cambridge MA: American Schools of Oriental Research, 1940), pp. 114, 125-147.
- ¹¹ E.g. K. A. Kitchen and T. C. Mitchell, 'Chronology of the Old Testament', in J. D. Douglas (ed.), *New Bible Dictionary* (London: Tyndale Press, 1962), pp. 214-216; revd. edn. (Leicester: IVP, 1982), pp. 191-192; K. A. Kitchen, *Ancient Orient and Old Testament* (London: Tyndale Press, 1966), pp. 57-75; *The Bible in its World* (Exeter: Paternoster Press, 1977), pp. 75-91.
- ¹² E.g. R. K. Harrison, *Introduction to the Old Testament* (London: Tyndale Press, 1970), pp. 174-176, 315-325.
- ¹³ E.g. the works cited in n. 2.
- ¹⁴ K. M. Kenyon, *Digging Up Jericho* (London: Ernest Benn, 1957), pp. 256-263; Kenyon subsequently modified the dates slightly: *Archaeology in the Holy Land* (London: Ernest Benn, 1979), p. 208. For the most recent treatment of the relevant Jericho material see P. Bienkowski, *Jericho in the Late Bronze Age* (Warminster: Aris and Phillips, 1986).
- ¹⁵ J. Marquet-Krause, *Les fouilles de 'Ay (et-Tell) 1933-35* (Paris: Bibliothèque Archéologique et Historique, 1949).
- ¹⁶ E.g. J. A. Callaway, 'New Evidence on the Conquest of Ai', *JBL* 87 (1968), pp. 312-320.
- ¹⁷ J. B. Pritchard, 'Culture and History', in J. P. Hyatt (ed.), *The Bible in Modern Scholarship* (Nashville: Abingdon, 1965), p. 319.
- ¹⁸ *Ibid.*
- ¹⁹ Bright, *History* (London: SCM Press, 1981), p. 130.
- ²⁰ Wright, *Biblical Archaeology*, p. 80; Yadin, 'Is the Biblical Account of the Israelite Conquest of Canaan Historically Reliable?', *BAR* VIII/2 (1982), p. 22.
- ²¹ W. F. Albright, 'The Kyle Memorial Excavation at Bethel', *BASOR* 56 (1934), pp. 3, 11.
- ²² Albright, *The Biblical Period from Abraham to Ezra* (New York: Harper Torchbooks, 1963), p. 30.
- ²³ J. M. Miller, 'Archaeology and the Israelite Conquest of Canaan: Some Methodological Observations', *PEQ* 109 (1977), pp. 87-93; E. F. Campbell and J. M. Miller, 'W. F. Albright and Historical Reconstruction', *BA* 42/1 (1979), pp. 37-46; J. M. Miller, 'Old Testament History and Archaeology', *BA* 50/1 (1987), pp. 57-58.
- ²⁴ Kitchen, *Ancient Orient*, pp. 63-65.
- ²⁵ See Bimson, *Redating*, pp. 188-191.
- ²⁶ C. H. Dyer, 'The Date of the Exodus Reexamined', p. 229.
- ²⁷ Kitchen, *Ancient Orient*, pp. 59-62; Kitchen and Mitchell, 'Old Testament Chronology', *New Bible Dictionary*, p. 191.
- ²⁸ See Bimson, *Redating*, pp. 61-68; R. G. Boling, *The Early Biblical Community in Transjordan* (Sheffield: Almond Press, 1988), pp. 11-35.
- ²⁹ Details in Bimson, 'Exodus and Conquest: Myth or Reality?', *Journal of the Ancient Chronology Forum* 2 (1988), p. 29.
- ³⁰ Kitchen, *Ancient Orient*, pp. 73-74.
- ³¹ Bimson, *Redating*, pp. 82-83; 'Archaeological Data and the Dating of the Patriarchs', in A. R. Millard and D. J. Wiseman (eds.), *Essays on the Patriarchal Narratives* (Leicester: IVP, 1980), pp. 82-83.
- ³² Kitchen, *Ancient Orient*, p. 74.
- ³³ E.g. *ibid.*, pp. 90-102; *The Bible in its World*, pp. 79-85; 'The Fall and Rise of Covenant, Law and Treaty', *Tyndale Bulletin* 40 (1989), pp. 118-135.
- ³⁴ Kitchen's evidence could theoretically be reconciled with a 15th-century date for the exodus by assuming literary activity on the relevant texts during the 13th century BC. That editorial work continued, e.g. on Deuteronomy, after the time of Moses is evident from Dt. 1:1; 2:12; 34:1-12.
- ³⁵ R. Gonen, 'Urban Canaan in the Late Bronze Period', *BASOR* 253 (1984), p. 69.
- ³⁶ Yadin, 'Biblical Archaeology Today: The Archaeological Aspect', in J. Aviram et al., *Biblical Archaeology Today* (Jerusalem: Israel Exploration Society), pp. 23-24.
- ³⁷ B. G. Wood, *Palestinian Pottery of the Late Bronze Age: An Investigation of the Terminal LB IIB Phase* (PhD thesis, University of Toronto, 1985), pp. 369-599; 'The Palestinian Evidence for a Thirteenth Century conquest: An Archaeological Appraisal', paper presented at the symposium *Who Was the Pharaoh of the Exodus?* held in Memphis TN, April 23-25, 1987, publication forthcoming. The absolute dates depend on the chronology adopted for Egypt. I have used the low chronology currently favoured by a number of Egyptologists; see K. A. Kitchen, 'The Basics of Egyptian Chronology in Relation to the Bronze Age', in P. Åström (ed.), *High, Middle or Low?* (Gothenburg: Paul Åströms Förlag, 1987), Pt. 1, pp. 37-55.
- ³⁸ M. Kochavi, 'Khirbet Rabûd = Debir', *Tel Aviv* 1 (1974), p. 2-33.
- ³⁹ D. Livingston, 'Location of Bethel and Ai Reconsidered', *Westminster Theological Journal* 33/1 (1970), pp. 20-44; 'Traditional Site of Bethel Questioned', *WTJ* 34/1 (1971), pp. 39-50.
- ⁴⁰ D. Ussishkin, 'Lachish - Key to the Israelite Conquest of Canaan', *BAR* XIII/1 (1987), pp. 35-39.
- ⁴¹ M. Noth, *The History of Israel* (London: Black, 1960), p. 81.
- ⁴² A. Alt, *Die Landnahme der Israeliten in Palästina* (Leipzig: Reformationsprogramm der Universität Leipzig, 1925). English translation in Alt, *Essays on Old Testament History and Religion* (Oxford: Blackwell, 1966), pp. 135-169.
- ⁴³ See M. Weippert, *The Settlement of the Israelite Tribes in Palestine* (London: SCM Press, 1971), pp. 25-46.
- ⁴⁴ Weippert, *op. cit.*; 'The Israelite "Conquest" and the Evidence from Transjordan', in F. M. Cross (ed.), *Symposia Celebrating the 75th Anniversary of the Founding of the American Schools of Oriental Research (1900-1975)* (Cambridge MA: ASOR, 1979), pp. 15-34.
- ⁴⁵ J. M. Miller, 'The Israelite Occupation of Canaan', in J. H. Hayes and J. M. Miller (eds.), *Israelite and Judaeon History* (London: SCM, 1977), pp. 279-284.
- ⁴⁶ For his final statement of this see Aharoni, *The Archaeology of the Land of Israel* (London: SCM, 1982), pp. 177-180.
- ⁴⁷ M. Kochavi, 'The Israelite Settlement in Canaan in the Light of Archaeological Surveys', in Aviram, *Biblical Archaeology Today*, pp. 54-60; A. Zertal, *The Israelite Settlement in the Hill Country of Manasseh*, PhD thesis, Tel Aviv University, 1986 (Hebrew with English abstract).
- ⁴⁸ See the summary in Weippert, *Settlement*, pp. 37-41.
- ⁴⁹ E.g. C. H. J. De Geus, *The Tribes of Israel* (Amsterdam: Van Gorcum, 1976), pp. 193-209; A. D. H. Mayes, *Israel in the Period of the Judges* (London: SCM, 1974), pp. 15-83, and more briefly in 'The Period of the Judges and the Rise of the Monarchy', in Hayes and Miller, *Israelite and Judaeon History*, pp. 299-308; R. de Vaux, *The Early History of Israel* (London: Darton, Longman & Todd, 1978), vol. 2, pp. 695-715; N. K. Gottwald, *The Tribes of Yahweh: A Sociology of the Religion of Liberated Israel* (London: SCM, 1980), pp. 345-357.
- ⁵⁰ Several scholars have suggested that tribal unity was an achievement of David, but this view raises its own problems; see G. W. Ramsey, *The Quest for the Historical Israel* (London: SCM, 1982), pp. 88-90, and references supplied there. Cf. P. C. Craigie, 'The Conquest and Early Hebrew Poetry', *Tyndale Bulletin* 20 (1969), pp. 76-94, for evidence of unity in the conquest period.
- ⁵¹ J. Bright, *Early Israel in Recent History Writing* (London: SCM, 1956), pp. 90-91.
- ⁵² B. S. Childs, 'A Study of the Formula "Until This Day"', *JBL* 82 (1963), pp. 279-292; 'The etiological tale re-examined', *VT* 24 (1974), pp. 387-397; C. Westermann, 'Arten der Erzählung in der Genesis', in *Forschung am Alten Testament: Gesammelte Studien* (Munich: Chr. Kaiser Verlag, 1964), pp. 40-44.
- ⁵³ Weippert, 'Canaan, Conquest and Settlement of', in K. Crim (ed.), *IDB Supplement Volume* (Nashville: Abingdon, 1976), p. 128.
- ⁵⁴ Weippert, *Settlement*, pp. 36-37.
- ⁵⁵ Weippert, 'Canaan, Conquest and Settlement of', p. 128.
- ⁵⁶ Weippert, *Settlement*, pp. 24-25.
- ⁵⁷ *Ibid.*, p. 13, n. 30.
- ⁵⁸ *Ibid.*, p. 18.
- ⁵⁹ Y. Aharoni, 'Problems of the Israelite Conquest in the Light of Archaeological Discoveries', *Antiquity and Survival* 2 (1957), p. 146.
- ⁶⁰ *Ibid.*, p. 149.
- ⁶¹ L. E. Stager, 'The Archaeology of the Family in Ancient Israel', *BASOR* 260 (1985), p. 3.
- ⁶² The debate is documented by Yadin, 'The Transition from a Semi-Nomadic to a Sedentary Society in the Twelfth Century BCE', in Cross, *Symposia*, pp. 57-68.
- ⁶³ I. Finkelstein, *The Archaeology of the Israelite Settlement* (Jerusalem: Israel Exploration Society, 1988), pp. 97-101, 315-317.
- ⁶⁴ Pritchard, 'Culture and History', pp. 320-321; A. Mazar, 'Israelite Settlement in the Light of Excavations', in Aviram, *Biblical Archaeology Today*, pp. 61-71; V. Fritz, 'Conquest or Settlement?', *BA* 50/2 (1987), pp. 96-97.

- ⁶⁵ Miller in Hayes and Miller, *Israelite and Judaeon History*, p. 270.
- ⁶⁶ Noth, *History*, p. 69.
- ⁶⁷ See especially Gottwald, *Tribes of Yahweh* (see n. 49), pp. 437-448; M. L. Chaney, 'Ancient Palestinian Peasant Movements and the Formation of Premonarchic Israel', in D. N. Freedman and D. F. Graf (eds.), *Palestine in Transition: The Emergence of Ancient Israel* (Sheffield: Almond Press, 1983), pp. 42-43.
- ⁶⁸ E.g. Weippert, 'The Israelite "Conquest" and the Evidence from Transjordan' (see n. 44).
- ⁶⁹ Fritz, 'Conquest or Settlement?', p. 98.
- ⁷⁰ Fritz, 'The Israelite "Conquest" in the Light of Recent Excavations at Khirbet el-Meshāsh', *BASOR* 241 (1981), p. 71.
- ⁷¹ Mendenhall, 'The Hebrew Conquest of Palestine', p. 67.
- ⁷² *Ibid.*, p. 71.
- ⁷³ *Ibid.*
- ⁷⁴ *Ibid.*, p. 73.
- ⁷⁵ *Ibid.*, p. 74.
- ⁷⁶ *Ibid.*, pp. 73, 79.
- ⁷⁷ E.g. R. de Vaux, *The Early History of Israel*, vol. 2, pp. 485-487. (However, note that de Vaux allows social revolt to play a part in his own reconstruction of the settlement of the northern tribes; *ibid.*, pp. 677-678).
- ⁷⁸ Bright, *History* (London: SCM, 1972), pp. 133-139; Mendenhall's theory is adopted even more confidentially in *History* (London: SCM, 1981), pp. 137-143.
- ⁷⁹ See e.g. the papers by A. J. Hauser and T. L. Thompson, with replies by Mendenhall and Gottwald, in *JSOT* 7 (1978), pp. 2-52, and the literature cited therein; also the essays in Freedman and Graf, *Palestine in Transition*, all of which were written during the 1970s.
- ⁸⁰ Most notably in *Tribes of Yahweh* (see n. 49), but also in several essays, some of which are referred to below.
- ⁸¹ Gottwald, 'The Israelite Settlement as a Social Revolutionary Movement', in Aviram, *Biblical Archaeology Today*, p. 35.
- ⁸² *Ibid.*, pp. 37-38.
- ⁸³ Gottwald, 'The Hypothesis of the Revolutionary Origins of Ancient Israel', *JSOT* 7 (1978), pp. 44, 50.
- ⁸⁴ Mendenhall, 'Hebrew Conquest', p. 73. Mendenhall has suggested that the Iron I settlements were the work of newcomers from the north (e.g. Mendenhall, 'Israel's Hyphenated History', in Freedman and Graf, *Palestine in Transition*, pp. 97, 99), a view for which evidence is completely lacking and which currently has no following.
- ⁸⁵ M. L. Chaney, 'Ancient Palestinian Peasant Movements', in Freedman and Graf, *Palestine in Transition*, pp. 39-90.
- ⁸⁶ Mendenhall, 'Hebrew Conquest', p. 74.
- ⁸⁷ Cf. Ramsey, *Quest*, p. 94.
- ⁸⁸ Gottwald, 'The Hypothesis of the Revolutionary Origins of Ancient Israel', pp. 41-42; *Tribes of Yahweh*, pp. 215-216.
- ⁸⁹ Cf. Hauser, 'Israel's Conquest of Palestine: A Peasants' Rebellion?', *JSOT* 7 (1978), p. 10, a point which stands in spite of Gottwald's reply (previous note).
- ⁹⁰ Gottwald, 'The Hypothesis of the Revolutionary Origins of Ancient Israel', p. 41.
- ⁹¹ Cf. E. F. Campbell, 'Moses and the Foundations of Israel', *Interpretation* 29 (1975), p. 152.
- ⁹² T. L. Thompson, 'Historical Notes on [Hauser's] "Israel's Conquest of Palestine: A Peasants' Rebellion?"', *JSOT* 7 (1978), pp. 24-25; Finkelstein, *Archaeology of the Israelite Settlement*, p. 311.
- ⁹³ L. E. Stager, responding to Gottwald, in Aviram, *Biblical Archaeology Today*, p. 84.
- ⁹⁴ J. A. Callaway, *ibid.*, p. 75; R. B. Coote and K. W. Whitelam, *The Emergence of Early Israel in Historical Perspective* (Sheffield: Almond Press, 1987), p. 135; Finkelstein, *Archaeology of the Israelite Settlement*, p. 313.
- ⁹⁵ Cf. Coote and Whitelam, *Emergence of Early Israel*, p. 127.
- ⁹⁶ Finkelstein, *Archaeology of the Israelite Settlement*, p. 308.
- ⁹⁷ In addition to those scholars whose views are discussed below, see G. W. Ahlström, *Who Were the Israelites?* (Winona Lake: Eisenbrauns, 1986).
- ⁹⁸ Developed and defended most fully in Finkelstein, *Archaeology of the Israelite Settlement*.
- ⁹⁹ See the review of Finkelstein's book by D. Esse, *BAR* XIV/5 (1988), p. 10.
- ¹⁰⁰ Finkelstein, *Archaeology of the Israelite Settlement*, pp. 343-345.
- ¹⁰¹ *Ibid.*, p. 344, though Finkelstein greatly plays down the extent of this continuity (cf. *ibid.*, p. 312).
- ¹⁰² *Ibid.*, pp. 339-348.
- ¹⁰³ *Ibid.*, p. 28.
- ¹⁰⁴ *Ibid.*, p. 27.
- ¹⁰⁵ J. A. Callaway, 'A New Perspective on the Hill Country Settlement of Canaan in Iron Age I', in J. N. Tubb (ed.), *Palestine in the Bronze and Iron Ages: Papers in Honour of Olga Tufnell* (London: Institute of Archaeology, 1985), pp. 31-49; also (in slightly different form) in Aviram, *Biblical Archaeology Today*, pp. 72-77.
- ¹⁰⁶ A view also held by G. W. Ahlström, 'Where Did the Israelites Live?', *JNES* 41 (1982), pp. 133-138.
- ¹⁰⁷ D. C. Hopkins, *The Highlands of Canaan* (Sheffield: Almond Press, 1985), pp. 179-184; cf. Finkelstein, *Archaeology of the Israelite Settlement*, p. 309.
- ¹⁰⁸ Callaway in Aviram, *Biblical Archaeology Today*, p. 75.
- ¹⁰⁹ *Ibid.*, p. 73.
- ¹¹⁰ Finkelstein, *Archaeology of the Israelite Settlement*, p. 311.
- ¹¹¹ N. P. Lemche, *Early Israel* (= *VT Supp.* 37) (Leiden: E. J. Brill, 1985), pp. 411-435; Lemche also puts forward his theory in a more popular form in his *Ancient Israel: A New History of Israelite Society* (Sheffield: JSOT Press, 1988), pp. 85-90, 100-102.
- ¹¹² *Early Israel*, pp. 415-416.
- ¹¹³ *Ibid.*, p. 416.
- ¹¹⁴ *Ibid.*, p. 427.
- ¹¹⁵ *Ibid.*, p. 428.
- ¹¹⁶ *Ibid.*, p. 428-429.
- ¹¹⁷ K. W. Whitelam, 'Recreating the History of Israel', *JSOT* 35 (1986), p. 60.
- ¹¹⁸ Coote and Whitelam, *The Emergence of Early Israel*, pp. 117-138.
- ¹¹⁹ *Ibid.*, p. 136.
- ¹²⁰ Lemche, *Early Israel*, p. 393.
- ¹²¹ Coote and Whitelam, *The Emergence of Early Israel*, p. 117.
- ¹²² Finkelstein, *Archaeology of the Israelite Settlement*, p. 321.
- ¹²³ B. G. Wood, *Palestinian Pottery of the Late Bronze Age* (above, n. 37), pp. 549-599. This date relies on the low chronology for Egypt which has been employed throughout this article (see n. 37). A higher chronology would raise this date, but would also raise the date of Merenptah's reference to Israel, so their relative dating would remain the same. See also D. Ussishkin, 'Level VII and VI at Tel Lachish and the End of the Late Bronze Age in Canaan', in the Tufnell Festschrift (see n. 105), pp. 225-226, for 1150 BC as the suggested date for the start of Iron I.
- ¹²⁴ Coote and Whitelam, *The Emergence of Early Israel*, p. 179, n. 3.
- ¹²⁵ The claim of Ahlström (*Who Were the Israelites?*, p. 40), that Merenptah's stela could refer to a territory called Israel, lies in the face of the inscription itself, which defines Israel by means of the determinative for a foreign people.
- ¹²⁶ See works cited in n. 33.
- ¹²⁷ E.g. Craigie, 'The Conquest and Early Hebrew Poetry' (see n. 50); D. N. Freedman, 'Early Israelite Poetry and Historical Reconstructions', in Cross, *Symposia*, pp. 89-96; B. Halpern, *The Emergence of Israel in Canaan* (Chico: Scholars Press, 1983), pp. 32-43, 117-133, 146-163.
- ¹²⁸ Bimson, *Redating*.
- ¹²⁹ *Ibid.*, pp. 106-223.
- ¹³⁰ Summarized most recently in Bimson, 'Exodus and Conquest: Myth or Reality' (see n. 29), pp. 34-36.
- ¹³¹ Cf. Jos. 3:14; 7:21-24; 22:4-8; etc.; on Israel's mode of life in the period of the judges, see Halpern, *The Emergence of Israel*, pp. 210-211.

Prosperity theology and the faith movement

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Several years ago, the author's thesis 'Christian Faith and Company Culture' was criticized for not dealing with the argument that all faithful Christians should automatically prosper as of divine right. At the time I was barely aware that such an attitude existed, but I thought it significant that the secular examiners were in a position to be able to hold it against me. Since then, however, this teaching has become well known in Britain, and together with its related dogmas, forms a corpus of beliefs which is increasingly taught and accepted around the world. Its birthplace is the United States of America, and the spread around the world seems to have been effected via two distinct routes. On the one hand, there are individuals who have travelled from America to establish new churches which preach this distinctive gospel. On the other hand, this gospel has been adopted by some established churches of the charismatic disposition.

It is the purpose of this article to trace the origins of this expanding world-wide movement and to deal with some of its tenets. I choose to call this particular corpus of beliefs *prosperity theology*, and the movement which adheres to it the *faith movement*. Neither of these titles is original to me, nor are they the only ones which are used, but what I mean by them should become apparent.

The origins of prosperity theology

An American, Daniel McConnell, has conducted a piece of research which is extremely illuminating in establishing the origins of prosperity theology.¹ His first move is to establish Kenneth E. Hagin as the father of the faith movement. Kenneth Hagin Jr. is quoted as writing of his father: 'Almost every major faith ministry of the United States has been influenced by his ministry.' Then, from correspondence with the major leaders within the movement, McConnell shows that Hagan Jr. is not merely boosting his father's ego. These leaders do in fact openly acknowledge Hagin as variously the human source of their inspiration, the fount of their teaching, and their spiritual mentor. These men include Kenneth Copeland, Frederick Price and Charlie Capps.²

But, if Hagin is the father of the faith movement, then he is not the author of its teaching. Hagin's claim that the new teaching was given to him personally by Jesus through a series of divine visitations during the 1950s does not match the evidence uncovered by McConnell. He places side by side several passages from the works of both Hagin and a man called Essek W. Kenyon, and the overwhelming conclusion is that Hagin has directly plagiarized Kenyon. The word-for-word uniformity of the two men is beyond the bounds of coincidence, and McConnell writes that the passages he cites are merely representative ones drawn from just eight books: 'Many more could be cited'. . . .³ All of Hagin's work post-

dates Kenyon's, who in fact died in 1948. Therefore McConnell sums up:

Whereas Hagin appears to have copied only occasionally from sources other than Kenyon, he has plagiarized Kenyon both repeatedly and extensively. Actually, it would not be overstated to say that the very doctrines that have made Kenneth Hagin and the Faith movement such a distinctive and powerful force within the independent charismatic movement are all plagiarized from E. W. Kenyon.⁴

Having isolated Kenyon as the source of prosperity theology, McConnell has one final surprise up his sleeve — that E. W. Kenyon was not a Pentecostal. Even though he may have influenced many of the post-war Pentecostal healers, the dominating influence on his theology is in fact the metaphysical cults which abounded at the turn of the century. He actually wrote that the Pentecostal movement was as destructive as it was instructive. Kenyon attended the Emerson College of Oratory in Boston, Mass., during the last decade of the nineteenth century, a college which was at the time immersed in the metaphysical cults, and the underlying *New Thought*. The influence of the metaphysical cults is clearly visible in his work, and while he claims to remain resolutely Christian, and indeed explicitly refutes elements of the metaphysical cults, yet he simultaneously, often in the same breath as his rebuke, asserts the foundational beliefs of these cults.⁵ Ern Baxter remembers that Kenyon spoke very positively of *Key to the Scriptures* by Mary Baker Eddy (the mother of Christian Science), claiming that there was a lot that could be learnt from her.⁶

It is clear from merely the titles of Kenyon's books that his was a polemical aim directed against the established churches with whom he had become disillusioned. *The Two Kinds of Life, The Two Kinds of Righteousness, The Two Kinds of Knowledge, The Two Kinds of Faith, The New Kind of Love*, and so on, all express his desire to correct what he saw as being awry in the church of his day. Living at a time when the metaphysical cults were growing rapidly, this was Kenyon's 'Christian' response — a 'Christianized' metaphysical cult. The mainline churches were failing because they produced no signs and wonders and Kenyon was keen to redress an anti-supernatural tendency which was driving bored Christians into joining such people as Mrs Baker Eddy. He sought to establish a teaching which provided Christians with all the benefits of the metaphysical cults, while remaining within the Christian fold. The result was prosperity theology, which is, with a very few embellishments, the theology of the present-day faith movement.

In spite of this, McConnell is wrong to ignore other influences on the faith movement. For example, Kenyon rejected tongues as being altogether too subjective an experience,⁷ while for the faith movement, speaking in tongues is a necessary sign that one has been *baptised in the Spirit*. Charismatic Pentecostalism has also left its mark, especially so because such are the roots of many of those in

the faith movement today. Thus, while the doctrines are undoubtedly those of Kenyon, very often the practices are those of the charismatic Pentecostals.

Attitudes to wealth

We need to realize that prosperity is the will of God. It is God's perfect will that everyone prosper in every area of life. Primarily, we are dealing with material and financial prosperity, because it has to do with tithes and offerings.⁸

When the subject of prosperity theology is broached, the immediate aspect of its teaching which springs to mind is God's guarantee of material wealth to *all* believers. But the question raised by the above statement is whether God does wish this financial prosperity on *all* believers. Conservative theologians would concur that the answer to this is no, and yet Price and his colleagues base their statements solely on biblical exegesis, and their efforts can be impressively convincing.

From the OT, Price takes the life of Abraham and shows how God made him *very* rich because he obeyed him (Gn. 13:2).⁹ Further, Abraham is more than a mere example because he is more than a mere man — he is the father of the faithful for he was justified by his faith. He thus becomes the representative of the faithful, demonstrating in his life the role of faith and the rewards for living a life of faithful obedience to the Word of God. That Abraham's wealth is material is revealed in Genesis 13:2: 'Abram had become very wealthy in livestock and in silver and gold.' The source of this wealth is described by Abraham's servant to Laban in Genesis 24:35: 'The Lord has blessed my master abundantly, and he has become wealthy. He has given him sheep and cattle, silver and gold, menservants and maidservants, and camels and donkeys.' God is no respecter of persons, continues Price, and so if a man is willing faithfully to obey God as Abraham did, he will be made materially prosperous just as Abraham was.

Deuteronomy also provides important passages for the prosperity theologians. Both Norval Hayes and Peter Gaunt draw great encouragement from Deuteronomy 8:18: 'But remember the Lord your God, for it is he who gives you the ability to produce wealth, and so confirms his covenant, which he swore to your forefathers, as it is today.'¹⁰ Deuteronomy contains the terms of the old covenant with Israel, in which she was promised, amongst other things, material prosperity in return for loyalty. Chapter 28 puts this over very succinctly and is referred to regularly, and Gaunt for one quotes at length from it. Stephen Matthew sums up this approach to the covenant when he quotes Deuteronomy 29:9: 'Carefully follow the terms of this covenant, so you may prosper in everything you do.'¹¹

Another important passage is Malachi 3 which deals with the payment of the tithe, and from which are extracted the rules governing prosperity. Their exegesis of this passage involves the confident expectation that those who present their tithe will receive back from God more than they gave in the first place. Further, 3:6 says that the Lord does not change and therefore NT passages like Mark 10:30, Luke 6:38, and 2 Corinthians 9:6-11 are seen to reinforce this fact. They argue that those who don't receive the bountiful outpouring of the floodgates of heaven are either ignorant of what is available, or lack the faith to claim it successfully.

The Psalms and Proverbs contain passages which also promise material prosperity to the faithful people of God. In return for, variously, obedience to God, fear of God, hard work, and generosity of spirit, prosperity is to be found — see e.g. Psalms 1:2-3; 25:13; 112:1-3; 128:1-4; Proverbs 13:21; 16:20; 21:5. The result of this and the rest of their interpretation of the OT, of which this has been a very truncated account, is the conclusion that God rewards all faithful Christians with material wealth.

Their NT exegesis also makes the same point but it does have a tendency to be over-literalistic at times. Such an approach is freely admitted and indeed justified by S. Matthew who writes:

At the start of this lesson, we posed the question: could we expect God to meet our every need on the strength of isolated texts like Philippians 4:19? Our answer must be *yes*, provided we are putting our faith to work.¹²

Gloria Copeland therefore takes Mark 10:30 and writes:

Give one house and receive one hundred houses or one house worth one hundred times as much. Give one airplane and receive one hundred times the value of the airplane. Give one car and the return would furnish you a lifetime of cars. In short Mark 10:30 is a very good deal.¹³

That this interpretation cannot withstand scrutiny is plainly obvious since Levi did not become phenomenally rich, nor did James and John receive a hundred fishing boats, or one boat worth a hundred times as much. A closer examination of the passage seems to suggest that the rewards for sacrifices made for the sake of the gospel are realized in the common life of the church. Just as one's family is multiplied, one assumes not literally but rather through entry into the new family of God, so one's possessions are likewise multiplied as this new family holds all things in common.

Another new and interesting piece of exegesis concerns the person of Jesus Christ. Because God wants all Christians to be rich and Christ-like at the same time, it follows that Jesus himself must have been rich. Thus Price writes:

Jesus must have had plenty. He was never caught short. He was responsible for feeding five thousand people at one time. Remember, He had a staff of twelve men who walked with Him every day. They did not work on any job that we have any record of for three and a half years. For that time, He took care of all their transportation, food, lodging, and clothing. He must have had something, somewhere, somehow, or He could not have had a staff of twelve.¹⁴

This is in stark contrast to the picture that Jesus paints of himself in Matthew 8:20: 'Foxes have holes and birds of the air have nests, but the Son of Man has nowhere to lay his head.' Also, if the gospel involves the endowment of wealth, why did Jesus send out his disciples without money or spare clothes, and with instructions to depend on the charity of others (Mt. 10:9f.)? There is also some doubt as to whether it is right to refer to the disciples as 'staff', and whether Jesus was responsible for their physical well-being. Finally, I would love to ask Price what he thinks was needed to take care of 'transportation' in first-century Palestine.

However, as mentioned above, there are passages in the NT which do support the claim of the faith movement, namely that if you give, then you will receive more than your

gift in return. What is crucial however is the motivation of the giver, and not the certainty of material abundance by way of divine reward. The prosperity theologian believes that God makes his people rich so that they can give away lots of money; after all, God cannot ask a man to give £500 if he has not got it. This is surely a sound statement on the basis of 2 Corinthians 9:6-11, but there are aspects of the faith movement's teaching on this subject which are not so certain. One of the first things that is encountered when reading their books on money is the statement that God wants *all* Christians to be rich, and the reason for this is not only so that it can be given away. Price believes that God also wants Christians to enjoy being wealthy:

Yet by walking in God's financial plan, you can have the \$15,000 automobile, wear the \$300 suit, and buy the \$100 designer shoes. God does not care. He wants His kids to look good.¹⁵

Michael Bassett takes this one step further and says:

If you want a nice watch, why don't you give one away and be expectant for your nice one to come in? If you want a nice car, why don't you sow for one, then you can be expectant for a new car?¹⁶

Sacrificial giving is not a concept that they seem to embrace: 'you cannot give away much when your own needs are not met. You cannot do it when you are struggling yourself to pay your electric bill.'¹⁷ Wilful poverty is seen as a 'denial of all that Christ has won through his death', and the ignorance of this fact which prevents financial endowment, as tragic.¹⁸ Surely this is not what is implied by either Jesus in Luke 6 or Paul in 2 Corinthians 9. How can such an interpretation of Luke 6:38 be consistent with the beatitudes earlier on in the chapter, or Jesus' pity for the rich young man of Mark 10:17ff.? Also Paul, in the words of C. K. Barrett, 'is not so crude a thinker as to mean that the Christian is always assured of such a material standard of living that he will be able to act charitably towards others – he knew in his own experience (2 Cor. 11:23-33; cf. Phil. 4:12) that this was not true'.¹⁹

Thus, it can be powerfully argued that God does not wish all Christians to be materially rich, and yet it is an oft-observed fact that when a poor person becomes a follower of Christ, his poverty disappears. John Wesley wrote:

I do not see how it is possible in the nature of things for any revival of religion to continue for long. For religion must necessarily produce both industry and frugality, and these cannot but produce riches. But as riches increase, so will pride, anger, and love of the world in all its branches.²⁰

Wesley's solution was to give away as much as possible and he himself did precisely that, but the burning issue is which item of expenditure is a justifiable necessity and which is a luxury? Wesley's policy in this matter was to avoid raising his standard of living and giving away his ever-increasing excess of income. This is surely more in keeping with Jesus' teaching than the teaching of the prosperity theologians whose chapter titles reflect the nature of their message: Steps to Prosperity, Avoiding Poverty Traps, Heavenly Banking with Tithes and Offerings, 20% – The Penalty For Robbing God, Deposits and Withdrawals, Prosperity NOW!

Another subtle variation in the teaching of the faith movement on giving is that whereas the Bible seems to lay an emphasis on charitable giving, it lays the emphasis on giving to finance mission. Gaunt says:

The needs today are tremendous. We're talking in millions and millions of pounds for the spreading of the good news of Jesus, and satellite television is just one aspect . . . so millions of pounds are going to be needed to see that the earth shall be filled with the knowledge of the glory of God.²¹

S. Matthew completes the argument:

Our aim is to be like Christ, to spread the gospel, to share his love with our needy world in a demonstration of power. . . . Our financial prosperity will finance its happening. . . . Money is needed to make it happen.²²

Thus, the gospel requires very large amounts of financial backing for it to be effective in drawing people to Christ, a stance which is controversial to say the least. Although Paul insisted that the full-time Christian worker was entitled to be paid by those whom he served, the only time that he actually made a collection was for alms and not evangelism, which as Paul and the apostles demonstrated, requires an active Spirit working through willing servants rather than 'big bucks'.

What therefore are we to make of the OT, and the fact that it seems to contain adequate grounds for arguing that God does want all Christians to be rich? In the first place, the OT must be used with extreme caution because, as Deuteronomy 8:18 says, the promises contained therein refer to the terms of the old covenant which was made with the sons of Abraham at a time when such sonship depended on physical descent. In other words, the covenant people of God in the OT are a national entity dwelling within geographical and political boundaries. This fact is fundamental to the understanding of the OT, and Deuteronomy in particular. The relationship between wealth and God's blessing was radically altered when the new covenant came into force with Jesus Christ, and to be a son of Abraham now involves not blood descent but rather having the faith which Abraham had. No longer are the people of the covenant distinguishable by nationality, and this renders inappropriate many of the promises made to the state of Israel, just as it does to many of the regulations laid down for her way of life.

Jacques Ellul notes that in the majority of OT references to the rich, they are found to be under God's condemnation. Therefore he concludes that wealth was not a blessing in itself, but rather a tangible sign of the blessing, and as such, a sacrament *which was apt for four reasons*:

First, it implied the freedom of election.

Secondly, the fact that wealth is used as the sign implies that the grace which brought about this election is abundant and that, not content to restrict himself to the provision of daily bread, God gives wealth which gives rise to luxury, comfort, and ease.

Thirdly, it has a role in the final judgment. In Isaiah 60:4ff. (and Rev. 21:24-26), all the riches of the earth are to be brought into the heavenly Jerusalem. Thus, human accomplishment has a place in the final eschatological plan.

Fourthly, the glory of the heavenly Jerusalem is to be present in our midst through this wealth. This also acts as a reminder that all a man's work and the totality of human power belong to God.²³ Ellul however believes that wealth was not a permanent sacrament and continues:

Wealth, well suited to bringing the gift of the Promised Land to mind, is certainly not suited to reminding us of the gift of the Child in a manger. It is not an adequate sign; therefore we find it stripped of its true value. God thus puts an end to the sign's ambiguity. Wealth is no longer a sacrament because 'God chose what is weak in the world to shame the strong' (1 Cor. 1:27). In Christ God chooses that which has no intrinsic value and makes it adequate to the work he is undertaking.²⁴

While some of the points that Ellul raises are not without their problems, his actual conclusion is matched by that of Thomas Schmidt who avoids the sacramental language of Ellul and introduces the means of acquisition as being important.

The OT declares that wealth is a confirmation of God's covenant with his people, a reward for keeping the terms of his covenant. Among the stipulations of the covenant are the justice imperative and the demand that man acknowledge God as the sole source of prosperity. The OT devalues wealth accrued in violation of these stipulations.²⁵

In fact if 'Bible' were substituted for 'OT', this statement would not be out of place in a prosperity theology publication. But Schmidt believes, as Ellul does, that Jesus changed all this. Schmidt's thesis is based on the synoptic gospels and he is struck by the repeated incidence of Jesus' followers either giving up everything to follow him, or being instructed to do so. In Mark for example, 'the commands in 8:34; 10:21, the statements in 10:28; 12:44, and the narrative accounts in 1:16, 18; 2:14 reveal a patterned or formulaic theological devaluation of wealth'.²⁶ It is impossible here to go into the details of Schmidt's work, but its well argued conclusion is that . . .

'hostility to wealth exists independently of socio-economic circumstances as a fundamental religious-ethical tenet consistently expressed in the Synoptic Gospels'.²⁷

The NT's account of the teaching of Jesus simply does not support the contention that God wants all his children to be materially rich, since such wealth would then be a blessing. This is not consistent with Jesus who is to be found stating, both explicitly and implicitly through parables, that the kingdom of heaven is for the poor and that there is no future comfort for the rich who have apparently already received their comfort (e.g. Lk. 6:24). The parable of the rich man and Lazarus never actually states that the rich man was unrighteous, but nevertheless he is to be found in a reversed situation subsequent to his death (Lk. 16:19-31). It is also interesting to note that in the parable of the wedding banquet, the Jews are represented by the wealthy, and the Gentiles by the poor (Mt. 22:1-14).

Teaching on healing

When the Bible talks about suffering, that doesn't mean 'sickness'. We have no business suffering sickness and disease, because Jesus redeemed us from that.²⁸

The grounds for believing this particular 'truth' fall, broadly speaking, into three categories – Jesus' example, the recorded activities of the apostles, and the substance of the atonement.

(i) Jesus' teaching and example

The reason why all those who pray faithfully for healing will receive it is because Jesus in his earthly life healed all those who came to him to be healed. Matthew 4:23 demonstrates

that Jesus healed all manner of diseases. Mark 1:32 further elaborates this truth, so making this divine healing available to anyone who would with 'faith' ask for it. This holds good today because after all, Jesus is the same yesterday, today, and forever.

This is a strong argument and yet it is not without its problems. Are we to believe that at the pool of Bethesda (Jn. 5:1-9), none of the other infirmed who gathered around it asked Jesus to heal them? Perhaps it is just conceivable that John did not bother to mention that Jesus healed them, but highly unlikely given the rest of the pericope (10-15).

(ii) Apostolic healing activities

After his resurrection, Jesus sent out his disciples to preach the gospel, and he endowed them with the power to heal. In Acts 3:1-8 Luke tells of Peter and John healing the cripple at the Temple gate. Further on, attention is drawn to the apostles performing miraculous signs and wonders, healing many (5:12-16). Philip did likewise in Samaria (8:4-8), and finally Paul is used by God to the extent that everyday articles that had come into contact with him could heal people (19:11f.).

It is clear, though, that sometimes those prayed for were not healed. There is the case of Timothy who, in 1 Timothy 5:23, is instructed by Paul to take a little wine for his stomach condition. Then in 2 Timothy 4:20, Trophimus is described as having been left sick in Miletus. There is even Paul himself, who only preached to the Galatians in the first place because he was ill (Gal. 4:13f.).

(iii) Healing as a consequence of the atonement

This argument starts with Isaiah 53:4ff.: 'Surely he took up our infirmities and carried our sorrows . . . and by his wounds we are healed', and then calls upon Matthew 8:16f. to show how this prophecy was fulfilled in the person of Jesus: 'When evening came, many who were demon-possessed were brought to him, and he drove out the spirits with a word and healed the sick. This was to fulfil what was spoken through the prophet Isaiah: "He took up our infirmities and carried our diseases."' This latter passage demonstrates that healing is part of the work of the atonement carried out by Jesus Christ upon the cross and this fact is central to the faith movement for which healing is as automatic as the forgiveness of sins.

John Wimber, in his book *Power Healing*, includes a chapter that seeks to explain why divine healing is not always granted. In it, he usefully lists a number of theologians who disagree on whether healing is in the atonement.²⁹ If it is accepted that it is not in the atonement, then we need go no further in refuting the above statement. However, if it is included in the atonement, and I believe that it is, why is healing not as automatic as the forgiveness of sins? Donald Carson finds Matthew 8:16f. as convincing as the prosperity theologians on this matter, but he writes that:

From the perspective of the NT writers, the Cross is the basis for all benefits that accrue to believers; but this does not mean that all such benefits can be secured at the present time on demand, any more than we have the right and power to demand our resurrection bodies.³⁰

Hence, though forgiveness is instant in the present time, the fact that healing is not always granted can be explained in that

while we are the new covenant people, yet we possess earthly and sinful bodies. When healing does take place, it is a reminder that Christians are a part of the new kingdom, it is a sign that God is supreme, and it is a portent of things to come. When healing does not take place, it shows that the new kingdom will not finally arrive until the second coming, and that until then, even Christians must face having to live in the domain of the evil one.

In the meantime, the faith movement has to deny that sickness can lead to premature death, and Price elaborates on life expectancy in a startling manner. First he quotes Luke 7:11-16, the raising of the widow's son. He continues:

This young man was *too young to die*. Do you know that in the accounts that we have in the Bible of Jesus raising the dead *He always raised young people*? Did you ever think about that?

Your minimum days should be seventy years, that's just the bare minimum. You ought to live to be at least 120 years of age. That's the Bible. God out of His mouth — in the Old Testament — said the number of your days shall be 120 years.

I didn't write it!

God said it. The minimum ought to be 70 years, and you shouldn't go out with sickness or disease then.³¹

The dualistic foundation of prosperity theology

Underlying prosperity theology is a dualism which has hovered around Christianity from the very beginning. This involves a belief in two mutually exclusive realms — the spiritual and the material. The former is the superior one, it is under the governance of the supreme God, and is the proper domain for people. The latter is ruled over by its own god, and is in eternal conflict with the former. All people start in the material realm, and the common aim in life is to aspire to the spiritual one, an aspiration that can only be satisfied with the aid of some outside agent from the spiritual realm. This scheme of things was adopted by a group of Greek sects in the first few centuries AD, many of whom took Jesus Christ as the outside agent of salvation. They were strongly rebutted by the early church and became known by the title *Gnostic*. The faith movement today stands condemned by many of being Gnostic due to its dualism,³² but whether a dualistic foundation is enough to make this charge stick is debatable, and the outworkings of doctrine do have some wide divergences, not least in the attitude to material things. Nevertheless, there are some marked similarities between the two.

Kenyon is thoroughly dualistic in his separation of revelation knowledge and sense knowledge.³³ The former is to do with the spirit and comes from the Spirit of God. The latter is worldly and is not only inferior to, but inhibits the development of the former. Sense knowledge comes from the five senses and so is limited to the physical environment. It is totally opposed to the things taught by revelation knowledge which is to be gleaned from the Bible. Kenyon displays a marked similarity to a second-century Gnostic named Marcion who championed Paul while omitting Matthew, Mark and John from his canon. Kenyon argues that revelation knowledge came only with the writings of Paul, and he denies that Peter and John knew the full details of eternal life.³⁴

Price echoes Kenyon when he too speaks of two kinds of knowledge — faith knowledge and sense knowledge — which clash most prominently in matters of healing. 'Remember,' writes Price, 'Satan is the God of the world, which includes everything in the sense realm. If you allow your faith to be affected by your senses, you will be defeated in every encounter of life.'³⁵ Here is surely a hint of the demiurge of the ancient Gnostics. Satan is indeed the prince of the world but he is not its God. He did not create it, he simply enslaved it. He rules the world, but only for as long as God chooses to let him. He is not God of the world, he does not order the seasons neither does he have control over the rain which the Lord makes to fall on the righteous and the unrighteous alike. He is the polluter, not the fount of goodness. Christians have long been convinced that the hand of God can be seen in creation and many find what is officially termed *natural theology* to be a valuable aid to faith, revealing the glory of God through his handiwork.

The ancient Gnostics believed that man was spirit trapped in a physical body and their latter-day counterparts are no different. 'You are spirit but you live in a physical body,' writes Price,³⁶ and Hagin concurs:

The real man is the spirit. The spirit operates through the soul: the intellect, sensibilities, and will. And the soul operates through the body. The real you (your spirit) and your soul live in a physical body.³⁷

The implications of such dualism can be frightening; in the case of divine healing they can be fatal. Because all diseases are physical manifestations of spiritual ailments, the former can remain after the latter have been dealt with — a deception of the devil so to speak and therefore to be denied. Price writes:

Remember you are *healed by faith*, and not by sight. *Faith is the evidence of healing — not the fact that the cancer has left your body. Your confession, between the time that you pray and claim your healing until it is seen in your body, is what causes it to come. . . .* Now your body may *scream louder than ever* that you are sick. Fever, pain, nausea, lumps, etc. This is where your *confession* comes in. **YOU MUST CONFESS THE WORD OF GOD IN THE FACE OF EVERY SYMPTOM AND EVERY PAIN.** This is *faith* versus *sense knowledge*.³⁸

Bryn Jones writes in a similar vein . . .

Someone may feel ill, seek and receive prayer for healing, but because they still feel ill conclude that God has not worked on their behalf. Faith is not acting in conjunction with feeling. Faith *acts in obedience* to what God has said.³⁹

Denying physical symptoms in the belief that this will demonstrate the faith which in turn works the healing can be deadly. Andrew Brandon tells of the tragic and unnecessary death of a Cornish pastor who delayed seeking medical attention for an ailment which is only curable if treated during its early stages — he died!⁴⁰ There is also the chilling story of Larry Parker in *We Let Our Son Die* (Harvest House, 1980), who withheld insulin from his diabetic child after he had been prayed for by a faith evangelist. He and his wife even believed that it was merely Satan's deception when Wesley, the son, died, and they prayed fervently for his resurrection for over a year. They were convicted of child abuse and involuntary manslaughter.

Another consequence of this dualism is a belief in the nature of the Christian who, once baptized in the Spirit, becomes a fundamentally superior being since he takes on board the nature of God. The worrying aspect of this is that it ushers in a grading of Christians. Kenyon talks of Christians who have sold out to sense knowledge, and those who base their lives on revelation knowledge; both are saved, but the latter are superior — a clearly divisive dogma.

At first sight, because this process is said to involve the 'baptism of the Spirit', the heresy is shrouded because many Christians also believe in the baptism of the Spirit. But Paul Scanlon's *School of the Word* study on *Living in the Anointing* raises the veil. Like many Pentecostals, he believes that all 'baptised in the Spirit' believers must speak in tongues to verify their baptism, but more importantly, he argues that such a believer also receives divine power on a permanent basis: 'It is the difference between my allowing you to borrow my car occasionally and my giving you the keys to the same car and saying: "Use it any time you like."' ⁴¹ Thus, we have the creation of the 'super-Christian' who is in all aspects (especially the tangible ones) superior to his non-tongue-speaking, non-miracle-performing counterpart. This means that faith is made to depend on the tangible, thus placing the tangible at a premium and relegating faith to being the means of providing the tangible. A Christian who becomes ill and is not healed has his faith denied him. A Christian whose business goes to the wall has his faith destroyed because it was dependent on business success. How an oppressed Christian in Albania views himself in these terms is unimaginable, likewise the Sudanese Christian who, his crops destroyed by drought, is on the brink of starvation.

Finally, there is one extreme outworking of this dualism which is taught by many prosperity theologians. Because the spiritual is over the physical, the physical death of Christ on the cross cannot end our spiritual separation from God. Price argues that if this were not the case, then the death of one of the thieves would have been enough. Copeland bluntly states that 'when His blood was poured out, it did not atone'. The belief is that it was Christ's three-day spell in hell which culminated in a spiritual death that finally effected the atonement. ⁴²

Positive confession

What is it you want to get from God? If you want healing, then sow healing seed! If you want a miracle, sow some miracle seeds! If you want to be unafraid, then sow some seeds along this line. If you want deliverance from fear, habits, Satan's power, then get the appropriate seeds and sow them in your heart. ⁴³

'Name it and claim it' and 'Believe it and receive it' are phrases that trip off the tongue and they have become the catchphrases of the teachers of this 'slot-machine' concept of *positive confession*. The idea is that you find the appropriate texts in the Bible, sow them in your heart, and then claim the thing that you want and which they offer. By behaving as if that which you have asked for has already been granted, you demonstrate the faith which will invariably be rewarded.

This attitude colours the faith movement's teaching on confession of sin. Whereas our Lord taught us to pray daily for the forgiveness of our sins, Kenyon argues that

When you confess your weakness and your disease you are openly confessing that the Word of God is not true and that God has failed

to make it good. . . . The believer who is always confessing his sins and his weakness is building weakness, failure, and sin into his consciousness. ⁴⁴

Thus he argues that God requires a positive confession that as Christians we are 'not only clothed in the Righteousness of Christ but actually partakers of His Righteousness. This is a photo of our present walk with Christ'. ⁴⁵

A girl with whom I worked in Bradford told me that the reason why she had left her evangelical Anglican church was that she had been convicted by the Harvestime fellowship in Bradford that confession of sins in church was wrong. Because she was in Christ, she knew instantly when she had sinned and could ask for forgiveness there and then. Hence, to confess them again in church was not only otiose, but also highly negative in that it focused attention on the devil's power in her life, thus hindering the work of the Spirit.

The faith movement today

The faith movement is well established in America. In 1979 Hagin's son-in-law, Doyle Harrison, founded the International Convention of Faith Churches and Ministers (ICFCM) and its founding officers include all the leaders of the faith movement in America. The ICFCM has over a hundred member churches, and more than 700 ministers. The ministers themselves have to have passed through Hagin's college, the Rhema Bible Training Center, and all must swear an affidavit submitting to the constitution, ethics, and tenets of faith of the Rhema Ministerial Association International, a body established in 1985 by Kenneth Hagin Ministries, Inc. The Hagin connection continues and Doyle Harrison is also the president of Harrison House, which is the faith movement's major publishing company.

In Britain, the faith movement is similarly structured and is beginning to gain a sense of permanency. If Hagin is the central figure of the movement in America, then Bryn Jones fulfils that role in Britain. He is described as 'the founder and driving force behind Harvestime Fellowship, *Restoration Magazine*, the Dales and Wales Bible Weeks, School of the Word, Word to the World, Dales Television, and the International Christian Leadership Programme'. ⁴⁶ Writing in 1985, Andrew Walker estimated that Jones' organization covered between 15,000 and 18,000 members plus children, in addition to several thousand who, while interested, remained in their own churches. Thus, after just ten years their numbers were virtually on a par with the Elim Pentecostals. He adds that these people are well organized, and led by powerful leaders who have a sound financial backing. ⁴⁷

In South Africa, Ray and Lynda McCauley, 1979 graduates of Hagin's Rhema Bible Training Center, have founded *Rhema Ministries South Africa*. This organization is already a large property owner and employs many people. It has a training centre turning out 300 graduates per year, and a distribution division sending out in excess of 10,000 tapes monthly. Elsewhere prosperity theology is preached, amongst other places, in Scandinavia, where Stanley Sjöberg and Hans Braterud have significant followings, Kenya and India where various Britons have established churches, and Argentina where Orvil Swindol operates.

In Britain, as in America, the faith movement is placing an emphasis on *televangelism*. Much money has been collected for this medium of outreach. Dales Television is just waiting for the chance to hitch a ride on the wave of satellite television. They have already produced thirty-eight programmes involving entertainers like Cliff Richard, which have been broadcast across Europe from the Oslo base of *New World*.⁴⁸

While the faith movement in Britain increases in size, influence and in its sense of permanency, yet it seems unlikely that it will ever grip the nation in the way that it appears to have done in the States. The reasons are cultural. In his celebrated religious history of America, Sidney Ahlstrom noted that even in the nineteenth century it was commonly acknowledged that those who suffered did so because of their sin and so were responsible for their own plight.⁴⁹ This was the corollary of the Protestant work ethic which saw wealth as a sign of election, and which dominated Calvinistic America. Thus, John D. Rockefeller judged his wealth to be a divine reward for his faith, and Andrew Carnegie wrote a book called *The Gospel of Wealth*. In spite of their scandalous business practices, there seems to be no doubting their sincerity. For a nation made up of individuals who believed that they had been elected by God, it was natural to assume that God would bless the nation in a special way, raising her up to dominate world politics. 'In God we trust' is therefore not so much a statement of fact as a claim to God's blessing. Thus, national and personal wealth have always been acknowledged as signs of God's blessing in America. This is not the case in Britain where money remains a taboo area, especially when it abounds. People love to be outraged when the *Mirror* newspaper publishes lists of Britain's top earners, and while things are gradually changing, it is still considered unsophisticated to flaunt wealth. Hence, S. Matthew has to write:

We need not be ashamed of our wealth or try to hide it. . . . Don't be embarrassed about God's blessing — welcome it.⁵⁰

In spite of this, the preaching of the gospel of wealth is driving some people away, and it seems that it was responsible for splitting the faith movement's church in Bath.⁵¹

Another big difference is that in America, all fundamentalist denominations including the faith movement are soaked in nationalistic fervour, America being seen as God's mighty instrument in proclaiming his gospel to all nations. The following prayer illustrates this point:

Father, in Jesus' name we pray your wisdom will come to our president, his advisors, senators, representatives, all other government and business leaders. We pray for the economy of the United States and for the prosperity of this country. Father, we remind you this day that:

This country has always given to those in need and has never hidden her eyes from the poor;

This country has given to the furtherance of the gospel in allowing full freedom for preaching, teaching, and evangelizing;

This country has given to the furtherance of the gospel in that her citizens, more than the citizens of any other country, have moved out over this world to win the world for Christ;

This country's economic system has given to the gospel the equivalent of millions of dollars by exempting property used in the

proclamation of the gospel from property taxes, by exempting the income of organizations which preach the gospel from income tax, and by exempting the tithes and offerings of God's people from income taxes;

Furthermore, the prosperity of this country has generated billions of dollars to be used in the spread of the gospel, and the continued prosperity of this economy represents the best hope of financing the evangelization of the world in this generation.

We therefore boldly agree, according to Matthew 18:19, that, in Jesus' name, the United States economy will experience a regeneration:

That research and technology will make sudden and dramatic breakthroughs;

That energy and dependence on foreign nations will come to an end and that the flow of dollars to heathen nations for oil will cease, releasing billions into the gospel;

That the crippling effects of inflation will be reversed and that the United States dollar will regain strength and honour in the world;

That communication capabilities will expand to the point that the entire world can be reached with the gospel message;

That believers all over the United States will receive the revelation of God's plan for prosperity and will give in abundance to the effective ministries for Jesus Christ in the earth.

For we pray as you have commanded us in 1 Chronicles 16:35: 'Save us, O God of our salvation, and gather us together, and deliver us from the heathen, that we may give thanks to thy holy name, and glory in thy praise.'⁵²

Reports are also filtering through on to our television screens of collaboration between right-wing fundamentalist Christian mission societies in Central America and the US Foreign Office, the latter seeing the former as a potent pro-US force in an area where the rise of anti-US socialism is a constant headache. Ironically, in Britain where one of the churches is a major part of the establishment, fundamentalism does not seem to inspire nationalism.

In the light of these transatlantic differences it will be interesting to see how the faith movement fares over the next few years. For example, how will the British viewing public take to *televangelism*? Will they be willing to finance it as Americans do? Will the traditionally reserved British character be able to cope with charismatic renewal if, as the faith movement demands, this is the only way forward?

Conclusion

In the end, prosperity theology is fatally flawed, and those who preach according to its tenets are surely guilty of being the teachers whom men gather around themselves 'to say what their itching ears want to hear' (2 Tim. 4:3). Not only do they preach an unbiblical gospel, but, and perhaps most significantly, they do not preach the uncomfortable message of Christ crucified, the one unifying factor of all Christian churches. In the blurb on the back cover of all Hagin's books is written the following: 'Mark 11:23 and 24 keynote Kenneth E. Hagin's life and ministry'. Look them up, and then compare what you read with what Alister McGrath writes in his book, *The Enigma of the Cross*:

The cross continually raises questions for the church, which dares to call itself 'Christian' after the one who was crucified and rose

again, and yet seems to prefer to look for the grounds of its identity and relevance elsewhere than in the crucified Christ.³³

Perhaps the sheer attractiveness and ease of it all should be enough to set alarm bells ringing because after all, Jesus taught that the narrow gate is the right one, though it is the hardest one to get through. Being a Christian does not ease one's life, rather it involves carrying a cross. To preach a Christian lifestyle that must involve perfect health, enough wealth to live off the fat of the land, and the ability to call, at whim, upon God to interfere with history on one's behalf, is to preach a faith that has no true biblical precedent. Prosperity theology is therefore heretical because its claim to be Christian cannot be substantiated, and the faith movement is to be rebuked wherever it is encountered.

¹ D. R. McConnell, *A Different Gospel: The Cultic Nature of the Modern Faith Movement*, a MS presented to Hendrickson for publication in 1988.

² *Ibid.*, pp. 12f.

³ *Ibid.*, pp. 19ff.

⁴ *Ibid.*, p. 21.

⁵ Several good examples of this are found in Kenyon's book, *The Hidden Man: An Unveiling of the Subconscious mind* (Kenyon's Gospel Publishing Society, 1970), e.g.: 'We are not dealing with mysticism, philosophy, or metaphysics. We are dealing with realities . . . we are dealing with the basic laws of man's being, the great spiritual laws that govern the unseen forces of life' (p. 35).

⁶ See D. R. McConnell, *op. cit.*, p. 56.

⁷ See *ibid.*, p. 49.

⁸ F. K. C. Price, *High Finance - God's Financial Plan* (Harrison House, 1984), p. 12.

⁹ *Ibid.*, pp. 12ff.

¹⁰ N. Hayes, *Prosperity NOW!* (Harrison House, 1986), p. 61, and P. Gaunt, *Kingdom Businesses* (recording of a Downs Week 1985 presentation).

¹¹ S. Matthew, *Money Matters*, from the *School of the Word* study series (Harvestime, 1987), p. 48.

¹² *Ibid.*, pp. 45f.

¹³ G. Copeland, *God's Will is Prosperity* (Harrison House, 1978), p.

54.

¹⁴ F. K. C. Price, *op. cit.*, p. 36.

¹⁵ *Ibid.*, p. 41.

¹⁶ M. Bassett, *Expect the Best* (Network Media, 1987), p. 52.

¹⁷ F. K. C. Price, *op. cit.*, p. 30.

¹⁸ S. Matthew, *op. cit.*, p. 58.

¹⁹ C. K. Barrett, *The Second Epistle to the Corinthians* (A. & C. Black, 1973), p. 237.

²⁰ J. Wesley, as quoted by K. Fullerton, *Calvinism and Capitalism, from Max Weber: Essays in Sociology*, translated and edited by R. W. Green, D. C. Heath and Co., 1959, p. 74.

²¹ P. Gaunt, *Kingdom Businesses*.

²² S. Matthew, *Money Matters*, p. 54.

²³ J. Ellul, *Money and Power* (Marshall Pickering, 1986), pp. 65ff.

²⁴ *Ibid.*, p. 71.

²⁵ T. E. Schmidt, *Hostility to Wealth in the Synoptic Gospels* (Sheffield Academic Press, 1987), p. 60.

²⁶ *Ibid.*, p. 118.

²⁷ *Ibid.*, p. 164.

²⁸ K. E. Hagin, *Must Christians Suffer?* (Rhema Bible Church, AKA Kenneth Hagin Ministries Inc., 1983), p. 2.

²⁹ John Wimber, *Power Healing* (Hodder & Stoughton, 1986).

³⁰ D. A. Carson, *The Expositor's Bible Commentary, Vol. 8* (Zondervan, 1984), p. 207.

³¹ F. K. C. Price, *Is Healing For All?* (Harrison House, 1976), p. 104.

³² E.g. D. R. McConnell, *A Different Gospel*; J. Swaggart, *Hyper-Faith: A New Gnosticism* (Jimmy Swaggart Ministries, 1982), and A. B. da Silva, *The Theology of Glory Movement*, from *Religion Today* Vol. 1, Nos. 2/3, Oct/Dec 1984.

³³ See E. W. Kenyon, *Jesus The Healer* (Kenyon's Gospel Publishing Society, 1968), pp. 5f.

³⁴ E. W. Kenyon, *The Two Kinds of Life* (Kenyon's Gospel Publishing Society, 1971), p. 81.

³⁵ F. K. C. Price, *Is Healing For All?*, p. 122.

³⁶ F. K. C. Price, *High Finance*, p. 55.

³⁷ K. E. Hagin, *Redeemed From Poverty, Sickness, and Death* (Rhema Bible Church, AKA Kenneth Hagin Ministries Inc., 1983), p. 25.

³⁸ F. K. C. Price, *Is Healing For All?*, p. 122.

³⁹ B. Jones, *According to Your Faith*, from the *School of the Word* study series (Harvestime, 1985), p. 21.

⁴⁰ A. Brandon, *Health and Wealth* (Kingsway, 1987), pp. 48f.

⁴¹ See P. Scanlon, *Living in the Anointing*, from the *School of the Word* study series (Harvestime, 1986), p. 10.

⁴² This is a concept which I have not encountered in British publications, nor in American ones at first hand, though McConnell, while accepting some confessions over the matter, nevertheless provides convincing evidence that this is indeed believed by most of the major faith movement teachers: see *A Different Gospel*, pp. 204ff.

⁴³ J. Osteen, *How to Release the Power of God* (John Osteen Publications, 1978), p. 21.

⁴⁴ E. W. Kenyon, *The Two Kinds of Life*, p. 60.

⁴⁵ E. W. Kenyon, *Two Kinds of Righteousness* (Kenyon's Gospel Publishing Society, 1965), p. 38.

⁴⁶ See B. Jones, *According to Your Faith*, p. 6.

⁴⁷ See A. Walker, *Restoring the Kingdom* (Hodder & Stoughton, 1985), pp. 96ff.

⁴⁸ See C. Martin, *Here Come the Evangelists*, *The Times*, 22 February 1988, p. 20.

⁴⁹ See S. Ahlstrom, *A Religious History of the American People* (Yale University Press, 1972), p. 798.

⁵⁰ S. Matthew, *Money Matters*, p. 59.

⁵¹ In a note on p. 296 of *Restoring the Kingdom*, Walker writes: 'A former member of R1 has told me that I have underplayed the Americanisation of Bryn [Jones]'s team. Since 1979, he claims, Bradford has become less of a community and more of an American-style mid-Western teaching centre. He also claims that the fact that American prosperity doctrines have recently split R1's Bath Fellowship shows that such doctrines are increasingly finding their way into Restorationist teaching.'

⁵² A. Houghton, *Power of Agreement* (Logos International, 1981), pp. 115f.

⁵³ A. E. McGrath, *The Enigma of the Cross* (Hodder & Stoughton, 1987), p. 11.

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Writing the history of ancient Israel: a review article

In his 1977 essay on 'The History of the Study of Israelite and Judean History' J. H. Hayes isolated four major 'current approaches' to the writing of ancient Israel's history.² The first was the conservative/orthodox/traditional approach presupposing the 'supernatural origin' and inerrancy of the original text of Scripture. This approach 'works primarily from the evidence of the biblical text, supplying this with illustrative and supportive material drawn from extra-biblical texts and archaeological data'.³

A second approach is archaeological, particularly associated with W. F. Albright and his pupils.⁴ Albright assumed the basic reliability of the OT traditions and believed that archaeological evidence functioned 'as a control against unnecessary dependency upon literary, philosophical, or fundamentalist hypotheses'.⁵ Hayes notes, however, that 'there are many archaeologists who would not share his methodological approach'.⁶

A third approach is via tradition history. This approach, associated with such names as G. von Rad⁷ and M. Noth,⁸ presupposed the late writing down of traditions which were subsequently redacted into the present books of the OT. Among Noth's distinctive contributions to Israelite historiography was the suggestion that the tribal union was modelled on the Greek amphictyony.

A fourth approach involves the use of socio-economic categories to illuminate ancient Israelite society. Associated initially with G. E. Mendenhall⁹ and made widely available in the monumental study by N. Gottwald,¹⁰ proponents of this approach have argued that Israel's origins are to be sought within Canaan, in a popular movement that sought a new way of life free from the existing oppressive social structures.

Hayes' concluding paragraph recognized that some contemporary histories of Israel 'cannot be said to be dominated by any exclusive methodology but were more eclectic'.¹¹ When, nine years later, Hayes co-wrote a History with J. M. Miller,¹² his comment would accurately describe

their own work. The importance of their history may be gauged by the fact that the *Journal for the Study of the Old Testament* devoted half an issue to it, providing a platform for different scholarly evaluations and then a response by Miller.¹³

A little over a decade later we have a new History: H. Shanks (ed.), *Ancient Israel. A Short History from Abraham to the Roman Destruction of the Temple*.¹⁴ Like the Miller-Hayes volume it was originally published in the States, and is a collaborative effort. However, while Miller-Hayes represents the work of just two scholars the Shanks volume presents contributions from a team of eight, all but one North Americans. Like Miller-Hayes its methodology is eclectic: no attempt has been made to impose a uniform approach between contributors.

How does the new work measure up in terms of accessibility and accuracy? We are delighted to be able to offer *Themelios* readers two different assessments. One is from a long-time contributor, Kenneth A. Kitchen, Professor of Egyptology at the University of Liverpool. Professor Kitchen is well known for his trenchant contributions to OT study; see e.g. *Ancient Orient and Old Testament*¹⁵ and *The Bible in Its World: The Bible and Archaeology Today*.¹⁶ Twenty-eight years ago he provided TSF readers with a minutely detailed review of Bright's History.¹⁷ The other is from Dr Richard ('Rick') Hess who reviewed G. J. Wenham's Word Commentary on Genesis 1-15 in the April 1989 issue. Dr Hess lectures in Old Testament at BTI Glasgow, and has worked on the Tyndale House Genesis 1-11 Project and the (Sheffield) Hebrew Dictionary Project. He has a major work on the Amarna Correspondence in press and is completing a monograph on Personal Names in Genesis 1-11.

Their differing assessments of the Shanks volume indicate something of the range of an evangelical response on the ongoing question of how to write the history of ancient Israel.

David Deboys,
Reviews editor.

¹ In J. H. Hayes and J. M. Miller (eds.), *Israelite and Judaeae History* (London: SCM, 1977), pp. 1-69.

² *Ibid.*, pp. 64-69.

³ *Ibid.*, p. 65. Hayes cites L. T. Wood, *Survey of Israel's History* (Grand Rapids: Zondervan, 1970), as illustrating this approach. The most recent example is E. H. Merrill, *Kingdom of Priests. A History of Old Testament Israel* (Grand Rapids: Baker, 1987). In the notes which follow, no attempt is made to be comprehensive bibliographically, I only cite representative works in English. Also ignored are the rash of current monographs dealing exclusively with the early history of Israel. For references see J. Bimson's article in the present issue.

⁴ W. F. Albright, *The Biblical Period From Abraham to Ezra* (New York: Harper Torchbooks, 1963). The best-known representative of this approach has been the successive editions of J. Bright's *A History of Israel* (Philadelphia/London: Westminster/SCM, 1981).

⁵ Hayes in Hayes-Miller (1977), p. 65.

⁶ E.g. W. G. Dever; see W. G. Dever and W. M. Clark, 'The Patriarchal Traditions', in Hayes-Miller (1977), pp. 70-120. An example of a recent history which attempts to work from 'archaeological results and non-biblical evidence' (publisher's catalogue, p. 14) is N. P. Lemche, *Ancient Israel. A New History of Israelite Society* (Sheffield Academic Press, 1987). The resulting picture is questionable. Another recent History, ostensibly written 'with a deep knowledge of philology and archaeology' (back cover), is G. Garbini, *History and Ideology in Ancient Israel* (ET; London: SCM, 1988); but see A. R. Millard's review in *Themelios*, April 1989, p. 105.

⁷ G. von Rad, *The Problem of the Hexateuch and Other Essays* (ET; Edinburgh/New York: Oliver & Boyd/McGraw-Hill, 1966).

⁸ M. Noth, *Das System der zwölf Stämme Israels* (Stuttgart, 1930); *The History of Israel* (ET; London: A. & C. Black, 1960).

⁹ G. E. Mendenhall, *The Tenth Generation* (Baltimore: Johns Hopkins, 1973).

¹⁰ N. Gottwald, *The Tribes of Israel. A Sociology of the Religion of Liberated Israel, 1250-1050 BCE* (Orbis: Maryknoll, 1979/London: SCM, 1980).

¹¹ Hayes in Hayes-Miller (1977), p. 69. Hayes instances S. Herrmann, *A History of Israel in Old Testament times* (London/Philadelphia: SCM/Fortress, 1975), and R. de Vaux, *The Early History of Israel: To the Period of the Judges* (ET; London/Philadelphia: DLT/Westminster, 1976). H. Jagersma, *A History of Israel in the Old Testament Period* (London/Philadelphia: SCM/Fortress, 1983), is probably to be included in this group.

¹² *A History of Ancient Israel and Judah* (London: SCM, 1986).

¹³ *JSOT* 39 (1987), pp. 3-63.

¹⁴ Washington: Biblical Archaeology Society, 1988/London: SPCK, 1989.

¹⁵ London: Tyndale Press, 1966.

¹⁶ Exeter: Paternoster Press, 1977. See now his contribution to the ongoing discussion of covenant in a review article of E. W. Nicholson, *God and his People: The Fall and Rise of Covenant, Law and Treaty*, *Tyndale Bulletin* 40 (1989), pp. 118-135.

¹⁷ *TSF Bulletin* 29 (1961), pp. 13-20; subsequently updated in *TSF Bulletin* 39 (1964) Supplement and then published separately in a 'third edition' in 1976.

H. Shanks (ed.), *Ancient Israel: A Short History from Abraham to the Roman Destruction of the Temple* (Washington: Biblical Archaeology Society, 1988/London: SPCK, 1989), xx + 267 pp., 29 text figures, 9 maps, 2 charts, 9 colour plates, £12.95 pb.

The dynamic editor of the liveliest popular periodical in biblical studies (*Biblical Archaeology Review*) has induced eight scholars (one French, the rest American) each to contribute a chapter to this new, compact history of early Israel from Abraham down to the Romans in AD 70. The concept was to combine a high level of scholarship with a highly readable text, incorporating recent developments and archaeological discoveries. As Shanks says, there is nothing quite of this scope in the size currently available (the recent essays by Garbini, *History and Ideology in Ancient Israel*, 1988, and Lemche, *Ancient Israel*, 1988, are tendentious, ill-informed and misleading). This gap, he aims to fill. The quality of production is good, and beyond any doubt, the entirely laudable aim of ready readability is certainly well

achieved — on that (dealing with scholars) all credit is due. But what of content? Here, matters are less clear-cut, as scrutiny reveals.

Chapter 1, 'The Patriarchal Age', by P. Kyle McCarter, begins by criticizing the biblical data for dating the patriarchs, mechanically lumping together all the figures with no regard for their varying nature. Not surprisingly, this brings only discord, because his treatment is wrong. These are ancient Near Eastern, not modern, Western-style documents, and must be treated accordingly; the transmission of ancient numbers also enters into play. This reviewer's treatment of a quarter-century ago (*Ancient Orient & OT*, 1966, pp. 41-56) is significantly ahead of McCarter's on both methodology and results, requiring only minimal modification. The claim that the 'patriarchal history reflects the political and religious viewpoint of the Judean monarchy and priesthood' (p. 3) is made without offering any proof, as are the statements that treat the documentary hypothesis as fact (*cf.* pp. 12-13 and *passim*) instead of theory. More cogently, McCarter successfully reviews various scholarly interpretations of the patriarchal narratives: of Albright and followers, of Noth, to both of which he records objections. External comparative data for an early second-millennium (BC) date for the patriarchs are rejected in dependence on the twin works by T. L. Thompson, *The Historicity of the Patriarchal Narratives* (1974), and J. Van Seters, *Abraham in History and Tradition* (1975), whose views and statements he adopts and cites uncritically. These works performed the useful service of clearing away misuse of some external sources, particularly in the Nuzi texts, as background for the patriarchal narratives, but they misrepresented the external evidence to a serious extent. They are strictly works for the non-thinking biblical scholar. It needs to be stated that Albright's old hypothesis of Amorite mass migrations, for example, has no bearing on the patriarchs.

On proper names, being so uncritical of his twin sources, McCarter is seriously in error. The so-called 'Amorite Imperfective' type of name (often beginning with J, I, in the English Bible), like Jacob, Joseph, Isaac, are admitted to be 'especially well known from Middle Bronze sources'. But McCarter (and his twin sources) then allege (p. 11): 'there is no reason to believe that its use diminished after the Middle Bronze Age; . . . well attested in Ugaritic and Amarna . . .', with Jacob-related names appearing in Aramaic and in Palmyrene (3rd century AD). Just how wrong can one be? The facts are as follows. First, this class of names is already found in the 3rd millennium BC — well before the patriarchs — at Ebla, as a recent and reliable reference work makes clear.² Second, from Gelb's monumental compendium on early 2nd-millennium Amorite,³ from over 6,000 names, some 1,360 of the 'Jacob/Isaac' type form *one-sixth* of the entire corpus (16%) — and 55% (over half) of all names beginning with I/Y. What about the Late Bronze Age (McC: not diminished)? At Ugarit, using Grondahl's two lists (alphabetically written, syllabically written names),⁴ the 1,860 alphabetic script names yield under 40 of these names — only 2% (contrast 16% in Middle Bronze), and only one-third of I/Y names (some 30%, not 55%). In syllabic script, it gets worse — 4,050 names yield only 20 'Jacob-type' names (down to 1/2%)! and now only one-quarter of the initial I/Y names (25% instead of 55%). In direct contradiction to McCarter and Thompson/Van Seters, there is clearly a massive diminution between (say) 1800 BC and 1300 BC — overall 16% drops to between 2% and 1/2%, while in the I/Y groups, it drops from 55% to between 30% and 25%. Going into the 1st millennium BC, the divergence gets worse again. Using Tallqvist's compendium (5,000 names),⁵ only some two dozen such names occur; half of these are 2nd millennium, leaving us only a dozen for the 1st millennium — of 5,000 names, only 0.24%, or 1/70th of the Middle Bronze Age proportion! And out of 740 I/Y names, it is still only 1.6%, a minute fraction of the 55% of the early 2nd millennium. For McCarter these figures are damning. The Aramaic references are a red herring; these are not of the 'Jacob' imperfective type, hence of no relevance (the root 'aqab is common) — the only one of our type (at Palmyra) is himself a Jew!⁶ In short, there was a drastic and continuing diminution in use of such names after the Middle Bronze/early 2nd millennium BC/patriarchal age.

Serious errors of this kind affect McCarter's other pronouncements on proper names. The name of Zebulon, with other sons of Jacob, is dismissed as merely geographical and not personal (p. 28), and 'means something like "highland"'. In fact, names most closely linked with Zebulon are (i) personal names only, and (ii) particularly

in the early 2nd millennium BC. We possess: (1 & 2) *Ziblanum*, two men so named in an Old Babylonian wage-list;⁷ (3) *Zabilum* and (4) *Zubalan*, both from Mari, same period;⁸ (5) a *Zabilanu*, ruler of Shutu in Transjordan, c. 1800 BC, earlier Exeeration texts (e6);⁹ and (6) *Zabilu-Haddu* ('Hadad is Prince'), in later such texts (E.16), in c. 1770 BC as ruler of [. . .] Jshi.¹⁰ In these names the root *zbl* means 'exalted', and hence 'prince', with variations ('little prince', etc.). The common noun 'prince', *zbl*, is well attested in the religious epics and legends from 14th/13th-century BC Ugarit, originating much earlier (c. 19th-16th centuries BC).¹¹ All these data are readily available to scholars, yet they are ignored. Even in Hebrew, *zbl* is far more metaphorical ('exalted', etc.) than literally 'highland'. McCarter's treatment of this name is inexcusably misleading. His treatment of other names (Abraham, Asher, etc.) is almost as bad, and equally misleading.¹²

Matters are no better in the legal/social sphere. Again, following Thompson and Van Seters, McCarter rightly rejects Speiser's Hurrian fantasies, based on mistaken use of Nuzi data (p. 11). Again, he admits that good background material is available from Old Babylonian and other texts of the early 2nd millennium for the responsibilities of a barren wife towards her husband, and so on. He then alleges parallels from a 12th-century Egyptian contract and an Assyrian document of 648 BC, to prove that such usage went on into the 1st millennium. Unfortunately, he has merely 'lifted' these two items from Thompson and Van Seters, without attempting to verify the facts. Both parallels are wrong. The Egyptian document is unique and is in content *not* a valid parallel, dealing with simply an unusual series of adoptions.¹³ The Assyrian piece is at best only a partial parallel, hence proves nothing, having a different emphasis from the patriarchal usage — it too is an abnormal document for its period, as its editor remarks.¹⁴ So, neither document proves patriarchal usage going into the 1st millennium — and all the really good and valid comparisons are, as before, in the first half of the 2nd millennium. It is regrettable that McCarter's view is so narrowly restricted that he fails to consider almost any view other than those of Thompson and Van Seters and those they tried to debunk, instead of reviewing a wider range of scholarly contributions,¹⁵ and going for facts not just opinions.

As for history of tradition, the guesswork of Alt and Noth is reviewed, thankfully a little more critically; then, the genealogical suggestions of Oden, at third remove from original sources. This is no way to handle such a topic. Like should be compared with like, on a wide factual basis.¹⁶ This procedure was given a first-ever systematic outline by the reviewer a dozen years ago; of the data and the treatment alike, McCarter seems unaware.¹⁷ Furthermore, even quite 'folksy' stories can be shown to preserve good historical data, and to involve strictly historical people. A parade example is the totally fictional 'Tales of the Magicians' (Papyrus Westcar) of c. 1600 BC, concerning characters of c. 2500 BC, almost a thousand years before.¹⁸ These tales correctly name three (originally four) monumentally-attested pharaohs of the Pyramid Age, in their correct order, three sons of Kheops (known to be real ones), and the three first kings of the succeeding 5th Dynasty, with only two errors in the relationships (on number of full brothers, name of mother). If an Egyptian folk tale (written in what was informal language for its day) can retain historical people so well in a thousand years' time-lapse, then — judged by these external standards — there is no reason for doubting the names, sequence or historicity of the Hebrew patriarchs, especially as our existing narratives are older than the monarchy period; these too could have transmitted data across a thousand years, but the actual period was probably rather less. However, evidence of this kind never entered McCarter's very limited horizons. It is distasteful to have to criticize his well-written essay so severely, but he has brought this upon himself.

Chapter 2, 'Israel in Egypt', by N. M. Sarna, is a considerable improvement on McCarter in both content and method. He too has problems (*i.e.* dating the exodus, questions of historicity), but opts — probably rightly — for the 13th century and a real event. Proper background data are duly utilized (foreign immigrants from Canaan into Egypt; scope of brickmaking; popular religion; features of the plagues, etc.), indicating a very definite Egyptian colouring to the narratives. The route of the exodus and Sinai travels is a difficult question, but Sarna is wrong to dismiss too lightly the general location of Gebel Musa for Mt Sinai — not on the basis of Byzantine

tradition, but on ecological grounds made clear by this writer and others.¹⁹ Sarna rightly stresses the importance of the covenant at Sinai, but fails badly to recognize the mass of external evidence that clearly dates the Sinai/Moab covenant to the 14th/13th centuries BC.²⁰ It is good to see him giving some realistic background for the tabernacle in the Late Bronze Age, using the Timna shrine and essential Egyptian data. As Sarna stresses, we have no explicit proof for the events of the exodus and after, but (as he notes) negative evidence is no evidence, and his overall assessment that the transmitted biblical narratives give us the best framework for the facts is a sound one.

Chapter 3, 'The Settlement in Canaan', is by the late J. A. Callaway. Its main value is simply the presentation of the views of a mere handful of scholars on Israel's 'conquest' and settlement in Canaan, where the archaeological picture has been in part transformed in recent years. Callaway reviews the views of Yadin and Malamat (who favour some kind of conquest), of Alt (favouring mainly peaceful infiltration by nomads), and of Mendenhall and Gottwald (seeking Israelite origins in a 'peasant revolt' within Canaan). Regrettably, an exaggerated antithesis is drawn between the books of Joshua and Judges, which reflects, as Callaway says (p. 53), the impression given by 'a casual reading' of those narratives. A casual reading is precisely what scholar or student should *not* be indulging in, if they want valid results. Only careful, observant study is valid here; like others, Callaway simply ignores statements like Joshua 13:1: 'very much land remains to be possessed', and appears not to realize that allocation of land is one thing, but actual physical occupation of it is quite another. The case of the few centres actually stated to have been conquered is presented less than fairly. It is all very well to say that Jericho and Ai 'should have identifiable traces of destruction dating to the time of the conquest' (p. 61) and that Jericho still has some 70 ft of occupation layers 'intact' — a misleading adjective to use here. It all depends on the *state of preservation* of the site and its layers, *not* all of which are intact. Callaway begins to admit this (p. 62) when he concedes that of Middle Bronze Jericho (*i.e.* patriarchal period), 'a substantial portion of the fortifications and the city [that was] burned about 1560 BC had eroded away — the part of the city on the top of the mound — so it is impossible to reconstruct the city's history after 1560 BC' (reviewer's italics). Exactly! And therefore, we are not entitled to misuse archaeology to deny, any more than to affirm, the reality of Joshua's reported capture of Jericho. It gives the lie to Callaway's misjudgment that — somehow — sufficient traces of the Late Bronze city of the 14th/13th centuries BC should have survived for our convenience. He admits major loss of the Middle Bronze town, which happened in hardly 200 years; hence in the 400 years from Joshua to Ahab — almost half a millennium — near-total erosion of a smaller settlement has been the result. And despite the common archaeological myth to the contrary, pottery is both destructible and degradable, even if it takes longer than with other artifacts. Callaway's rejection of Yadin's view is based on the fallacy of negative evidence, hence is inherently invalid; Yadin's view is in some measure more probable.

Ai is currently more difficult to assess. Callaway (who dug at Et-Tell) persists in dogmatically identifying Ai with Et-Tell, even though the latter's occupation history better suits Beth-Aven.²¹ It should be stated in fairness that no other suggested location for Ai has proved acceptable so far (despite Livingston and others); so, as with much else in the ancient world, Ai must remain an enigma for the present. At Gibeon, Callaway falls into a common trap; excavations there found no Late Bronze city, so there is a problem. What he fails to make clear is that the Pritchard expedition touched barely 5% of the area at El-Jib, and the few Late Bronze tombs that *did* turn up only did so in the last of four seasons' work. It is the besetting sin of Palestinian archaeologists to trumpet negative results based on excavation of minute areas of sites as if they were irrevocable and definitive, leaving between 95% and 98% of a site undug and out of consideration.

Hazor is a different case. Here, we have a very definite destruction in the late 13th century BC (despite some quibbles) of level XIII, that might well reflect Joshua's impact. It should be noted that the Jabin II of Judges is called king of Canaan more often than king of Hazor — the latter name remained as name of his North Canaanite state rather than his erstwhile capital. The confused opinions reported by Callaway (pp. 66-68) are of little merit. Incidentally, the supposed

bottom date for Hazor's fall, 1230 BC (p. 68), is now wrong, because based on an outdated and too-high Egyptian chronology — it can be lowered by a decade or two if need be. It is possible to attribute level XI to Jabin II of Deborah's day; Israelite occupation of Hazor almost certainly happened much later. No other city destructions can be attributed to the incoming Israelites with any certainty, either on biblical or any other evidence. For example, Lachish level VII (and not VI as Ussishkin has suggested) *might* have suffered the impact of Hebrew attack, but Joshua-Judges do *not* authorize us to claim this — the destructions of Lachish levels VII and VI may as easily have been done by local Canaanite foes, incoming Philistines, or the Egyptians crushing revolts, to name but a few.

Then Callaway draws upon the considerable amount of new digging and wide surface surveys recently done in Israel, rightly noting (p. 73) that these appear to reveal 'a considerable influx of newcomers to the hill country of Canaan . . . at the end of the 13th century BC', citing the findings of Stager, Kochavi and others. Thus, something new had clearly happened; such newcomers (as Callaway notes also) had increased 'too rapidly to be ascribed to natural growth'. Here the Mendenhall/Gottwald internal peasant revolt theory (as is widely acknowledged) is worthless. The main movement seems to have entered from the east and spread westward, particularly through Ephraim. This tallies perfectly with an Israelite entry from across the Jordan, and early occupancy of areas like Shiloh, for example. On this point, Callaway observes, again rightly, that these people were *not* Alt's imaginary desert nomads; they were herdsmen and cultivators. These are skills that the Hebrews already had as patriarchal clans (cf. Genesis), and still had on their exodus from Egypt; cf. Ex. 9:4, 6-7; 10:24-26; 12:38, for the Hebrews' livestock then. All of this fits together better than might be gleaned from this chapter. Callaway knew his Palestinian archaeology, but his use of that and the biblical data leave a lot to be desired.

Chapter 4, 'The United Monarchy', by André Lemaire, covers Saul, David and Solomon. In his account of Saul's reign, the impact of archaeology is limited to a summary list of sites (p. 91) in which the candidature of Tell el-Ful for Gibeah of Saul is not even mentioned, still less discussed. Again, the Near Eastern evidence that authenticates Samuel's description of levantine kingship on the eve of Saul's appointment to kingship (1 Sa. 8) is ignored.²² Attribution of Saul's wars to David is merely unsubstantiated speculation. There follows a fair summary of David's wars and transformation of Israel into a fully organized state. Half of Lemaire's account is devoted to Solomon's reign in many of its aspects. This is a useful outline, including archaeological data, but has its weaknesses. Thus, the essential link between Solomon and wisdom literature (including Proverbs) is seriously underestimated, and the Egyptian links with his administration not understood.²³ Again, Hadad of Edom (p. 103 and n. 59) has nothing to do with Aram; (H)adad as a deity is common to all the West Semitic groups. Additional data in Chronicles is needlessly dismissed, whereas we know that Chronicles actually preserves authentic data not in Kings — the Sukkiim of king Shishak, for example (Tjukten of Egyptian sources). Nor are these the only weaknesses.

Chapter 5, 'The Divided Monarchy' (Judah and Israel), is by S. H. Horn, who provides a reasonable, straightforward account of the twin kingdoms down to the Assyrian and Babylonian exiles respectively. Apart from the obvious mentions of the Moabite Stone, Hezekiah's tunnel inscription and the Samaria and Lachish ostraca, plus the Assyrian references, the use of archaeological and external data is rather sparse. Throughout, one glaring fault is that this author is twenty-five to thirty years out-of-date on matters relating to Egypt. He treats as the last word on the subject (p. 248, n. 1) Mazar's impossible views on the list of Palestinian place-names left us by the pharaoh Shoshenq I (Shishak); several other treatments have been offered since 1957, and the whole matter dealt with in proper Egyptological terms by this reviewer.²⁴ The Samaria ivories may be evidence of Ahab's ivory house', although a later date has been suggested. On pp. 130-131, Horn has followed the outdated views of Goedicke (quarter-century obsolete): So is a king, *not* the city of Sais, and most probably Osorkon IV of Tanis.²⁵ Again, on p. 136 Horn clings to the wrong view of two Palestinian campaigns by Sennacherib (now rejected by virtually all competent authorities), instead of one actually attested in 701 BC, misunderstanding even the biblical data. The simple fact remains that our present account in Isaiah and Kings was cast in its

present form *after* 681 BC, as it records the death of Sennacherib. By that time, Taharqa had already been king in Egypt for nine years, and hence is referred to as 'king' in all contexts, including earlier ones, just we would say 'Queen Elizabeth II was born in 1926', although she was not queen in that year. The whole matter was discussed in full fifteen years ago and again three years since,²⁶ so Horn's ignorance is inexcusable at this late stage. P. 117, the first five lines of dates are ten years too low (typographic error). The mess over Egyptian data impairs the usefulness of this chapter.

Chapter 6, 'Exile and Return', by J. D. Purvis, gives quite a good survey of Judah under neo-Babylonian and Persian imperial dominion, useful and competent.

Chapter 7, 'The Age of Hellenism', by Lee I. A. Irvine, and Chapter 8, 'Roman Domination', by S. J. D. Cohen, take us respectively through the period of the Ptolemies, Seleucids and Hasmonaeans, then of Herod and the Romans to AD 70. Both authors give vivid, masterly treatments of their periods, dealing with social and religious developments as well as the indispensable historical and political events. One curious omission from both chapters struck this reviewer: neither ever makes even the slightest reference to the role of the neighbouring and powerful kingdom of the Nabataeans (creators of rose-red Petra) in this period! The notes and an index close the volume.

The whole book is prefaced by H. Shanks's introduction, largely an enthusiastic 'blurb' for his brain-child. He makes a good case for the limits chosen (Abraham to AD 70). Much less satisfying is his idea that the further back one goes in the Bible (especially beyond annalistic works like Kings), the less reliable the biblical record becomes, and the less correlation there is with external sources. This betrays serious misunderstanding of those sources, both biblical and Near Eastern; effective correlation is *not* to be limited just to finding named biblical individuals in external sources (a naive error shared with McCarter), nor are reliable historical data to be found solely in annalistic-type writings. Space forbids further comment on so vast a subject.

How then shall the work be viewed? The last three chapters overall are commendable; chapters 2 and 4 are competent and useful, but have important gaps. Chapter 3 is not wholly satisfactory, and chapter 5 is thin and positively misleading on the several Egyptian connections. And the important chapter 1, patriarchs, is replete with substantial errors. One would dearly love to have been able to commend this well produced and readable book for the role its editor hopes for — but the very serious deficiencies mean that, factually, it cannot be recommended as a textbook or reference work; the flaws are too serious.

K. A. Kitchen.

¹ In *Ancient Orient and OT*, the findings of Glueck on the occupation history of Transjordan in the 19th-13th centuries BC require modernization (p. 43), likewise the matter of seasonal occupation of the Negev (p. 49), and (in part) the use of Nuzi parallels (p. 51); the rest still holds good today.

² Data in M. Krebernik, *Die Personennamen der Ebla-Texte, ein Zwischenbilanz* (Berlin, 1988), p. 10, S 3.2.3.1 ('very common', quoting *ib-dur-il = Iptur-il*, 'El has redeemed'), and pp. 199ff., *passim*.

³ J. J. Gelb et al., *Computer-Aided Analysis of Amorite* (Chicago, 1980).

⁴ F. Gröndahl, *Die Personennamen der Texte aus Ugarit* (Rome, 1967).

⁵ K. Tallqvist, *Assyrian Personal Names* (Helsinki, 1918).

⁶ M. Noth, *Die Israelitische Personennamen* (1928, repr. 1966), p. 46 n. 1. Even in Phoenician in the 1st millennium BC, using F. Benz, *Personal Names in Phoenician and Punic Inscriptions* (1972), pp. 54-186, his 32 pages of Semitic names include only 4 pages of Y-names (12%); at most, perhaps half of these come clearly into the 'Imperfective' category, i.e. barely 6% of the whole (or, only 1/3 of the usage seen in the early 2nd millennium BC).

⁷ See A. Goetze, *Bulletin, American Schools of Oriental Research*, No. 95 (1944), pp. 23-24: B.16,26.

⁸ J. Birot et al., *Archives Royales de Mari, Textes*, XVI/1 (1979), p. 238 (foll. on J. Kupper, *ibid.*, VI (1954), pp. 84/85, No. 57:9 and pp. 86/87, No. 58:20; and on J. Bottéro, *ibid.*, VII (1957), p. 94, No. 198, iv: 28').

⁹ In K. Sethe, *Die Ächtung feindlicher Fürsten, Völker und Dinge* (etc.) (1926), p. 47:e6.

¹⁰ G. Posener, *Princes et Pays d'Asie et de Nubie* (1940), p. 73: E.16.

¹¹ On term *zbl*, cf. (e.g.) remarks by A. Caquot, M. Szynger, A. Herdner, *Textes Ougaritiques*, I (1974), p. 74 & n. 2, who carefully distinguish this *zbl* from *'be ill'*, from a distinct root (cf. Arabic *dhbil*).

¹² Possibly comparable names at Ebla, Krebernik (n. 2, above), pp. 300, 281 (less likely). In the late 2nd millennium, sole comparable example is at Ugarit, Pi-Zibli, 'mouth of rulership'; Gröndahl (n. 4, above), p. 183, cf. 168, 170. For the 1st millennium BC, Tallqvist has no comparable name. On Asher (McCarter, p. 241 & n. 88), the denial of Albright and Kitchen in favour of Yeivin is wrong, as Yeivin was refuted by the latter. For Abraham, a good early 2nd millennium parallel is Aburahan in the Execration texts; *n/m* alternation, cf. in 2nd millennium, Nahrina/Nahrima (forms of Naharaim in Eg. & other texts). Abram must not be confused with Abiram, as McCarter does.

¹³ This document, Gardiner & Zulueta, *Journal, Egyptian Archaeology* 26 (1940/1), pp. 23-29; most recent translation, E. Cruz-Uribe, *ibid.*, 74 (1988), pp. 220-223; irrelevant to Genesis, see Kitchen, *Bible in its World* (1977), pp. 70-71.

¹⁴ So Postgate; cf. Kitchen, *op. cit.* (n. 13), p. 71.

¹⁵ E.g. the important essays in A. R. Millard, D. J. Wiseman (eds.), *Essays on the Patriarchal Narratives* (1980); Kitchen, *Bible in its World*, chapter 4, better and fairer assessment than McCarter's.

¹⁶ Not as in the lopsided manner of D. Irvin, *Mytharion* (1978), omitting much relevant comparative data.

¹⁷ See Kitchen, *Bible in its World*, pp. 61-68, using the proper range of comparative Near Eastern data.

¹⁸ See typical translations, e.g. M. Lichtheim, *Ancient Egyptian Literature*, I (1973), pp. 215-222 (not in Pritchard's *ANET* volume).

¹⁹ Cf. Kitchen in J. D. Douglas, N. Hillyer, etc. (eds.), *Illustrated Bible Dictionary* III (1980), pp. 1643-1645.

²⁰ Reference should have been made to Kitchen, *Bible in its World*, pp. 79-85; *cf.* also in *Tyndale Bulletin* (1989), forthcoming.

²¹ Cf. J. M. Grintz, *Biblica* 42 (1961), p. 201-216.

²² See I. Mendelsohn, *Bulletin, American Schools of Oriental Research*, No. 143 (1956), pp. 17-22, using the data from Ugarit.

²³ See Kitchen, *Vetus Testamentum, Supplement-Volume*, 40 (1988), pp. 120ff. with papers cited, p. 120 nn. 71/72.

²⁴ In the standard work for Egypt on this period, Kitchen, *Third Intermediate Period in Egypt* (1986), pp. 294-300, 432-447, 575-576.

²⁵ Full discussion, *ibid.*, pp. 372-375, 551-552, 583. The reviewer's identification of So with Osorkon IV is cautiously accepted and endorsed also by I. E. S. Edwards, *Cambridge Ancient History*², III/1 (1982), p. 576.

²⁶ Full treatment, *ibid.*, pp. 154-162 (esp. 160), 383-386, 552-553, 554; Kitchen in Görg (ed.), *Fontes atque Pontes, Festgabe: Hellmut Brunner*, pp. 243-253.

Do we need another history of Israel?

Hershel Shanks has provided the teacher and student with a well informed, up-to-date account of study on the history of ancient Israel. His sales pitch in the introduction argues for an affirmative to questions both student and teacher would ask: Is it balanced and comprehensive? and Is it interesting and fun to read? As to the first, the book lies well within the American (more specifically American Schools of Oriental Research) perspective of 'orthodox' archaeological and historical interpretation. Those who seek the defence of alternative views or a new synthesis must look elsewhere. As to the second question, we have the editor's hand throughout guaranteeing reader-friendly prose, accompanied by notes on difficult terms and names. There are also the pictures (including nine colour plates), reflecting *Biblical Archaeology Reviews*' (which shares the same

editor as the volume under consideration) wide inventory, and the maps (we overlook the confusion of the Tigris and Euphrates on the first one; by no means characteristic of the others). Many of the major issues and directions are covered in the chapters. Perhaps the brevity of the study, intended as a short history, explains the sometimes schematic presentation of the data which, in its attempt to show the 'progress' of research in the field, can omit some of the precise contours of what the data actually suggests. What is certain is the importance of this work as an influential introductory text on the history of ancient Israel for the English-speaking world in the coming years.

For example, in the first chapter, by Kyle McCarter, we learn that the Amorites did not invade Palestine at the EB/MB transition. But does that necessarily mean that Amorites did not exist or that an Abram figure could not have been an Amorite? Again, does the fact that the personal names of the patriarchs have parallels in later West Semitic history mean that the dominance of single-element yodh-prefixed names among these figures says nothing about their similarities with the Amorite names from Mari and elsewhere in the early 2nd millennium (where similar features obtain to a degree not found later)? McCarter wisely avoids such judgments, but unqualified criticism of a theory may invite the reader of the text to dismiss more than is necessarily intended. In any case, this criticism should not ignore a competent review of Noth's tradition-historical approach with criticism and updating. The discussion of the names of the sons of Jacob raises some questions. Why must Zebulon be a description of a place? Could it not more easily originally have been a personal name sharing the same Canaanite root as Jezebel and Zebul? The historical conclusions of the 'patriarchal period' reflect a move away from alignment with any particular extra-biblical events of the second millennium and towards an acknowledgment of something preserving a memory of this period, at least in the names and occasional glosses, but not coalescing as a nation until the 12th century in the central hill country of Canaan, and waiting another two centuries before appearing as a literary unit.

Editorial uniformity does not prevent alternative views from surfacing. Thus while we learn from McCarter that the Hyksos period does not fit the Joseph story (p. 27), Sama informs us that such a context 'makes considerable sense'. Indeed, the difference between these two chapters is striking, reflecting far more than a distinction in the literary forms of the biblical text. Whereas McCarter's approach begins with the classical theories and proceeds to demonstrate how they have changed in the light of new evidence, Sama focuses on the biblical narrative, pausing here and there to comment on relevant archaeological and epigraphical materials. Thus the issues Sama addresses are not whether there was an exodus or which segment of Israel came out of Egypt, but what Egyptian deities are challenged by the plagues and what can we know about the route of the exodus. Tradition-history questions such as the relationship of the exodus and the Sinai 'traditions' are entirely omitted. On the other hand, we come away from Sama's chapter with a much better idea of what the biblical text itself relates about the exodus and the wilderness wanderings.

Responsibility for the chapter on the present interpretative crux of the settlement of Israel in Canaan fell to the late Joseph Callaway. His distinction between the historiography of Joshua and that of Judges leads to discussion about redactors and the emergence of a 'canonical' or 'official' version. As is often the case in discussions of this kind it is not clear why two competing versions were allowed to stand side by side throughout the period in which the biblical documents came together into their present form. Callaway's discussion of the archaeological evidence reviews the difficulties which the conquest model has encountered over the past generation. He prefers the alternative view which finds in Joshua a redacted account which served political and religious purposes of generations far removed from the time of Israel's appearance in Canaan. Is not a third alternative possible? Can we find in the texts of Joshua and Judges accounts which need not be read as contradictory or primarily redactional, but as reflecting the styles of literary expression found elsewhere in the ancient Near East; whether in the annalistic accounts of wars of conquest by Egyptian and Mesopotamian rulers, or in the lists of cities 'conquered' by the pharaohs of Egypt throughout the second half of the 2nd millennium BC? The former suggest a literary approach which makes sense of the texts as we have

them by defining their form and motif as part of the language of war used throughout the ancient Near East. The latter suggest that the taking of cities such as are recorded in the Karnak list of Thutmose III did not imply their destruction (see J. K. Hoffmeier, 'Reconsidering Egypt's Part in the Termination of the Middle Bronze Age in Palestine', *Levanti* 21 [1989] and the literature cited there). The point is not that we have solved the problems of the descriptions of the accounts of Joshua and Judges, only that there are aspects of the literature which need to be considered before other options can be eliminated. Although Callaway did not have access to the most important synthesis on the subject (I. Finkelstein, *The Archaeology of the Israelite Settlement*, Israel Exploration Society, 1988), his treatment of the survey results which were in his possession suggests an awareness of the importance of this material for the topic at hand. After reviewing the three approaches to Israel's origin, conquest, peaceful infiltration, and peasant revolt, he concludes by noting aspects of village society in the archaeological and biblical evidence and by following the observations of Miller regarding the origins of Yahwism in the desert.

Lemaire's chapter on the United Monarchy returns to the style of Sarna, basically following the material of the text with occasional notes from extra-biblical sources. Accepting neither the extreme scepticism of a Garbini, with his rejection of the achievements of David's reign, nor the literalism which would overlook the problems of a local king such as Saul making war with the power of Aram, Lemaire argues for the importance of the records of the reigns of David and Solomon and for their fundamental basis in reality. For David's reign he focuses on the military achievements, and for Solomon's he studies the administrative achievements. Lemaire's own work in scribal schools and their presence in ancient Israel leads to an appreciation of the period as a time of literary creativity, though this as well as the use of Josephus and classical sources in reconstructing the parallel early history of Tyre seem overshadowed by the comprehensive catalogue of relevant archaeological finds from Palestine which are relevant to the period. But this is only a reflection of the quantitative differences between the two.

Horn's chapter on the divided monarchy is the longest in the book. It follows the style of Lemaire with summaries of the biblical narrative, including the addition of relevant archaeological and epigraphical finds. Photographs of many of the important texts supplement the text, though it is not clear if the photograph of the Siloam inscription is of the original which is in the museum in Istanbul (a fact mentioned in the text), not in the Israel museum, the source of the photo used. Horn accepts two campaigns for Sennacherib and the historicity of Manasseh's repentance. His

observations on the Arad sanctuary destruction levels must now take into consideration the redating of the evidence by D. Ussishkin, 'The Date of the Judean Shrine at Arad', *Israel Exploration Journal* 38 (1988), pp. 142-157. An additional reason for Josiah's confrontation of Necho at Megiddo could have been his desire to reincorporate the Northern Kingdom into a new United Kingdom. Thus Necho's move would be interpreted by Josiah as a territorial invasion. Horn incorporates significant epigraphic finds as well as the archaeological materials. However, the focus is political and religious rather than social or economic. Thus neither the major olive oil processing installations at Miqne (Ekron) nor the interpretations of Israelite society suggested by the Samaria ostraca receive much attention.

Purvis' chapter on the exilic period and the subsequent return begins with the biblical text and supplements it with discussions from the Elephantine papyri, the seals, and what is known of Persian history. He raises the question of the existence of a synagogue or perhaps a temple in Babylonia during this period. Of course, the Samaritans and their origins are considered here. In comparison with the preceding period, the archaeological evidence for this period in Palestine is meagre. Part of the problem may have to do with the relatively small percentage of the population of Judah which actually went into exile. This suggests more of a continuity with the preceding period for much of the land than previously supposed.

The final two chapters lie beyond the OT period. Levine's chapter on Hellenism observes later wisdom literature as a means of coming to terms with this distinct culture. They bring us through the period of Jesus Christ, with three pages devoted to the Pharisees, Sadducees, and Essenes, and almost one page devoted to Christians. No attempt is made to incorporate the material in the later chapters of Daniel. Neither Levine nor Cohen deal with the canon's formation.

A modern study of the history of ancient Israel must take into account the increasing variety of methods and evidence used in the study. An introductory survey must encounter readers with little or no biblical background and provide them with some sort of intelligible account of the ideas and people involved. The volume under consideration seems suitably designed to achieve this. The criticisms made here are not intended as peripheral but neither do they vitiate the work's value as a means of introducing a new generation of readers into the fascinating world of ancient Israel's history and how scholars try to recover it. Do we need another history of Israel? Probably not, unless it provides a new synthesis to broaden the minds of scholars, or reworks the existing interpretation so as to make it available to the general reader.

R. S. Hess

Book Reviews

John D. W. Watts, *Isaiah 1-33* (Word Biblical Commentary, Vol. 24; Waco, Texas: Word Books, 1985), lvii + 449p pp., \$24.95.

This is the first of a two-volume series (second volume published in 1987) covering the entire book of Isaiah and, as the division at Isaiah 33 indicates, does not present a conventional approach. The 'Editorial Preface' describes this commentary series as expressing an 'evangelical' perspective and endeavouring to meet the needs of 'the fledgling student, the working minister as well as . . . the colleagues in the guild of professional scholars and teachers' (p. xiii). Though people who are very conservative may question whether this is an evangelical commentary (for example, Watts believes Sodom and Gomorrah were not historical places but etiological (legendary) references (p. 19)), it should probably be regarded as such. It is,

however, doubtful that a fledgling student or a busy minister will profit greatly by a commentary which is so unusual in its handling of the book of Isaiah, especially since Watts' approach is based upon such weak evidence.

Watts considers the book of Isaiah to be an example of drama which was intended for oral presentation; therefore he assumes various speakers ('herald' (1:2); 'heavens and earth' (1:4); 'people of Jerusalem' (1:9); etc.). Watts himself admits that his recognition of the speakers is sometimes quite arbitrary (p. 3). He divides the book of Isaiah into twelve acts using historical periods (chs. 1-6 (Uzziah/Jotham); chs. 7-14 (Ahaz); etc.) (p. li). For five pages (xlv-xlix) Watts discusses the possibility of drama in ancient Israel (where his strongest evidence is drawn from Greek literary style) but is unable to demonstrate that the book of Isaiah is written in this form.

Watts' major contention is that the book of Isaiah is a drama composed c. 435 BC, which he believes would be late enough to include all historical references in the book (p. xxiv). Watts arrives at this date by examining Isaiah 63:1-6 which appears to speak of the

recent destruction of Edom. He argues that before the exile, Edom regularly occurs as one of Judah's neighbours (e.g. Is. 11:14; Je. 9:26; 25:21) but Ezra and Nehemiah do not mention Edom (cf. Ezr. 9:1); thus he assumes that Edom was no longer a threat by 450 BC. One might hope for stronger evidence upon which to build such an important part of one's interpretation.

Some recent scholars (R. E. Clements (*Interp* 36 (1982), pp. 117-129; *JSOT* 31 (1985), pp. 95-113); J. J. M. Roberts (*Interp* 36 (1982), pp. 130-143)) have suggested a theological unity of Isaiah; but Watts argues for a literary unity (i.e. that the whole book was written for a fifth-century audience). Several other recent scholars (such as H. Barth (*Jesaja-worte*, 1977), R. E. Clements (*Isaiah 1-39*, 1982), and J. Vermeylen (*Du prophète Isaïe à l'apocalyptique*, 1977)) have suggested a later rereading of the Isaianic tradition, but no-one has suggested so late a date which redirects the whole purpose of the book of Isaiah. It is unclear what the evidence is for Watts' interpretation since a normal reading of Isaiah gives no indication that this book was being directed toward a fifth-century audience.

To his credit, Watts has done a superb job with the textual notes. He seems to have handled the material fairly and explains it clearly so that a non-specialist can understand the evidence. Watts generally favours the MT, unlike his teacher H. Wildberger who generally emends the text (e.g. Watts retains Is. 8:23a (pp. 129-130) while Wildberger assumes it is a gloss (1982, 1, p. 356), Watts rejects Wildberger's emendations at 8:23b and 9:3 (1982, 1, p. 364) as unnecessary).

The bibliography at the beginning of each section is very helpful and the surnames in bold print make it much easier to use than Wildberger's commentary, but there are many mistakes in the bibliographies so that they need to be used with care: e.g. misspelled names — Hoffmann (pp. 11, 30, 43); Lipiński, É. (p. 47); Westermann (p. 57); Jeppesen (p. 125); Sages (p. 186); misspelled words — rhythimical (p. 11); *messianum* (p. 129); incorrect page numbers — Jones, *SJT* 21 (1968), pp. 320-329 (p. 22); Gerstenberger, *JBL* 81 (1962), pp. 249-263 (p. 57). The publication of an errata sheet would be helpful here.

Other reservations concerning this commentary include: 1. Most examples of what Watts terms 'arches' (p. 15) are rather 'sagging' because they are so forced (see especially pp. 15, 24, 33-34, 185, 195). 2. Watts' interpretations often colour his exegesis. This can be seen in several passages but two are particularly noteworthy: (a) In Isaiah 10:33-34 Watts is so convinced that this passage is speaking about Judah that he interprets it as referring to God as a divine gardener pruning the wickedness from Judah (see pp. 165f.). But it has been convincingly argued (by Clements, NCB 1982, p. 121, and Høgenhaven, 1988, pp. 122f.) that this passage describes a terrifying destruction rather than an awe-inspiring pruning; (b) In Isaiah 9:3-5 Watts understands the *kl* as a conditional particle. This is possible, but the context and the majority of scholars appear to go against Watts' suggestion. Surely the *kis* in these verses give three reasons for the joy mentioned in verse 2 (3) (cf. Gesenius, *Hebrew Grammar*, para. 158b; R. J. Williams, *Hebrew Syntax*, 1976, p. 72) and a conditional element would be completely foreign to the context.

This commentary contains some very helpful information but one must read it cautiously, especially with regard to his approach and suggested 'arches'. For those who have limited financial resources this would not be a commentary I would recommend.

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P. R. Ackroyd, Studies in the Religious Tradition of the Old Testament (London: SCM, 1987), xiv + 305 pp., £12.50 pb.

Peter Ackroyd was formerly Samuel Davidson Professor of Old Testament at London University. Perhaps known best for his translation of the massive *Introduction* by Eissfeldt, he is respected by all who know him as a scholar of wide knowledge of the whole OT field and for his penetrating thought and balanced judgment. It is therefore very pleasing to have this collection of essays which date from 1961 to 1986 dealing with a theme which has interested

Professor Ackroyd throughout that time: continuity within the religious tradition of the OT.

There are three parts to the book. The first deals with continuity and discontinuity. In other words he investigates the way in which the religion of Israel changed during its long history and in what way it remained the same. At different times in Israel's history there were different emphases: new insights were gained, tension was perceived between certain aspects of their faith and experience. How did they relate their received traditions to new situations that arose? This is one of the central questions to which Professor Ackroyd offers some answers.

The second part of the book is entitled 'Aspects of the Prophetic Tradition'. It concentrates on the book of Isaiah, chapters 1-12 and 36-39, with special attention to the way the material is slanted in the text that we have. Ahaz and Hezekiah feature prominently. Part three contains four essays that relate to the canon: 'A judgment narrative between Kings and Chronicles? An Approach to Amos 7:9-17' (which suggests that the brief story of Amos's confrontation with Amaziah may be part of a longer narrative concerning the judgment of the Northern Kingdom); 'The Open Canon'; and 'Original Text and Canonical Text'. The Epilogue, the only new essay in the collection, is entitled 'The Old Testament Religious Tradition: Unity and Change'. Professor Ackroyd attempts to look at what happens in living religions when they both attempt to remain true to their roots while living through changing circumstances. He concludes: 'However much we may hope or believe that we are maintaining the past, even recovering the primitive, we are in fact making the adjustments without which faith ceases to be real.'

It is not an easy book to read. The style is concise and the reader is obliged quite frequently to refer to biblical references and to weigh carefully what is said. It will be more difficult, too, for evangelical students who do not accept his presuppositions. For instance, they may feel that he assumes a large body of editors at work on the biblical text, who were more concerned to get their own theological point across than to preserve a reliable record of Israel's history and experience. If that is how the book strikes you — persevere! As long as you do not feel compelled to deny editorial activity or to regard editors as uninspired, there is a great deal to be learned from this book.

Mike Butterworth, Oak Hill College.

Pinchas Lapide, The Sermon on the Mount: Utopia or Program for Action? (ET, Maryknoll, NY: Orbis Books, 1986), vii + 148 pp., \$9.95.

Carl G. Vaught, The Sermon on the Mount: A Theological Interpretation (Albany, NY: State University of New York Press, 1986), xiv + 217 pp., \$34.50/\$9.95 (pb).

For those who are interested in the Sermon on the Mount (SM), here are two new books which purport to contribute to a more accurate reading of a text that has evoked a flood of scholarly interest. Both books suggest very different solutions to the theological impasse in NT scholarship with regard to these three chapters in Matthew's gospel.

Lapide's book is a reading of the SM 'by Jewish eyes through Hebrew lenses' (p. 11). The author, an orthodox Jew, is one of a number of Jewish scholars who have shown a keen interest in rediscovering the Jewishness of Jesus. Accordingly, Lapide argues that the SM has been fundamentally misunderstood due to a neglect of the Jewish context in which Jesus lived. Examined from this Jewish perspective, the Sermon's teachings are not new, but simply a 'Torah exegesis' by the Nazarene (p. 15). Therefore, everything that Jesus says can be paralleled in rabbinic literature which was available to any first-century Jew. As a result, Jesus' teachings should not be pitted against the very religious heritage of which he is a part, but rather they represent its very quintessence.

Lapide, with one eye on first-century Judaism and the other on the SM, reconstructs the Jewish context of Jesus. The work is divided into three parts. The first deals with introductory matters and with what Lapide calls the 'preamble' (Mt. 5:1-2, 17-20). There are several indicators in these two pericopes which profoundly signal Jesus' Jewishness. The mountain motif — Jesus sits like a Rabbi and does not stand to teach his disciples — and Jesus' conscious alignment of himself with the Torah of Sinai in Mt. 5:17-19, all point positively toward Judaism rather than viewing it antithetically. Typical of Lapide's passion to interpret Jesus as a Jew, he writes of Mt. 5:17-19, 'In all rabbinic literature I know of no more unequivocal, fiery acknowledgement of Israel's holy scripture than this opening to the Instruction on the Mount' (p. 14).

In the second part of the book the Beatitudes are compared to Ex. 20:2. Just as the Exodus text forms the basis for what follows in the Decalogue, the Beatitudes demonstrate God's compassion toward all fellow humans, thus paving the way for the instruction which follows. The third largest part of the book is dedicated to a more detailed study of what has traditionally been referred to as the six antitheses of the SM (Mt. 5:21-48). In an attempt to reconstruct a more accurate context, Lapide continually translates the Greek text into Galilean Aramaic or Hebrew of the first century. He maintains that at several crucial points the Greek text is ambiguous. For example, in the six so-called antitheses where the Greek text puts *ego de lego* on the lips of Jesus, Lapide hypothesizes that the Hebrew equivalent would have been *va ami omer lachem*. Thus, 'but I say to you' becomes 'and I say to you'. Thereby Jesus cannot be accused of antinomism, but rather should be seen as one who elucidates the Torah. Accordingly the so-called antitheses are more accurately 'supertheses' (p. 46).

This book is not only a welcome contribution to the ongoing process of understanding the SM, but is also significant because it allows students to sharpen their focus on the historical Jesus. Too often Protestant scholarship has produced a Protestant Jesus who speaks and looks like another Reformer. However, the fact is that Jesus was a Jew, he thought and spoke like a Jew, and therefore the primary context which must be reconstructed for understanding him must be Jewish. Lapide, as a Jew, has done that in a fresh and very readable book which does not shy away from present-day application. The only limiting factor of the book is its brevity. Mt. 6 and 7 are omitted from the study, and oddly no explanation is given for their omission.

As the subtitle indicates, Carl Vaught's book, *The Sermon on the Mount: A Theological Interpretation*, is a very different work from that of Lapide. Vaught bemoans the fact that most biblical exegesis loses the text in its insatiable desire 'to analyze the components and the strata from which the interpreter insists that it has been constructed' (p. xiii). Therefore, he proposes a 'theological interpretation'. This is defined as an interpretation which assumes the larger biblical context. Vaught writes, 'This commentary is theological just insofar as it brings the theological dimension of the other gospels and epistles to bear upon what might appear to be an essentially ethical, social, or political document' (p. xi).

The hermeneutical key, according to Vaught, which unlocks the meaning of the SM is 'divine perfection'. Hence, Mt. 5:48 becomes the paradigm through which the entire SM is seen and Jesus' demand for divine perfection is the overarching theological theme which unites the various themes in the Sermon. This fits neatly with Vaught's 'theological interpretation'. Thus the focus becomes primarily theo-logical rather than anthropological. Instead of interpreting the Sermon as a collection of unrealistic ethical demands made on humankind, the starting-point is the divine perfection which was originally embodied by the preacher of the Sermon. Perfection is simply defined as Christian maturity; 'what we were meant to become; to be perfect is to be mature; to be perfect is to reach the stage of mature self-development that moves beyond the initial moment of Christian commitment into the fullness of life to which Christ calls his followers' (p. 117).

With this schema, Vaught approaches the text in order to expound his 'correct' interpretation of the SM. He divides the Sermon into four parts. The first includes the prologue (5:1-2), the Beatitudes (5:3-12) and the metaphors of salt and light (5:13-16). In this section, the context of the SM is given and inner and outward conditions for divine perfection are outlined. In the second part, the author deals with five practical problems which impede divine perfection.

Strangely, Vaught only acknowledges five issues in Mt. 5:21-48, while the text clearly contains six separate introductory formulas signalling six issues rather than five. Adultery and divorce are lumped together as one. Vaught would have done better to follow the obvious division which arises out of the text. The third part deals with 'six expressions of perfection' (6:1-7:6). In this portion of the Sermon, Jesus outlines in very practical terms how divine perfection is expressed in everyday life. Finally, the fourth part of the book deals with the remaining portion of the Sermon (7:7-29) under the title of 'Final Considerations About God's Kingdom'. In other words, like most other works, this one as well struggles with how to understand the miscellaneous details which are in the latter part of the Sermon.

Vaught's book reads like an extended sermon. In fact this genre might be dubbed 'philosophical devotional exegesis'. Without doubt the philosophical overtones are present as Vaught himself warns, being a philosophy professor. That in itself makes the book interesting reading. It is necessary that laypersons take their turn at biblical interpretation. The Bible is too important to leave its interpretation solely in the hands of the professional theologian. To its detriment, however, the book contains a number of obvious errors which could have been avoided with a more careful handling of the text. For example, commenting on Mt. 5:3, Vaught makes a point on the *future* tense of the verb when in reality it is present.

Both books are an attempt at the 'correct' interpretation of the SM like all previous books on the topic. Neither is definitive, but each makes a contribution toward that goal. Many insights can be gleaned from both, but Lapide's book is by far the more interesting and more significant in the process of understanding Jesus' powerful message contained in the SM and is highly recommended.

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Darrell L. Bock, *Proclamation From Prophecy and Pattern: Lucan Old Testament Christology* (Sheffield: JSOT Press, 1987), 413 pp., £10.50/\$15.95 (pb).

This is a revision of an Aberdeen PhD first submitted in 1982. As befits such study, Bock begins with a chapter which surveys what has been said in the field, focusing particular questions which he wishes to address. These concern the type and source of the christologically-focused text citations or allusions, their *tradition-history*, their *hermeneutics* (the relation of Lucan contextual meaning of the citation to the OT meaning and to the event to which it is applied), and the *redactional purpose* of the use of the OT citations or allusions, particularly their significance in the thematic development or framework of Luke-Acts as a whole (and hence Bock's analysis follows the order of Luke-Acts, with chapters on the OT christology of Lk. 1-2; 3-24; Acts 2-5 and 7-13, rather than analysing by *literary source* or by *type* of OT usage). The very wide scope of Bock's study makes for a thesis that is not easy on the reader — it is not only very terse, but also a wood in which it is all too easy to become lost for the thick growth of trees.

Perhaps it is the sheer breadth of scope of the enterprise which is chiefly responsible for some of the weaknesses of the work. These include (i) some strange omissions (*e.g.* of reaction to Jervell's important theses that 'in your [Abraham's] seed' at Acts 3:25 refers to the Jewish Christian *church*, not to Christ; indeed Jervell makes no appearance either in bibliography or indices!); (ii) places where opposing arguments seemed to be less than felicitously represented and patiently heard (who would have guessed, for example, the strength of the case that Acts 2:33 depends on Ps. 68:18, and Jewish traditions of interpreting it, from Bock's presentation of Lindars and Dupont, and these virtually alone (181f.??)); (iii) occasional places where previous work is entirely or seriously misrepresented (see Bock's handling of the reviewer's own positions at almost every point!); (iv) some very strained argument (*e.g.* that used to assert that the Father-Son relation is not a crucial or emphasized Lucan christological motif, and to justify the claim that the inclusion of Is.

58:6 in Lk. 4:18-19 turns the focus of the whole from an Isaianic Servant Christology to a regal messianic one); and (v) what appears to be a slightly unannounced Jewish messianology (e.g. in the assumption that the Mosaic Prophet was neither messianic nor royal in Judaism, and that a merely real Messiah (except in 1 Enoch) was sufficiently widely expected to make it worth Luke showing Jesus was more than that (contrast Neusner *et al*, *Judaisms and their Messiahs!*). But such things are possibly inevitable, albeit regrettable, in a thesis that attempts to cover so much, and in such detail.

Positively, I think Bock has achieved some important results. First, it is commonplace to maintain that the theology of the speeches in Acts is based on the LXX and not the Semitic OT tradition, and to deduce that the speeches in Acts 2-13 cannot therefore be authentic. But Bock has argued that while it is the LXX that is usually (not exclusively) cited, nevertheless not once does the conceptual framework of the argument depend on use of the LXX as opposed to the Masoretic text (or other Semitic OT tradition). Some will think Bock has gone too far, but his thesis at least redresses the balance. Second, he elucidates a more controlled and subtle interface between OT passages and Luke's messianic interpretation of them than hitherto offered (e.g. he builds a much more plausible bridge of meaning than usual between Pss. 16 and 110 in the OT and their use by Peter in Acts 2). Third, time and again, Bock manages to demonstrate a clear prophecy-fulfilment motif: whether by showing the OT text had a prophetic overtone, or that a prophetic dimension to the passage was already overt in Jewish interpretation, or that Luke's text clearly indicates a fulfilment dimension (or in a combination of these). He shows that Luke does not use his OT citations as an apologetic device ('proof from prophecy') — *contra* Schubert: cf. the absence of Matthew's 'he did this to fulfil the scripture which said . . . ' and *contra* Rese who attempts to eliminate the prophecy-fulfilment aspect from so many citations, reducing them thereby to static OT illustrations of the NT events.

Fourth, he establishes that a major purpose of this use of OT passages is to proclaim and elucidate Jesus in terms of the fulfilment of God's promises made to Israel in the pattern of her redemptive history and in the prophecies given to her (hence the title). Fifth, he at least in part successfully argues that there is a christological development through the presentation of the OT citations in Luke-Acts. Jesus is at first identified as Messiah, then gradually shown to be more than a 'mere regal Messiah' (I would prefer to say the type of his messiahship is clarified): he is 'Son of Man', Servant, and especially *Lord* — the divine sense of the last only emerging most fully in Acts 2 and 10:36-42, and becoming the justification for mission beyond Israel; Jesus is Lord and judge of all.

Bock's work will rightly join Holtz and Rese as a standard work on Luke's use of the OT.

Max Turner, King's College, Aberdeen.

Thomas E. Schmidt, *Hostility to Wealth in the Synoptic Gospels* (Sheffield: JSOT Press, JSNT supplement 15, 1987), 250 pp., £18.00/\$27.00; £8.95/\$13.50.

Thomas Schmidt's work, which began life as a PhD, researched at Tyndale House, seeks to elucidate the attitude found in the synoptic gospels towards wealth and possessions.

Schmidt's thesis is strongly critical of the sociological method to which we have become accustomed in recent years. Theissen, Mealand and Gager, in differing ways, have sought to demonstrate that the primary determining factor giving rise to hostility to wealth was the socio-economic condition of the deprived communities responsible for the production of the synoptic gospels. Mealand comes in for special criticism on the grounds of his assumption of the theological diversity caused by progressive accommodation to the changing economic conditions assumed to be prevailing from AD 30 to 70. Schmidt questions that it really was civil unrest that led to the Jewish war. Issues such as overpopulation, the tax burden, natural disasters and class conflict have, according to Schmidt, been

exaggerated, leading to an over-negative appraisal of the socio-economic climate of the immediate pre-war years.

Schmidt prefers to elucidate the development of the ideological background. He classifies hostility to wealth into five different stages (p. 44): 1. Injustice = greed; 2. Injustice = wealth; 3. Wealth = injustice, so value something more than wealth; 4. Value something instead of wealth; 5. Devalue wealth in order to value something else. Whilst these stages are never neatly chronological, Schmidt does discern a development up to the point of the writing of the gospels.

Beginning with the ancient Near Eastern material, Schmidt describes a reciprocal community in which charitable behaviour towards the poor was seen as the religious-ethical duty of the rulers, without ever threatening the social structures of that society. No conception of social equality is to be discerned. Turning to the Jewish canonical material, Schmidt points out a certain satisfaction with the status quo along with the ideas of wealth as a confirmation of God's covenant in response to justice and acknowledgment of him. Here, however, we do find developments towards the idea that 'Wisdom is better than gold': this in the writings not of the poor agrarian masses, but of the political, religious and economic hierarchy. The intertestamental writings reflect a time of less apparent control over the texts by the ruling aristocracy, but with no corresponding increase of interest in economic factors. Philo consistently expresses hostility to wealth but not out of sympathy for oppressed Jews. At Qumran references to themselves as 'poor' cannot be taken as reflecting economic need but a particular religious self-awareness. With this Schmidt completes his survey of the developing tradition, concluding that hostility to wealth is largely a preserve of the upper classes and quite independent of socio-economic conditions.

Schmidt's treatment of the synoptic material reveals considerable parallels with the Wisdom tradition. Here he finds a consistent lack of substantial sympathy for the poor, alongside a teleological devaluation of wealth. Even Luke is found to present dispossession not as a means of relieving the poor but as a way of expressing faith and trust in God. Schmidt finds little evidence that socio-economic conditions influenced the gospel writers in their attitude to wealth. In one remarkable passage (pp. 119-120), based upon 2 Cor. 8:9, Schmidt gives hesitant support to the theory that Jesus himself was not poor but hailed originally from an upper middle class background. All of which fits the theory already embraced. . . .

A counterblast to sociologically oriented treatments of the NT was overdue and we must be grateful to Schmidt for this stimulating and indeed provocative work. It is, nevertheless, sometimes difficult to accept his thesis. The lines of development are less easy to discern than he would have us believe. Moreover, he tends to play down the sheer severity of prophetic denunciations. Lk. 6:20 and 7:22 amongst other texts might be understood differently. Perhaps the major weakness is the concentration solely upon sayings with a clear statement about wealth, failing to balance them against other statements concerning the poor. Furthermore the thesis lacks a unifying appreciation of the new state of affairs brought about by Jesus' preaching of the kingdom of God.

So the thesis fails finally to convince, but does provide a useful corrective to the approaches it seeks to attack.

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Rabbi M. Hilton and Fr. G. Marshall, *The Gospels and Rabbinic Judaism: a Study Guide* (London: SCM, 1988), viii + 167 pp., £6.95.

This book arose out of the experience of discussion groups conducted jointly by a Jewish rabbi and a Roman Catholic priest. It consists of seven 'units' designed to form the basis for similar groups where Jews and Christians wish to come together to discuss their common roots and the reasons for their differences.

The subjects studied include the more theological ('The Great Commandment'; 'Who Can Forgive?'), the historical ('The Synagogue'), the literary ('The parable and the *Masha!*'), and the halachic ('The Ox in the Pit'; '*Shabbat!*'; 'Divorce'). More

fundamental issues which separate the two religious traditions, such as Messiahship, salvation or resurrection, are deliberately omitted.

The Hebrew Scriptures are taken for granted as common ground. The passages for study are drawn from the texts written after Christianity and Judaism began along their separate roads, from rabbinic Jewish writings and from the NT. It is assumed that those who use the book will need some fairly elementary instruction on the basics of the other religious tradition. This means that the Christian reader is likely to find the comments on the chosen gospel passages brief and unsophisticated (and, for me at least, often questionable!), but may expect to find the Jewish material more useful.

Rabbinic literature is a notoriously difficult and treacherous area for the non-specialist to explore, and not a few NT scholars have been rightly chided for ill-informed or simplistic use of Jewish 'background' material. Introductions to rabbinic literature are typically complex and forbidding, full of obscure words half understood. This book will not give you a comprehensive grasp of rabbinic scholarship, but Rabbi Hilton's considerate guidance through a number of specific issues and texts offers a more 'user-friendly' introduction. The result should be a more sympathetic awareness of what the rabbis were trying to achieve, once we realize that their apparently illogical arguments derive from a context of shared traditions to which we come as outsiders. It is a pity, however, that the nature of the project means that we are presented only with short extracts from both rabbinic and NT writings, and have no opportunity to get the feel of these writings as literary wholes.

The book is not designed primarily for solo reading, but for group discussion. I am not sure how many *Themelios* readers are likely to be able, or wish, to join or to initiate the sort of open-ended Jewish-Christian dialogue here envisaged. The authors see it as a process of mutual discovery and understanding, and discourage any idea that either religious tradition is 'better' than the other, still less any desire to win converts. They expect the process of dialogue to be surprising, and often painful, and encourage the users of the book to bring their suspicions and prejudices into the open. If the book is used as they intend, the result is likely to be more in the area of exploring the attitudes and relationships of those involved than in any 'objective' understanding of the texts studied.

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A. J. M. Wedderburn, Baptism and Resurrection: Studies in Pauline Theology against its Graeco-Roman Background (Wissenschaftliche Untersuchungen zum Neuen Testament 44; Tübingen: J. C. B. Mohr, 1987), x + 487 pp., DM 138.

Dr Wedderburn, who lectures in NT in the University of St Andrews, here presents the fruits of his post-doctoral research, the kind of work which would be worthy of a DD or similar degree. His book is a work of considerable erudition and complexity, displaying mastery of a vast range of ancient literature, critical analysis of modern scholarship, and discussion of areas unfamiliar to many students. It is not, therefore, likely to offer easy reading to the clientele of *Themelios*, although the author does his best to guide the reader with helpful signposts and summaries.

The theme, broadly speaking, is the possible influence of the Mysteries upon Pauline theology. Wedderburn argues that the Mysteries were indeed alive and well in the first century; he denies that there was a common theology to them, and he claims that some of their language was broadly known and used in the first century. Hence the possibility of influence on early Christians (such as the Corinthian church) and on Paul himself from ideas originally at home in the Mysteries cannot be ruled out.

The particular area in which such influences have been seen is that of Christian baptism, with the associated ideas of union with Christ, sharing in his death and resurrection, and living in the Spirit. There is

a widespread view that when we read of believers sharing now in Christ's resurrection (Eph. 2:6; Col. 2:12; 3:1) this idea is derived from that of sharing in the experience of a dying and rising god in the Mysteries. Wedderburn argues against this view in three ways. First, he claims that in Rom. 6 Paul never speaks of sharing now in Christ's resurrection but only of sharing in his death. Second, the Corinthians who did not believe in a future resurrection are sometimes held to have believed that they had already experienced resurrection (2 Tim. 2:18), but Wedderburn argues that this is an unlikely interpretation. Third, the language of resurrection is strongly associated with the physical body in contemporary Judaism and Hellenism. Later spiritualization of the idea was due to reinterpretation of Christian ideas.

Wedderburn enquires in detail whether the deities in the Mysteries did experience death and resurrection and asks how salvation was gained in these religions. He shows that hope rested on the power of the deities rather than upon sharing in their experiences. On the other hand, he finds a more plausible explanation of the Christian understanding in terms of Jewish ideas which suggested some kind of solidarity with Christ. It is true that Christian initiation involved some kind of anticipation of the future, but this was through proclamation of what Christ had done rather than through any kind of ritual re-enactment of it.

What, then, of the experience of the Spirit, especially in the lively forms current in Corinth? Wedderburn does find some links here to Graeco-Roman ecstatic experiences, particularly in prophecy, but again the main background is Jewish. It was a sign of spiritual life but not of anything that amounted to resurrection.

Nevertheless, despite this massive criticism of the concept of influence from the Mysteries, Wedderburn does allow that Paul must have spoken in terms that made sense to his Hellenistic readers. He argues that the concept of 'dying in order to become' was widespread in the ancient world, particularly in rites of passage, and is found both in the Mysteries and in Christian initiation. The Christians drew it from the general stock of ideas available to them.

How, then, did the idea that Christians have already risen with Christ develop? Wedderburn argues that it was a logical step to move from the 'asymmetry' of Rom. 6, where Christians already share in the death of Jesus but not in his resurrection, to a more symmetrical kind of statement in Eph. and Col., particularly in view of the way in which the concept of life or new life was already firmly established as a description of the nature of salvation.

The scholarly caution with which this book is written and the care with which different positions are fairly assessed may hide the impact of the argument as a whole. In his concluding sentence Wedderburn claims 'to have set a large warning sign at the entry to... a "dead-end" in Pauline studies, the interpretation of Paul's doctrine of union with Christ as derivative from the mystery cults of his day'. It would require a specialist in Graeco-Roman religions to assess his arguments properly, but to the present reviewer it seems that Wedderburn has not merely erected a warning sign: rather he has very effectively sealed off this *cul de sac*.

Wedderburn admits that he has not solved the problem of what exactly the Corinthians meant by their denial of the future resurrection. Where I find him not altogether persuasive is in his denial that a present resurrection experience can be found in Rom. 6. The matter may simply be one of terminology. Paul does not apply the actual language of resurrection to believers, since it had strong physical associations and therefore he reserves it for the future experience of bodily resurrection. Yet he does draw a clear parallel between the resurrection of Jesus and the fact that Christians 'walk in newness of life', and 'shall live' with him. He speaks of Christ being raised 'from the dead' and believers being alive 'from the dead'. I am not entirely convinced by Wedderburn's attempt to play down this parallelism. Nor is it clear that the main force of his argument depends upon establishing this point. For if dying with Christ cannot be derived from the Mysteries, neither can being raised with him.

There is room for more work on the question of the origin of the idea of union with Christ, together with the phrases 'with Christ' and 'in Christ', which Wedderburn explains in terms of the representative figure of the past with whom his descendants are linked and of their solidarity with him. He has himself made some important suggestions on this problem in two essays, 'The Body of Christ and

Related Concepts in 1 Corinthians', *SJT* 24 (1971), pp. 74-96, and 'Some Observations on Paul's Use of the Phrases "in Christ" and "with Christ"', *JSNT* 25 (1985), pp. 83-97, and it is much to be hoped that he will go on to follow these up with a fuller study of these often neglected phrases.

I. Howard Marshall, University of Aberdeen.

Klaus Wengst, *Pax Romana and the Peace of Jesus Christ* (ET, London: SCM, 1987), x + 245 pp., £8.50.

This interesting book, which lacks some of the material in the notes of the German edition (1986) but still offers only 143 pages of text, falls into two parts. The first analyses the *Pax Romana* in its military, political, economic, legal, cultural and religious aspects. A providential reading of history has long accustomed Christians to view the *Pax Romana* predominantly in terms of its advantages and benefits, as part of the *praeparatio evangelica*. Wengst's well-researched account shows that, although voices were not lacking to portray 'the whole earth . . . arrayed like a paradise' (Aelius Aristides), others saw things very differently. One such was the British chieftain Calgacus, to whom Tacitus attributes the speech that includes the famous charge against the Romans, 'They create a desert and call it peace.'

The second, longer, part of the book illustrates the spectrum of attitude and response in the early Jesus movement. In Jesus' 'other way' Wengst discerns 'a critical detachment from the rule exercised in his time, which secured the *Pax Romana* . . . this basic negative tone in the evaluation of the *Pax Romana*'. Paul experienced its guarantors as potential and often actual persecutors; history shaped by the *Pax Romana* is 'the history of death which Christ will break off'. The loyalty of Rom. 13 is that of the unassimilated alien and cannot be total.

Luke-Acts, on the other hand, depicts Rome and its representatives in an explicitly favourable light. Luke omits the violence practised in the *Pax Romana*. Although there are limits to Christian loyalty to ruling bodies, Luke demonstrates them only in connection with Jewish, never Roman, authorities. In similar fashion, the author of *I Clement* shows himself to be 'a theological apologist for the *Pax Romana*'. As a Roman he identifies with his fatherland and keeps quiet about persecutions, which he blames on the Christians' own misdemeanours. Very different is Clement's contemporary, John of the Apocalypse, who depicts Rome and its actions in unreservedly dark tones.

The fact that the two texts closest in time, Revelation and *I Clement* (for which Wengst does not consider a pre-70 date), display the most widely divergent positions emphasizes the importance of standpoints and perspectives. It does not prevent Wengst concluding from some reflections on Heb. 11 that Christians have no alternative than 'to go out from the fortress of a policy of security which is already fatal'. But the variety of stance that he finds in the documents can scarcely allow for such a singleminded message.

Students of Christian beginnings as well as those concerned to work out a biblical political theology will find much illumination in this book. Wengst is an attractive writer, with a suggestive rather than dogmatic style. His comments on the taxation question (Mk. 12:13-17), for example, are penetrating. But I felt that this exegesis from time to time appeared strained in the direction of an over-politicized reading of the text. Do 1 Cor. 15:24-26 speak inescapably of Roman power? The 'peace with God' of Rom. 5:1 may be more than mere 'peace of the heart', but Wengst's explanation is tortuous and not readily preachable. The treatment of Paul in such a book is bound to be the acid test, given Rom. 13:1-7. If Wengst is not wholly persuasive about Paul, his work remains a valuable exercise in contextualizing Jesus and a selection of early Christian writings.

D. F. Wright, New College, Edinburgh.

John Bowden, *Jesus: The Unanswered Questions* (London: SCM Press, 1988), xxi + 259 pp., £9.50.

Some Christians enjoy being sceptical about Christian belief, and perhaps it is not surprising that the editor of SCM Press should be one of them. Books he has published naturally feature prominently among the sources from which he builds up a case against, not only traditional Christology in the narrow sense, but the role which the figure of Jesus Christ has hitherto played in Christianity. Other books he has published take account of the kinds of difficulties he raises but find them no obstacle to positive christological construction, but these books — by, for example, Pannenberg, Moltmann, Sobrino, even Bowden's friend Schillebeeckx — seem to have influenced his thinking about Jesus not a jot. He attempts to forestall criticism by admitting (in contradiction of his subtitle) that many of his questions have been 'discussed, and indeed answered, with enormous sophistication elsewhere, at very great length, and scholars will inevitably criticize me for being naive and superficial. My problem, however, is that I do not even begin to understand many of these complex, long, sophisticated (and also, it has to be said, often mutually contradictory) explanations' (p. 12: my italics). This is surely a little disingenuous: to know that complex explanations are mutually contradictory one needs to understand them rather well! It is true that 'there is never any harm in simple questions' (p. 12). But it is also possible for simple questions to become a kind of stubborn scepticism which will not attempt that kind of looking at a whole matter from another angle which satisfying answers to simple questions often require.

My criticism of this book is certainly not that it asks questions. Asking honest, difficult, disturbing questions is essential to the health of theology and the church. To 'suppose such questions is indeed, as Bowden charges, to turn Christianity into an ideology. So I had hoped to find in this book at least an agenda for contemporary Christology — a set of questions sharply and insightfully posed. But I was disappointed. In fact the book does not really ask questions, but sketches broad areas of disquiet liberally scattered with often too-confident assertions. Of course, extremely important issues are raised, such as in the chapter entitled 'Can Jesus be Everyman?' (This title is curiously question-begging, in view of the fact that feminist difficulties about a male Saviour are a major ingredient in its topic.) But this chapter turns out to be a confused tangle of issues which are never properly analysed and defined. They are issues which really do need much more careful attention than they have usually received in Christology, but this chapter will not do much to promote that. The impression throughout the book is that Bowden already has his own answer to all his 'questions' — that the role of Jesus in Christianity must be radically reduced — and is no longer interested in the possibility of other answers.

In Bowden's valiant attempt to end on a positive note Jesus scarcely features at all. That his questioning takes place in the context of real faith in God is clear, but that it is any longer a questioning faith in Jesus seems very doubtful. Perhaps the most extraordinary aspect of the book is the author's apparent conviction that the decline of Christianity in the West can be arrested by this kind of resolute scepticism about Jesus and Christology (p. 196).

Richard Bauckham, University of Manchester.

Lucas Grollenberg, *Unexpected Messiah: or How the Bible can be Misleading* (ET, London: SCM, 1988), viii + 196 pp., £6.95.

I have had difficulty deciding where in my shelves this book should go. Ch. 2 belongs under intertestamental Judaism, ch. 3 under lives of Jesus, ch. 4 under Christian origins, ch. 5 under NT use of the OT, ch. 6 under Christian anti-Judaism, and chs. 1 and 7 under christology. And all this in a modest paperback!

Its subject is essentially how far Jesus fitted into or conflicted with Jewish messianic expectations, and why his followers found it

necessary to distance themselves increasingly from Judaism. Father Grollenberg is painfully aware that Christians traditionally appeal to the OT in ways which conflict with most modern scholars' views on the meaning of the text and also that their adoption of the Jewish scriptures into the Christian Bible not only offends Jews, but also promotes a superior and judgmental approach to Judaism which is easily exploited by anti-Jewish prejudice. He writes explicitly as a Christian, but, on this issue at least, an embarrassed one.

His sixth chapter ('The New Scriptures: Anti-Jewish?') considers at some length the impression given by John, Matthew, Paul and Luke. His analysis of the data would form a useful non-technical basis for discussion of the increasingly fashionable subject of NT 'anti-Judaism'. His own conclusions are clear and provocative. The attitudes he discovers lead him to apologize for the existence of such 'anti-Jewish' texts. We would be better off without the NT canon, and must not speak of the 'inspiration' of such writings in such a way that God is implicated in their objectionable views.

Yet he declares himself a convinced Christian (it is 'in his genes!'), and so feels it necessary to undertake the task, so familiar now in Jewish-Christian discussion, of dissociating Jesus from his embarrassing followers. The final chapter is revealingly entitled 'Jesus. A "Fulfillment" Nevertheless'. He cannot accept the old uncritical idea of Jesus literally fulfilling prophetic predictions, but finds 'fulfilment' more in what has traditionally been known as typology: he prefers to speak of some OT figures as 'preliminary studies'. He offers as examples Jeremiah (especially in his polemic against the temple), Ps. 22, the son of God figure (especially as developed in Wisdom 2), and the calling of Israel to be the source of Yahweh's blessing to the nations. Such models find their fulfilment in Jesus and his movement, despite the tendency of many of its earliest spokesmen to develop the idea of fulfilment in ways which Jesus, the preacher of love, could not have countenanced.

Behind this conclusion lies a readable and generally well-compiled account (in chs. 2-4) of Christian beginnings in the context of Jewish expectations. Grollenberg is not one of those who make Jesus so unremarkable that one wonders why anyone ever noticed him. He was surprising, challenging, unique. The divisions among Jews which resulted from his ministry are sensitively and believably analysed.

He goes on to argue that the use of the Jewish scriptures by Jesus' followers is not out of place in an age which produced the Book of Jubilees, the allegories of Philo, and the growth of midrash. The first Christians, like other Jews, 'read their sacred texts in terms of their own situation and their own faith and expectations, without being concerned with the question what the biblical authors might have meant by their words in their own day' (126). Unfortunately, we can no longer read the OT like that.

The whole book raises important questions in a clear-sighted and arresting way. If we do not like Grollenberg's conclusions, it is up to us to show where his analysis of the early Christian movement is at fault — or, if he is right, to explain how the NT outlook on the people of God can be held acceptably in the light of literary criticism and of the fear of anti-Jewish prejudice.

So where will it go on my shelves? In the section on the NT's use of the OT, because it is there that his problems begin and his most significant argument is focused.

Dick France, Wycliffe Hall, Oxford.

R. P. C. Hanson, *Studies in Christian Antiquity* (Edinburgh: T. & T. Clark, 1985), 394 pp., £17.50.

This is a wide-ranging series of essays on the early church, all but one of which have previously appeared at different times in varied journals. Professor Hanson claims with justice that each essay makes a positive contribution in its own sphere. Sometimes the issue in focus is a precise textual point; while at the other end of the spectrum we find subjects as vast as the attitude of Christianity to Graeco-Roman paganism and the formulation of Trinitarian doctrine up to the end of the fourth century. The collection is no antiquarian's

indulgence. Several themes — e.g. ministry, the eucharist and the *Filioque* clause — play a central part in modern ecumenical debate.

We cannot expect such varied essays to have a unified theme, but all alike are infused with the confidence that a reasonably accurate picture of Christian origins is possible and worthy of our understanding. Hanson has no time for historical scepticism, particularly when it is arbitrarily confined to writers prior to the Enlightenment. He sees this as spelling the death-knell of Christianity, and can draw on his knowledge of ancient historians like Herodotus and Thucydides to illustrate the excessive and unparalleled criticism to which the NT documents have been submitted.

Hanson possesses in rare combination the skills both of an historian and of a theologian. He has a perceptive eye for subtle developments of doctrine and the context in which they occurred. This naturally raises the important question of Scripture and its relationship to doctrinal developments subsequent to the apostolic age.

Undeniably, considerable development occurred from the primitive statements of the sub-apostolic age. To correlate this fluidity in Christian doctrine with the unchanging word of Scripture is no easy matter. Hanson's own view runs along the following lines. It is the insufficiency of the Scriptures themselves which, in part at least, necessitates doctrinal development. He also pinpoints the missionary activity of the church as a contributory factor; for this forced the church to answer the challenge of competing philosophies.

Hanson's attitude to the insufficiency of Scripture is clear when he writes, 'The Bible does not give us a specifically Christian doctrine of God, though it gives the raw material for this' (p. 238). Certain cardinal doctrines are embraced in this. He believes, for example, that the Cappadocians went beyond Scripture (and were right to do so) when they ascribed a full and separate place to the Spirit in the Godhead.

But Hanson offers us no adequate reason for accepting the validity of this development. True, he does suggest some thoughts arising out of the nature of Christian religious experience, but this should not deny the essential subjectivity of his judgment. In short, he fails to lay down principles to distinguish legitimate from illegitimate developments in doctrine. It comes, then, as no surprise that Hanson is against excessive dogmatism, which with some exaggeration he finds a modern rather than a patristic phenomenon. He is prepared to countenance the simultaneous emergence of quite distinct theological traditions without judging between them. This is how, for example, he handles the *Filioque* dispute.

That is not to deny that Hanson's comments on particular issues do often hit the mark. He rightly states that the opponents of Arianism did more justice to the NT, especially John's Gospel, than did the Arians. He is aware that borrowing from Greek philosophical terminology was inescapable, but can point to ways in which it was a mixed blessing. Again, his essay on the development of an ordered ministry sets out parameters beyond which it would have been detrimental for the church to go. He denies Jewish precedents for the ministries of the early church, and adumbrates a case for the view that the structures of the Christian ministry will inevitably contain an *ad hoc* element. Perhaps his comments about excessive dogmatism are more in place here than with the doctrine of the Trinity.

Hanson's style is lively and clear. At times it might almost be described as swashbuckling. He does not suffer fools gladly, whether they be modern scholars or ancient churchmen! This makes for entertaining reading as well as for the occasional overstatement, which may be forgiven as it does more to stimulate a response from the reader than to mislead.

Not least among Hanson's virtues is his ability to compress much into a short compass. For this reason some essays are particularly useful to undergraduates or those new to the field. I have in mind his account of the development of the doctrine of the Trinity prior to the fourth century (pp. 238-243), his treatment of office in the early church (117-143) and early Christian attitudes to pagan religions (144-229).

But I ought not to leave the impression that this book is for beginners only. Serious scholars of the early church will not be able to ignore such central themes as are ably considered in this collection.

Graham Keith, Ayr.

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ἐποικοδομηθέντες ἐπὶ τῷ θεμελίῳ τῶν ἀποστόλων καὶ
προφητῶν, ὄντος ἀκρογωνιαίου αὐτοῦ Χριστοῦ Ἰησοῦ.