

By K. A. KITCHEN BA, *Lecturer in Egyptian and Coptic in the University of Liverpool. A discussion of I and II Kings by John Gray (SCM Press, Old Testament Library, 1963. 744pp. and 3 maps. 75s.).*

THIS MASSIVE NEW VOLUME is contributed to a now well-known series by the Regius Professor of Hebrew in the University of Aberdeen. Conscious of the publication of Montgomery's ICC volume on Kings hardly twelve years ago, Professor Gray aptly remarks (p. 9) that 'the rapid accumulation of fresh archaeological evidence and the reassessment of former data' still leaves scope for a fresh treatment while benefiting from the work of earlier investigators.

As Part I of the book comes an *Introduction* (pp. 11-74) on the composition and text of Kings, and on the chronology of Kings and the Hebrew monarchies, with a table of dates from David's death to the fall of Jerusalem. Parts II, III and IV (respectively, Solomon's empire; Israel and Judah to the fall of Samaria; and Judah to the fall of Jerusalem and the Gedaliah and Evil-Merodach 'appendices') are the commentary proper — Dr Gray's own translation of Kings, a paragraph at a time, preceded by brief 'critical' comment and followed by a verse-by-verse commentary. Readers should note that Dr Gray follows the Hebrew, not English, chapter and verse numbering, when these differ. The work ends (after Abbreviations) with 20 pages of bibliography, three welcome indexes (Hebrew words, place-names, subjects) and three outline maps (Solomonic and Assyrian empires, Jerusalem).

With the review-copy, the publisher enclosed notice that the book contains many small errors, esp. in Hebrew transliteration, and that suitable revisions are in hand. These have not troubled the reviewer much, and some minor errors are here noted only where they are probably original.

A really detailed review of this 700-page book is impossible here. Instead, a selection of points raised by the book

is treated; despite the apparent length, both the selection and the treatment are limited. Briefly, the book is more useful for Hebrew text-study than for the historical background. On history and Near Eastern background, this book is not all that it might (and should) have been. But as accessible commentaries are few, the reviewer would offer corrections not merely negatively but with additional relevant data towards better views and filling the gaps. On general issues it is not so much Professor Gray as Old Testament studies in general that are at fault. What follows comes from the relatively objective basis of Ancient Oriental studies with limited but essential documentation as guarantee of the facts invoked. For special abbreviations, see at end of the review.

### I. GENERAL POINTS

#### 1. COMPOSITION AND 'DEUTERONISM'

(a) *Composition.* The last explicit date in Kings is the accession of Evil-Merodach of Babylon (2 Ki. 25: 27) in 561 bc; final compilation of Kings seems to have followed the death of Jehoiachin (2 Ki. 25: 30, 'as long as he lived') sometime later — by c. 550 bc? There is no compelling reason why the present book(s) of Kings should not be the work of a single compiler-author about then. One may alternatively posit such a writer about 580 bc, and regard 2 Kings 25: 27-30 as a later supplement — or have an earlier writer and longer supplement(s). The author-compiler of such a book would excerpt (and part-quote) and summarize from fuller historical records, using their data to construct a framework of reigns and events, and add thereto brief descriptive comment to enforce his lesson.

(b) *'Deuteronism'.* Old Testament scholars, including Prof. Gray, usually prefer the supplement-theory of compilation, but unfortunately not in so simple (and Near Eastern) a way. Firstly, following Noth (*cf.* Gray, pp. 12, 15, 16), they label the Kings author(s) as 'Deuteronist'. While

this is more serious for biblical books other than Kings, it is too narrow a label. Views common to Kings and Deuteronomy are not exclusive to these books. Gray himself admits this — the word of God, obedience, disobedience and its consequences both here and in the prophets for example (pp. 41, 42). These things (and Deuteronomy with its 14/13th century bc covenant-form, see *AO/AT*, part I: B.3, sect. iib) are all part of Israel's heritage and experience going back to the Sinai-covenant of the 13th century bc. Centralization of worship is not the main theme of Deuteronomy which in any case did not (and could not) specify Jerusalem.

Secondly, and much more serious for Kings, OT scholars including Gray go beyond the Deuteronomic writer merely *selecting* matter to suit his theme. They and Dr Gray (pp. 143, 157/8, 197; 219, 221/2, and esp. 252-4) blandly assume that their Deuteronomist fabricated and interpolated incidents, dreams, speech and additional 'details', as though nothing having 'Deuteronomic' qualities could happen or be expressed before the 8th/7th centuries bc. Apart from parallels in the OT at large, the plain fact is that various 'Deuteronomic' phenomena are universal and *ancient* Near Eastern. Two examples may make this clearer.

*Apud* 1 Kings 6: 11-14 (pp. 157/8) and 9: 1-9 (pp. 219/221-2): if a divine message to king Solomon (at least once in a dream) with a conditional promise is a Deuteronomic invention, what shall we say of the pharaoh Tuthmosis IV (c. 1400 bc) in whose dream at the Sphinx the sun-god promised him the kingship, but required him to clear the Sphinx of sand (*ANET*, 449, known to Dr Gray, p. 120)? Or of the Egyptian high priest Pasherentpah (1st century bc) conditionally promised a son by the god Imhotep (Oppenheim, *Interpretation of Dreams in the Anc. Near East*, 1956, p. 252)? Are these and others all Deuteronomic too?

*Apud* 1 Kings 8: 14-61 (p. 197): if the address, prayer and benediction of Solomon, because rhetorical and concerned with sin and its consequences, is 'Deuteronomic', then again, what of the speeches and prayers of a whole series of Egyptian, Mesopotamian and Hittite monarchs? Royal rhetoric is not Deuteronomic but universal. And on royal concern for sin and its consequences, compare the plague-prayers of Mursil II, cited below (section, 'Concept of Exile'), esp. *ANET*, 395b, § 9, for 1 Kings 8: 46.

It is one thing to attribute to a writer (Deuteronomist or not) descriptive comment upon the times that he tells of. It is quite another to have him inventing history, and foisting unhistorical attitudes and sentiments upon historical characters. When precisely this run of actions and attitudes is demonstrably common and *ancient* Near Eastern, there is no warrant factually for denying an equivalent authenticity to their appearance in Kings. The reverse of the coin, e.g. Elijah's non-mention (and assumed ignorance) of Deuteronomic issues like centralized worship (Gray, p. 365), is as worthless. Elijah's crisis was no occasion for theological debating-points such as one altar v. many, when the very existence of any worship at all of Israel's true God seemed at stake.

## 2. THE CONCEPT OF EXILE

Whenever the Hebrews are threatened with deportation by some enemy (*i.e.*, 'exile') as a punishment (Deuteronomy, the prophets, *etc.*), with destruction of land, homes and temple, *etc.*, then OT scholars (Prof. Gray included) automatically jump to one fatal conclusion — that practically all such passages reflect experience of the Babylonian Exile (and 'must' be exilic or post-exilic) or at least of the Assyrian deportations of c. 734, 722 bc. This widespread belief is erroneous and betrays ignorance of the relevant Near Eastern background. Here, these views affect Solomon's prayer (1 Ki. 8: 44-53; pp. 28, 189, 197, 198, 210: 44-53), God's word to Solomon (1 Ki. 9: 7; pp. 219, 221: 7), and the condemnation of Jeroboam I (1 Ki. 14: 15; p. 308: 15). Being carried away into captivity and devastation of the homeland were a *constant* threat to many people in the Ancient Near East at all periods, and can be studied in detail from the 2nd millennium bc onwards. Here are some examples. From the first of about 16 Syrian campaigns, Tuthmosis III of Egypt brought back 2,503 people, about 25,000 animals and much other plunder (*ANET*, 237b). The next pharaoh, Amenophis II (c. 1420 bc) took captive to Egypt over 100,000 assorted Syrians in two campaigns (*ANET*, 247 and n. 48). His successor Tuthmosis IV records at Thebes of a foundation: 'The settlement (so-and-so) with Syrians . . . (of) the town of Gezer' (*ANET*, 248a). Many more are known. In Anatolia, the annals of the Hittite king Mursil II (c. 1320 bc) are *full* of

'exiles' — in his 3rd year, over 15,000, and the same next year followed by another 66,000 captives from Arzawa (Götze, *Die Annalen des Mursilis*, 1933, *passim*). In the Egypt of the 13th century BC — age of Moses (and of Deuteronomy, not least 28: 63ff.) — Ramesses II is termed at Abu Simbel 'he who has removed Nubia to the Northland, and the Syrians to Nubia; who has placed the Shasu-Asiatics in the Westland (= Libya), and established the Libyans on the (sc. Eastern) hills . . .'; likewise Ramesses III, c. 1180 BC (*cf.* Sauneron & Yoyotte, *Revue d'Égyptologie* 7 (1950), pp. 67-70, esp. 70). Also in the 13th century BC, Shalmaneser I of Assyria carried off the youths of Urartu for service in Assyria (*LAR*, I, § 114) and 14,400 captives from Hanigalbat (*ibid.*, § 116). His successor Tukulti-Ninurta I brought away 28,800 Hittite warriors (*ibid.*, §§ 164, 171). About 1100 BC, Tiglath-pileser I took 4,000 men from Urumai and Abeshlai in year 1 (*ibid.*, § 318) and 20,000 from Kumani in year 5 (*ibid.*, § 321). And so for the rest of Assyrian history. In 879 BC Assur-nasirpal II peopled Calah with captives from all lands that he had conquered (*LAR*, I, §§ 489, 511, *etc.*); then Shalmaneser III in 10 years or so carried off the ultimate 44,400 people into Assyria (*ibid.*, § 616).

The plain fact of the matter is that for people living in Syria and Palestine (or anywhere in reach of Egypt, Anatolia or Mesopotamia) during at least the 15th to 6th centuries BC, such warlike deportations and destructions were a constant threat, ravaging them or their neighbours in every generation. Israel need not wait 1,000 years to learn *this* fact of life, as the distortions of Old Testamentarian theory would demand. Moses (Dt. 28: 64ff.), Solomon (1 Ki. 8: 44-53) and Jeroboam I (1 Ki. 14: 15) could not escape knowing about the ultimate physical sanction, already practised for centuries.

Likewise, the supposedly 'Deuteronomic' ideas on sin (p. 189) as in 1 Ki. 8: 46 ('there is no man that sinneth not') are *not* peculiar to Israel (let alone a Deuteronomist). In another royal prayer, the Hittite Mursil II three centuries before Solomon confessed that 'it is only too true that man is sinful' (*ANET*, p. 395b, §9). Was Mursil's prayer also supplied by the meddling Deuteronomist? I doubt it. We have, therefore, no warrant whatever to invoke him for Solomon either, any more than to refer every mention of 'captivity' to after either 734/ 722 or 587 BC.

### 3. KINGS AND ANCIENT NEAR EASTERN HISTORIOGRAPHY

While Professor Gray is only one among many offenders on points 1 and 2 above, here, sadly, he fails solo. As background for an intelligent consideration of the Hebrew sacred historiography in Kings, he should have had a separate section in the Introduction on relevant aspects of Ancient Oriental historiography. Instead, merely incidental reference is made to the Behistun text (p. 30), Babylonian chronicle-synchronisms (p. 30/31), the relation between annals and stone inscriptions (p. 31), and the supposed 'Königsnovelle' of Egypt (pp. 26, 117, 120). References to the chronicles of Israel and Judah, *etc.*, are noted, and a Temple history or records supposed, but no more. Instead of checking on ancient usage, Prof. Gray is too often tempted to criticize Kings for its arrangement of data or 'saga-like' general remarks, as if it were a modern Western work and not an Ancient Oriental one (*e.g.* pp. 21, 28, 38/39, 114, 244, 691).

This is no place to fill the gap. The following hints must suffice. 'Temple histories' start with the Sumerians before 2000 BC — witness the Tummal Chronicle (Hayes, Rowton, Stubbings, *Camb. Anc. History*, I/II (new ed.), *Chronology* fascicle, 1962, pp. 31/32, 54, 65), and later Babylonian and Assyrian building-texts (*ibid.*, p. 32 and n. 2). Lengths of reigns and synchronisms (*cf.* Kings) occur in the synchronistic class of Babylonian and Assyrian chronicles, *e.g.* those covering c. 702-539 BC (*ANET*, 301-307), whose high historical quality is universally admitted today. Variety of length and detail in treating subjects is common in such texts — consistency was not maintained, avoiding inclusion of what seemed irrelevant then. This also applies to Kings. Again, verses like 1 Kings 4: 20, or 10: 20b, 21b, are not 'saga' insertions into factual records. They are common usage of Ancient Oriental 'factual' annalists in expressing emphasis. 'There was not the like' repeatedly occurs — but is not due to 'saga'. For titles like 'the Acts of Solomon' *cf.* the Deeds of Suppiluliuma (Hittite, ed. Güterbock, *JCS* 10 (1956)). From Mesopotamia, Dr Gray would have gained useful data in Olmstead's *Assyrian Historiography: a Source-Study*, 1916 (missing from his bibliography), besides Egyptian and Hittite data. On Babylonian chronicles, *cf.* D. J. Wiseman, *Chronicles of Chaldean*

*Kings*, 1956, pp. 3-5, and now A. R. Millard, *Iraq* 26 (1964), pp. 14-35, esp. 32-35.

#### 4. MIRACLES AND NUMBERS

(a) *Miracles*. These are not tolerated by Dr Gray (e.g. pp. 336, 340: 14, 460, 461, 635/6), though even he is driven to protest when rationalizing runs visibly to extremes (p. 357). Likewise, prediction is *ex eventu* (cf. p. 293/4). Miracles are automatically 'saga' or, like prophecies, labelled with the hang-dog epithet 'midrashic' (p. 636), or blamed on a Deuteronomist (cf. 293/4). Prof. Gray is entitled to his own opinion; but the mere occurrence of miracle (real or alleged) is not itself a sufficient criterion for labelling passages as 'saga', etc. — more objective grounds must be found.

(b) *Numbers*. These are always a special problem to themselves in ancient documents, a fact not sufficiently allowed for by Dr Gray, for whom large-looking numbers almost inevitably reflect 'saga', 'exaggeration' or the like (e.g. pp. 370, 434, 453/4, 454: 10, 499, etc.). However, in reference to numbers (outside as well as inside the OT), several points must be borne in mind.

(i) Difficulty in textual transmission of numbers. Evidence of early Hebrew ostraca suggests that in the OT period numbers were written as figures ('99') rather than as words ('ninety-nine'), as later and in extant OT Hebrew text. This made scribal errors easier. (ii) Numbers like 40 or 7 are not always mere approximation or 'ideal' symbol respectively, but do occur literally in real life too. On 40, see next section. If '70 sons of Ahab' shows 'the influence of the saga-form on the historical narrative' (p. 499), what shall we say of the 70 brothers of Panammu II king of Sam'al murdered with that king (cited, Gray, *loc. cit.*)? 'Saga' could not affect that first-hand inscription. Surely the 70 in each case merely reflects the approximate number of male relatives and possible pretenders to these thrones. (iii) Some large figures, when more closely considered, may not be so exaggerated after all; cf. on Solomon, below. (iv) Variant figures (e.g., as between *Kings* and *Chronicles*) are simply a scribal problem common to all ancient Near Eastern documents — e.g., in the various 'editions' of the annals of Assyrian kings. Careful study of details is the only solution here.

#### 5. CHRONOLOGY (cf. pp. 55-74, etc.)

(a) *General Chronology of Kings*. Rather than merely accept one or other of the reigning 'systems' (e.g., Thiele or Albright), Prof. Gray compactly surveys the biblical and external data in the light of previous studies, and essays his own reconstruction. As his table shows (p. 74), his results are close to those of Thiele, *MN*, and in *NBD*, 219-221.

(b) *The Number Forty* (pp. 55, 101/2, 527). While this *can* be a round number, reigns of 40 years can and do occur; note that David's reign was 7 + 33 years. Irishum I of Assyria is given 40 years.

(c) *Reigns and Synchronisms* (pp. 55-59). Hiram I of Tyre is better at c. 979/8-945/4 BC (*HHAHT*, Table III), and Shoshenq I of Egypt at c. 945-924 BC (*ThIP*), for all related facts to fit Solomon in c. 971-931 BC (re. p. 55). We have no warrant for referring either regnal figures or synchronisms (or both) to the schematization of late redactors — compare the equal reliability of both in the Babylonian synchronous records (see above, *Historiography*). To classify such material in *Kings* as 'original' or late and schematic is just impractical guess-work.

(d) *The Fall of Samaria* (pp. 60/1, 580/1) took place in all likelihood *not* in 721 BC, but in 722; Dr Gray has not only ignored Thiele's careful treatment of this period, but has also totally failed to make any use of H. Tadmor's fundamental study of the chronology of the campaigns of Sargon II in *JCS* 12 (1958), 22-40, 77-100.

(e) *Superficial Discrepancies* (pp. 63, 64). Dr Gray rightly solves these much as did Thiele in *MN*. A good Egyptian parallel exists for their treatment of the 20 years of Pekah as partly retrospective, covering those of discredited predecessors — the Ramesside scribes attributed the regnal years of the 'heretical' Amarna pharaohs to Haremhab their 'orthodox' successor (*SAP*, pp. 9, 10). In postulating a co-regency of Hezekiah with his father from c. 729 BC, Dr Gray steps ahead of Thiele, into line with *NBD*, 217, 220 and *HHAHT*, Table II.

(f) *Dating Vocabulary*. Dr Gray would force 1 Kings 6: 1 (pp. 150/1), 6: 38 (pp. 165/6) and 8: 2 (p. 190, n. a) into a literary-critical straitjacket. Firstly, he would distinguish *yerah* and *hodesh* as 'early' and 'late' words for 'month'. No objective evidence exists to make 'month' a late meaning of *hodesh* — only literary presuppositions

about other OT occurrences. Secondly, he would dismiss the numbering of months ('second', 'seventh', etc.) as late glosses. Unnecessary, because this was used throughout the OT period alongside the old West-Semitic names like Ziv and Bul (*cf. refs., NBD, 178b*). Thiele demonstrated conclusively, on the data from Solomon's temple-building, that Solomon and Judean kings had normally a regnal year counted from Tishri (7th month), distinct from the common year that began in Nisan = Abib (*MN, pp. 30/1, cf. 32, 33*). This renders superfluous Gray's 'late' Mesopotamian influence. The spring new year was old, and the regnal year a 'new' usage. The practice of numbering the months probably dates from the Exodus period, reflecting Egyptian usage. (*Cf. long ago Kaufmann, *Vetus Testamentum* 4 (1954), 307ff., and now esp. S. Külling, *Zur Datierung der 'Genesis P-Stücke'* (. . . *Gen. XVII*), Kampen, 1964, pp. 190-192.*)

#### 6. THEOLOGICAL 'DEVELOPMENTS'

Factually-outdated shibboleths duly appear. Thus (p. 145, end), on the 'name' of God, Prof. Gray most appropriately refers to this very usage in the texts from Ugarit (besides elsewhere) — and then promptly forgets this evidence of the 14th/13th centuries BC at latest, in making this use in Kings a 'Deuteronomic' (hence, late: 8th/7th cent. BC) refinement after 'J's' anthropomorphism. In fact, there is no question of 'refinement' thus at so late a date. From c. 2000 BC onwards (earlier where evidence exists), so-called 'advanced' and 'naïve' modes of thought were used side by side throughout the ancient Biblical East, and from the 2nd millennium BC onward afford no criterion. For early universalism, *cf. briefly NBD, 348a*; for early personification (as Wisdom in Prov. 1-9), *cf. Kitchen, *Tyndale House Bulletin*, No. 5/6 (1960), pp. 4-6.*

On p. 216 again the 'evolutionary' traits: 'pre-exilic times, . . . less dominated by sin-consciousness', and so post-exilic times more so. This is pure Wellhausen and pure error. It is now perfectly clear that there is no growing sense of sin with the progress of Israelite history, as shown (on non-Levitical material) by R. J. Thompson, *Penitence and Sacrifice in Early Israel outside the Levitical Law* (Brill, Leiden, 1963). For the scale of entertainment at the dedication of the Temple, *cf. Assur-nasir-pal II's city-dedication at Calah in 879 BC*

for 69,574 people for 10 days' festivity (Wiseman, *Iraq* 14 (1952), pp. 24ff.).

On pp. 518/9, Dr Gray's identification of 'eduth'/'edoth' as the covenant-obligations or stipulations and his refusal to follow Wellhausen's superfluous emendation (2 Ki. 11: 12) I heartily endorse — but *not* his supposed Egyptian parallel data. He outdoes von Rad, even producing a *casket* of deeds, etc., for the pharaoh's legitimation. I would like to see his evidence for *that* receptacle! In fact, the pharaoh's 'protocol' is solely a titular of five formal names, and has nothing whatever to do with credentials and conditions of office, etc. This erroneous theory of von Rad is refuted in *AO/AT*, part IB, 3 section *iid*.

On p. 203, Dr Gray contrasts Solomon standing to pray in 1 Kings 8: 22, and kneeling in 8: 54, perhaps 'a reflection of later custom'. The simple answer is sufficiently given by the fuller 2 Chronicles 6: 13: he stood, then knelt.

#### 7. LINGUISTIC POINTS

Throughout the commentary, Prof. Gray is constantly weighing the Hebrew vocabulary of Kings. For difficulties, he at times turns to Ugaritic, a language very close in time, nature and place to Biblical Hebrew. But far more commonly, he resorts to Arabic, even on words where the Hebrew contexts should suffice. This is methodologically dangerous; the vast mass of Arabic literature dates from the Islamic era (*i.e.* 7th century AD, ff.), nearly 2,000 years after Solomon, for example. The state of Arabic lexicography is such that one can always find something in Arabic vocabulary, but without adequate control of usage and origins. The grammatical structure of Arabic retains much that is archaic — but this fact does *not* guarantee an equal antiquity for vocabulary and usage. It is, therefore, far sounder to turn to languages contemporary with Biblical Hebrew when context fails — Ugaritic, West-Semitic inscriptions (and loanwords in Egyptian) and Akkadian (Assyro-Babylonian).

(a) A case in point is p. 137 on verse 2. *Solet* (in 1 Ki. 4: 22; Heb. 5: 2) is far likelier to be 'fine flour' than the late Arabic 'unhusked barley'. Dr Gray has overlooked far earlier and more relevant evidence: West-Semitic *silt* as a loanword in Egyptian (*trt*) in the 13th century BC, where the meaning is 'fine flour' in Papyrus Anastasi IV: 17, 4 under flour and with other foodstuffs (also 13, 12, loaves). Hence this rendering in *NBD*, 431, 432. P. 137: 4, 'beyond the river'

is merely the term used by the exilic writer of Kings, not an interpolation.

(b) *Apud* p. 400: 9. Heb. *saris* from Akkadian *sha-resh-(sharr)i* first (as in Akkadian) meant merely 'official', 'courtier'. This early usage of the 2nd millennium BC fittingly occurs in Genesis 37: 36. The word then came more commonly to mean 'eunuch' in both languages; see Kitchen, *JEA* 47 (1961), 160. Other Mesopotamian and Egyptian terms changed in the same way. Appeal to Arabic is here pointless. The earlier general meaning still persisted till Persian times. Atiyawahy, a Persian official in Egypt under kings Cambyses to Xerxes, was a '*saris* of Persia' (Posener, *La Première Domination Perse en Egypte*, 1936, pp. 117-124, Texts 24-30).

(c) On pp. 431 n.k and 434 on 2 Kings 3: 4, the irreverent will probably greet Mesha the hepatoscopist (*noqed*, 'sheepmaster') with hoots of mirth! The suggestion is ingenious, and no joke in itself. But it is needless. The plural 'shepherds' occurs in Hebrew in Amos 1: 1. The various Ugaritic references to *nqd(m)* are best taken as 'shepherd(s)' — so both Gordon, *Ugaritic Manual*, III, *sub verbo*, and Virolleaud, *Syria* 21 (1940), 151, *etc.* The chief priest in Ugarit who is also chief of the *nqdm* may simply be responsible for the temple flocks and herds, as (for example) were three high priests of Amun in 18th-Dynasty Egypt ('Overseer of Cattle' . . . ; Lefebvre, *Histoire des Grands Prêtres d'Amon de Karnak*, 1929, pp. 231 (Hapusonb), 232 (Ahmose), 237 (Mery); some also 'Overseer of Fields' and 'Granaries'). Instead of the recherché *naqadu* 'to probe', the correct Akkadian cognate for Hebrew *noqed* is *naqidu*, 'shepherd, herdsman', occurring plentifully from the 23rd to the 5th centuries BC (Gelb, *Glossary of Old Akkadian*, 1957, p. 205; Bottero and Finet, *Archives Royales de Mari*, XV, 1954, p. 234; Friedrich, *Hethitisches Handwörterbuch*, 1952, p. 287a *sub* NA.KAD; Delitzsch, *Assyrisches Wörterbuch*, 1890, p. 479a, *etc.*). Also, perhaps the 100,000 each of wool-yielding rams and lambs looks less enormous if one thinks of (say) about 2,000 Moabites having about 100 animals each, or even only 200 owning about 1,000 animals each. Moab was sheep-country par excellence, *cf.* D. Baly, *The Geography of the Bible*, 1957, p. 237. (d) P. 496, *puk* (kuhl) is galena/lead sulphide rather than antimony in OT period; see *NBD*, 260b and refs.

## 8. SOME GOOD POINTS

(a) *Versions*. Dr Gray throughout makes constant reference to the Greek versions of Kings which sometimes diverge from the Hebrew but usually present an inferior text; this is highly convenient for text-study.

(b) *West-Semitic Linguistic and Literary Background*. Dr Gray makes use of Ugaritic (in which he has specialized). That Hebrew literature shared and inherited West-Semitic style is utilized by Dr Gray, *e.g.*, p. 85, where he rightly notes verbal repetitions as part of epic style instead of condemning them as *Altestamentler* tend to do. Likewise pp. 90: 38ff., and on royal justice, *etc.*, p. 120. Dr Gray also makes allowance for the history of the Hebrew script (pp. 51-54), a point so often neglected by textual commentators.

(c) *Useful Textual Points*. The reviewer would be more cautious than Dr Gray on textual matters, but finds some very feasible suggestions. At random, one may mention p. 78 n.a (Re'i/friend) and p. 103 n.a ('obeisance' to queen-mother), while his reference to Amos 3: 14 (p. 94: 50) is attractive, and suggestions about Azariah's last abode (p. 559 n.b) and on the Mizpahs (p. 322) are well worth consideration.

(d) *Caution and Sense* compared with some OT scholars is one of Prof. Gray's merits; so on chronological data in Kings on p. 63 top, in rejecting Whitley's speculations (p. 369), and distinguishing three Benhadads (four on p. 320 is a slip of the pen).

(e) *Palestinian Archaeology*. It is good to see Prof. Gray cognizant of Yadin's important work at Megiddo (*cf.* pp. 228/9, 271/2), and using the latest Jerusalem data (p. 227, *etc.*). His suggestion about the 'ophel' of Samaria or other cities as the fortified citadel mound is very attractive (p. 458). Likewise, his treatment of Mesha of Moab on p. 410 is constructive.

## 9. SOME WEAKNESSES

(a) *Egyptian slips*. P. 88: since when has Horus been a *fertility-god*? 'Long live the king' (marks loyalty) is over-interpreted; no real comparison exists between a divine pharaoh and a Hebrew king under covenant.

P. 328: The Syrian in the Delta was 1200 BC, not 11th century and was called Arsu not Arshu (different letter—no link with Arza of 1 Ki. 16: 9). In fact, Arsu in Egypt was merely an epithet ('Self-made (man)'), for the Syrian

chancellor Bay, not a name (Gardiner and Cerny, *JEA* 44 (1958), 21; *HHAHT*, chapter iii).

P. 675/6: There is no question of Psammetichus I (664-610, not 671-617, incidentally) challenging the Assyrians — Egypt was still their ally (but no longer their vassal) under Necho II. P. 678/9, re. 2 Kings 23: 29. '... made an expedition against ...' should be corrected to '... went up unto the king of Assyria ...'

P. 680, there is no need whatever to accuse the Hebrew compiler of error, or to emend 'al to 'el — the preposition 'al is quite sufficiently attested in the meaning 'to', either after verbs of motion (as here) or as a dative (Brown, Driver, Briggs, *Hebrew and English Lexicon of OT*, p. 757: 7c: (a), (b), (c); and 758: 8. Prof. Gray should know this).

P. 695: Apries acceded in 589 (in accordance with 664 for Psammetichus I). (b) *In Phoenicia*. Pp. 144, and esp. 146. There is no warrant for asserting that the term 'Sidonians' for Hiram's subjects 'reflects conditions somewhat later than the time of Solomon' (p. 146); a single king-list suffices for Tyre and Sidon from Abibaal (father of Hiram I) to the Persian period (*HHAHT*, Table III); apparently realized on p. 333.

## II. THE AGE OF SOLOMON

### 10. SOLOMON'S PHARAOKS AND HADAD (cf. pp. 55, 115/6, 261-6, 276)

See Nos. 6, 7 and 8 in *NBD*, p. 980b sub PHARAOH. Shoshenq I (cf. § 5c, above) must date to c. 945-924 BC (not a decade later): (i) because new results for his dynasty require 10 years' earlier dating for his later successors; (ii) because his Theban monuments of the Palestinian campaign (5th year of Rehoboam) are unfinished, begun at the end of his reign just after the campaign; (iii) because — unperceived by Albright (whose lunar date for Takeloth II is fallacious) — Egyptian Dynasty 22 dates depend on Hebrew chronology to some degree, not *vice-versa*. All details in *ThIP*.

Therefore, the pharaoh that took Gezer (pp. 115/6) early in Solomon's reign 20-25 years before Shishak (Shoshenq I) *must* belong to Dynasty 21, as rightly seen by Dr Gray (p. 116) on less cogent grounds. As pointed out in *NBD*, pp. 344/5, 980b, No. 7, king Siamun is the best candidate; he would be contemporary with Solomon's first

years if his successor Psusennes II reigned 14 years (c. 959-945 BC; *ThIP*). On this phase of Solomon's activities *re* Egypt and Philistia, see now the important article by A. Malamat, *JNES* 22 (1963), 1-17, esp. 9-14 (Egypt) and 14-17 (Philistia and Gezer). Dr Gray could have used Malamat's earlier study in *Biblical Archaeologist* 21 (1958), 97-99 for Egypt.

On this basis, we may reconsider the Edomite Hadad's flight to Egypt *via* Paran (pp. 261-266). Dr Gray rightly rejects the idea of two conflated stories (Hadad in Egypt; Adad in Midian), but instead assumes quite unnecessarily two traditions for Hadad (Egypt; Midian). Hadad was a child when taken by his supporters first into Paran and then on into Egypt. There he was assigned a house, provisions and land. This fits in perfectly with Egyptian usage; the produce of the land gave Hadad and co. an income, just as for royalty in this and earlier periods. There is no need to delete the reference to 'land' as Gray would do (p. 263 n. a). Genubath grew up at court 'among the sons of Pharaoh' (Hebrew text as well as Greek), a phrase that cannot be dispensed with (despite Gray, 263 n.d), as this is again Egyptian usage: upbringing and education at court with the royal children is a feature in texts throughout Egyptian history. Hadad's Egyptian stay probably fell into the time of kings Amenemope, Osochor and Siamun; *ThIP*. *Apud* 1 Kings 11: 40 (p. 276), Shishak is far more identifiable with Shoshenq on the Hebrew text (*q* and no *m*) than on the Greek; read Heb. Shushaq as in text of 1 Kings 14: 25.

### 11. SOLOMON'S WISDOM (cf. pp. 99, 112, 119-120, 138-142)

On 1 Kings 4: 33 (Heb. 5: 13 in Gray), Dr Gray at first approves of Alt's idea that Solomon's concern with flora and fauna might reflect Near Eastern listing of natural phenomena (pp. 99, 112, 139), but then he rejects it (p. 141/2) in seeing here a reference to figures of speech (cf. Prov. 30: 25ff., etc.) — and rightly so, as these listings are not limited to natural phenomena.

As for the Instruction of Amenemope and the book of Proverbs (p. 141), there is now *no* adequate warrant for assuming any special relationship (*i.e.*, borrowing *either* way) between these works, despite common belief to the contrary. This results from a thesis-project recently completed at Liverpool [not by me],

scheduled for publication in due course.

Prof. Gray refers twice (pp. 119/120, 138/9) to R. B. Y. Scott's view that the traditions of Solomon's wisdom are late and exaggerated, and rightly reacts (p. 139) against part of Scott's fallacious reasoning. E.g. 'late' language in Kings on Solomon's wisdom has no bearing on the date of the wisdom-tradition, but only on the date of Kings (exilic) in which it is found. Scott's other arguments are no better. On linguistic dating-fallacies in OT studies, cf. *AO/AT*, part I.B.5, section iib. There is no good warrant for disputing Solomon's 'wisdom'. Despite both Scott and Gray (p. 120: 'valid'), royal wisdom linked with prosperity is not late (people eating, drinking, joyful, etc.); compare e.g. the *first-hand* bilingual texts of king Asitawata of Cilicia c. 720 BC (*ANET*<sup>2</sup>, 499/500), 'in my days, the Danunites had all good, plenty to eat and well-being . . . (repeated 4 times later on!). . . Every king considered me his father, because of my justice, my wisdom and my kindness of heart'. On Rahab of Jericho (inn-keeper?), see now Wiseman, *Tyndale House Bulletin*, 14 (1964), 8-11.

## 12. SOLOMON'S ADMINISTRATION

(a) *A Torn Document?* Dr Gray accepts (pp. 126, 130 n. b) Albright's old theory that in 1 Kings 4: 7-13 five names are lost ('x ben/son of Y') of Solomon's district officers. Ingenious but unnecessary; RV and RSV are correct. We have (esp. from Ugarit!) masses of indubitable Ben- names — how could Dr Gray miss them? I list here the main occurrences. Some 15 or 16 in Nougayrol, *PRU*, III, pp. 49, 58, 60, 78, 83, 103, 104, 112, 126, 139 (Bin-hatiyanu, a woman!), 148, 165, 173. On pp. 197ff., see texts 11.839, 15.09 (34 out of 60 names!), 15.42, 16.257; 11.787 has 9 out of 10 names *mar-x*, same as *Bin-x*. In the index of personal names in Virolleaud, *PRU*, II, I found over 170 *Bn-x* names; runs of them in 'admin.' documents like the Kings reference (*PRU*, II, pp. 72, 82, 89, 95, 132); or 'Bn-sln and his heir', p. 88, No. 63, 11, 14-15. Semites in Egypt (13th century BC) include Ben-'Anath and Ben-Ozen (*NBD*, 343b or 845a). Hence keep Ben-names in 1 Kings 4: 7-13.

(b) *Ministers of State.* More needless changes. Shavsha and Elihoreph, cf. *NBD*, 1171b. Despite Noth and Gray, Elihoreph can well be a proper name — 'my God is leader' (participle), from

IV *chrp* (Brown, Driver, Briggs, *Hebrew and English Lexicon*, 358); cf. names Hareph and Hariph (1 Chr. 2: 51; Neh. 7: 24; 10: 20), and Old Babylonian Hurapum (*Chicago Assy. Dictionary*, Vol. H/6, p. 245b, *hurapu* (c)). 'Over the year' would be a strange title in Hebrew. The priests are no problem; Zadok takes precedence over the rusticated Abiathar who was actually high priest at Solomon's accession till deposed; Zadok was apparently succeeded by his (grand)son Azariah I (verse 1), the latter's father Ahimaaz (1 Chr. 6: 8, 9) perhaps predeceasing Zadok? On high priests, cf. *Westminster Bible Dictionary*, 245/6, Table. (Also Jehu 'son' of Nimshi, really grandson of Nimshi). Zabud 'priest and King's Friend' may have been Solomon's 'chaplain', cf. 2 Samuel 8: 18?

(c) *The Commissariat* (pp. 136/7, 146). With Gray (following Montgomery on very late parallels), one must stress the *feasibility* of the figures for Solomon's food-supplies, including payments to Hiram of Tyre (*pace* Gray, p. 146: 25). For detailed background and evaluation, based on palace-accounts from early Egypt and Mesopotamia, see *NBD*, 431-2, using sources available to Gray, but neglected by him. On *solet*, 'flour', cf. § 7a, above.

(d) *The Labour-force* (pp. 147/8, 233/4). Inadequately treated; the contradictions do not exist. See already *NBD*, 1197, § e. Solomon conscripted the non-Israelite remnant for *mas-'obed* (1 Ki. 9: 21-23), permanent state *corvee*, which must *not* be confused with plain *mas*, ordinary temporary *corvee* (cf. Haran, *Vetus Testamentum* 11 (1961), 163/4) required of the Israelites (1 Ki. 5: 13-16 (Heb. 27-30)) only for 7 years temple-building. Thus 1 Kings 5 and 9 refer to two separate *corvees* and are not contradictory. The Israelite levy is given as 30,000 on shifts of 10,000 per month under Adoniram in Lebanon. The 150,000 burden-bearers of 1 Kings 5: 15 (Heb. 29) are non-Israelite according to 2 Chronicles 2: 17, 18, under 3,300 officers (scribal variant 3,600 in Chr.) besides (*i.e.*, *not* the same as) Solomon's chief officers. This gives a proportion of 1 overseer to every 50 people, allowing 300 chief overseers over 10 at a time of the 3,000 ordinary ones. Solomon's chief officers are given separately as 550 (1 Ki. 9: 23); their distribution is unknown — doubtless some in Lebanon and some over the *mas-'obed* and burden-bearers and their overseers. These suggestions are at least less speculative than Dr Gray's (p. 147,



n.e) and do not need secondary verses. Note also (*re* p. 234: 22) that 1 Kings 9: 22 does *not* exclude non-Israelites from military attainment, hence is not unreliable, it merely stresses the leading role played by Israelites.

(e) *Revenues and Commerce*. It must be admitted that 420 talents of gold from Ophir (1 Ki. 9: 28), 666 gold talents net annual income (1 Ki. 10: 14) and 120 talents each from Hiram of Tyre (1 Ki. 9: 14) and the Queen of Sheba (1 Ki. 10: 10) are very large figures (p. 236, n.c.; 246: 14). The variant figures for the 420 of 450 (2 Chr. 8: 18) and 120 (Gk.) suggest that we have here a problem of textual transmission rather than conscious exaggeration. Nevertheless, compare in the Persian Empire the annual 360 talents of gold from the province 'India' (= North West Pakistan), gold-bearing like Ophir (Herodotus iii. 94). With the 120 talents of Hiram and Sheba, compare the 150 talents of gold paid as tribute by Metten II of Tyre c. 730 BC to Tiglath-Pileser III of Assyria (*ANET*, 282b); Dr Gray could have used these. Unspecified figures (p. 247, top, etc.) are indeed to be taken as silver shekels; the admin. texts from Ugarit (*PRU*, II and III) show this.

That Prof. Gray finds so little on *almug* (or *algum*) trees is surprising (p. 243). They are anciently well-attested, whatever their kind. Note *elammakum*-wood used at Mari in 18th century BC (Biro, *Archives Royales de Mari*, IX, 1960, 322-3; Dossin, *Syria* 32 (1955), 26); *elmaki*-wood for furniture at Alalakh, 15th century BC (Wiseman, *The Alalakh Tablets*, 1953, p. 13, 'sandalwood?'); *almg*-wood in a list of materials at Ugarit, 14th/13th centuries BC (Text 120: 8, Gordon, *Ugaritic Manual*, 1955, III, 237, No. 125 and *Ugaritic Literature*, 1949, p. 130 end). Mesopotamian references, *Chicago Assyri. Dict.*, Vol. E/4, 1958, pp. 75, 76, and von Soden, *Akkadisches Handwörterbuch*, pt. 3, 1960, p. 196b.

Likewise the price of horses and chariots (p. 245, n. j). Hacks are quite cheap in the Hittite Laws at 20 shekels (Güterbock, *JCS* 15 (1961), 75, 78), and among the Nuzi Hurrians at 30 shekels (*PRU* III, 41, n. 2, refs.), as these people got them from horse-growing areas. But good horses were much dearer among royalty. In a Mari letter, 2 horses from Qatna were worth 300 shekels each (c. 1750 BC), Dossin, *Archives Royales de Mari*, V, No. 20: 6ff., and later a king of Ugarit bought one at 200 shekels c. 1250 BC (*PRU*, III, 41). Hence Solo-

mon's price of 150 shekels in 950 BC is better than Gray's emendation to 50 shekels. Comparative chariot-prices are lacking; but Tutmosis III of Egypt repeatedly looted from Syrian rulers 'chariots wrought with gold and silver' (Sethe, *Urkunden IV*, 717, etc.), and Tutankhamun's tomb c. 1340 BC yielded superb gold-adorned chariots — worth several hundred shekels?

(f) *The Armed Forces*. (i) *Chariots*. One must agree with Dr Gray that 1,400 (1 Ki. 10: 26; 2 Chr. 1: 14) is a reasonable figure, comparing Ahab's 2,000 chariots at Qarqar in 853 BC (p. 249), also 1,200 from Aram and 700 from Hamath. (ii) *Horses*. With Dr Gray (p. 136, n.f), one tends to prefer the 4,000 stalls for chariot-horses of 2 Chronicles 9: 25 to the 40,000 of 1 Kings 4: 26 (Heb. 5: 6), again not an exaggeration but as a small scribal error (only one letter different, *t/m*) of later date. Compare the 2,000 horses negotiated on by a king of Ugarit 300 years before, doubtless in addition to what he already had (*PRU*, II, p. 28; Albright, *BASOR*, 150 (1958), 36-28). (iii) 'Horsemen'. However, Dr Gray (pp. 137/138 top) would treat *parashim*, 'horsemen' (12,000 in 1 Ki. 4: 26/Heb. 5: 6) as an exaggeration if chariotry is meant, or as a later insertion if cavalry be intended — or read it *parashim*, 'horses' (pp. 245, n. d; 249: 26 with ref. to 231: 19), changing only a vowel. This slight vowel-change would be alright in itself, in 10: 26, but would make nonsense of the parallel verse in 4: 26 (Heb. 5: 6) — hence retain the reading 'horsemen'. This in turn need be neither exaggeration nor late insertion. For charioteers, at 3 men per car, this would allow 1 active and 2 reserve crews for 1,200 of Solomon's chariots and 1 active, 1 reserve for 200. Men not on active service would live their own normal lives on the land, in their villages and towns — this is clear from Ugaritic documents listing towns and their charioteers in the kingdom of Ugarit (e.g., *PRU*, III, pp. 192-3, No. 12.34). Now, while the common meaning of *parashim*, 'horsemen', (from 9th/8th centuries BC?) was 'cavalry', this meaning *cannot* be mechanically applied at periods when chariotry was the rule and formal cavalry not yet 'invented'! In pre-cavalry days, therefore, we should take 'horseman' to mean 'charioteer' — one who rides behind a horse, not on it. This is *not* special pleading. In Egypt, in the 16th-12th centuries BC, the texts repeatedly refer to this or that pharaoh *hr htr*, 'on horse', meaning *in*

his chariot! On one fine stela, Amenophis III 'appears "on horse" like the sun's radiance', and this text describes a scene of this king driving his chariot (Petrie, *Six Temples at Thebes in 1896*, plate 10). And so in many temple-scenes. Hence in OT also, understand 'horseman' as 'charioteer' till perhaps Ahab's day or later — then as cavalry. These matters will be gathered up in *HHAHT*, chapter iv.

### 13. SOLOMON'S TEMPLE (pp. 152-189)

Dr Gray wrestles valiantly with the undoubted lexical and technical difficulties in this section. But a desperate resort to literary fission — 'primary' and 'secondary' accounts (pp. 158-164; 178/9) — seems too drastic, a symptom of failure. Deuteronomic disease strikes again on pp. 157/8.

P. 153: Dr Gray is right to reject Waterman's ridiculous view that the amount of storage-room at the Temple indicates that it was first a royal treasury, not a shrine. Storage-space commonly exceeded 'worship-space' in Near Eastern temples. In the 14th/13th centuries BC, compare (i) the Hittite 'Temple I' at Boghaz-koy (e.g., Ceram, *Narrow Pass, Black Mountain*, 1956, p. 200 and fig.), and (ii) in Egypt the Ramesseum and Medinet-Habu temples with brick magazines and houses occupying 2 or 3 times the space of the central temple proper (e.g., W. Stevenson Smith, *Art and Architecture of Ancient Egypt*, 1958 (Pelican History of Art), pp. 218-9, figs. 71-72).

P.163: Perhaps the gold overlay of the temple-floor is not entirely 'unlikely' or 'legendary' if one recalls the references to the 'silver floor' in the Karnak Temple of Amun in Egyptian Thebes, from the 15th to the 7th centuries BC, where the god Amun gave oracles. The Abydos temple of Sethos I had a staircase with a silver floor, c. 1300 BC. See Nims, *JNES* 14 (1955), 116; Parker, *A Saite Oracle Papyrus from Thebes*, 1962, pp. 7, 8.

Pp. 186/7: the familiar vices of much OT 'scholarship' show themselves. Instead of detailed description, Kings merely summarizes the gold vessels — which 'contrasts strangely' therewith, 'has been taken as suspect' and (of course!) 'is probably a late elaboration' — in fact, post-exilic because similar to the Tabernacle-furnishings of 'P'. None of this stuff occurs in the goldwork of 1 Kings 6: 20ff., and this 'profusion of

gold' is unknown to 2 Kings 12: 13, to 25: 14ff., and to Jeremiah 52: 18ff. One hesitates to blame Prof. Gray too much for merely following the vices of the crowd. But when will it be understood that it is commonplace in Ancient Near Eastern narratives to summarize whenever greater detail seemed superfluous to the ancient writers? The Egyptian general Uni c. 2300 BC gives detail of only two out of several campaigns: the first one and the last (with novel tactics); and so on. There is nothing strange or suspect about this. The goldwork merely continued the Tabernacle tradition and needed no new description. The Tabernacle itself (and contents) was *not* a post-exilic pipe-dream, but drew on a long Egyptian tradition of 'prefab.' shrines (c. 3000 to 13th century BC ff.); cf. Kitchen, *Tyndale House Bulletin*, 5/6 (1960), 7-13, and Gooding, *NBD*, fig. 202 and pp. 1,231/4. As for the 'ignorant' passages cited, 1 Kings 6: 20ff. is irrelevant because it deals only with the *décor* of the temple structure (including cherubs). 2 Kings 12: 13 is also irrelevant, because it merely states that the *new* income in Joash's chest was not diverted to furnishings but kept to the job in hand. 2 Kings 25: 14ff. and Jeremiah 52: 18ff. (both relating to 587/586 BC) are also irrelevant; Dr Gray is here 10 years too late, because Nebuchadnezzar had already cleared out most of the gold worth having in 597 BC! Dr Gray should have read 2 Kings 24: 13 and Jeremiah 27: 16 for his missing gold. An appalling misfire.

### III. THE DIVIDED MONARCHY

#### 14. QUE AND MUSRI (cf. pp. 250, 466, 468 n.g, 473; 585)

In 1 Kings 10: 28, Solomon obtained horses from 'Egypt' and *miqqwē*. The latter is now generally (and probably rightly) taken to be 'from Que', i.e., Cilicia (e.g. rsv; Gray, 250). But since the discovery of one or more lands Musri, 'Egypt' (Misrayim) in this verse has often been thought to be one of these, esp. one in South East Anatolia — so Prof. Gray of this verse and 2 Kings 7: 6. But there are good reasons for retaining the meaning Egypt: (i) The Northern Musri of Assyrian texts is N.E. of Assyria (*not* in Anatolia), and was early brought under Assyrian rule; it had no kings by the time of 2 Kings 7: 6. (ii) The Anatolian/Cilician Musri just does not exist; it never appears among the Neo-Hittite

and Aramaean kingdoms of the Hittite-hierogl. texts and Assyrian sources. Dr Gray has failed to use the important article by Tadmor on 'Que and Musri' in *IEJ* 11 (1961), 143-150, and Garelli's detailed study of 'Musur' in *Dictionnaire de la Bible, Supplément*, V, 1957, cols. 1,468-74, both vital. (iii) A North-Musri in 1 Kings 10: 28 makes economic nonsense. The Neo-Hittite and Aramaean states were in *direct* frontier contact with Cilician Anatolia, and could obtain horses there without any help from Solomon far to the South! Hence read 'Egypt', and Solomon's role of middleman between Egypt and North Syria is clear. He could import horses (different sorts) from both areas, selling the Egyptian ones (and Egyptian-made chariots as luxury-articles) to the North-Syrian rulers. See *HHAHT*, chapter iv. (iv) As for 2 Kings 7: 6 (pp. 250, 466, 473), the Musri-contingent at Qarqar in 853 bc (or later) was probably from Egypt. Note the 'tribute' from this Musri: Arabian camels, hippopotamus, rhinoceros, monkeys, etc. (*ANET*, 281), exotic gifts that make sense from Egypt (with its links into Africa), but impossible from Anatolia! Hence we may assume that Osorkon II of Egypt (roughly 870-850 bc) first sent help to the Syrian allies, then later sent a diplomatic present to Assyria. The plural 'kings of the Hittites and kings of Egypt' may be simply parallelism of expression, or reflect the co-regency of Osorkon II with Harsiese if not with Takeloth II. On p. 584/5, Dr Gray still toys with an Arabian Musri, even though (as pointed out by Garelli and Tadmor) this has been thoroughly refuted more than once by Assyriologists. Shilkanni (and variants) is therefore an Egyptian ruler; see So, below.

#### 15. ARAMAEANS AND HITTITES

(a) *Rezon*. There is *no* contradiction (*pace* p. 136) between the extent of Solomon's realm at his accession and following (from Euphrates to the Egyptian border) in 1 Kings 4: 21 (Heb. 5: 1), and the revolt of Rezon and his capture of Damascus (11: 23ff.). For many years Rezon was no more than a *condottiere*, like David and the men of Adullam (so also Gray, p. 267), and only secured Damascus later on in Solomon's reign as Dr Gray eventually admits (p. 266 and *n.c.*, but with needless emendation); see *HHAHT*, Table IV, commentary. The structure of David and Solomon's empire was as follows: (i) Direct

rule of the homeland (Judah and Israel); (ii) Direct rule by governor(s) of Damascus and Aram-Zobah (until lost to Rezon); (iii) Overlordship of Hamath as subject-ally (Toi and his son). The kingdom of Hamath reached to the Euphrates, and so therefore did the power of David and Solomon; see *HHAHT*, Tables IV, V, and ch. iii. 'The land of the Philistines' is unobjectionable; see now Malamet, *JNES* 22 (1963), 14-17. Solomon's rule from Euphrates to Egypt is not an 'extravagant claim' (p. 136): it was plain fact. (b) *Hezion*. Professor Gray rightly rejects (p. 320) various silly emendations for the name; a like-named Aramaean ruler of this period at Gozan was Hadianu (Albright, *BASOR*, 87 (1942), 26, n. 7 and *Anatolian Studies* 6 (1956), 84, n. 53; cf. *HHAHT*, Table X). He is commendably cautious over the Benhadad stela (p. 320/1), and rightly rejects Albright's attempt to fuse Benhadad I and II (p. 374).

(c) *Later points*. On p. 376 (for 1 Ki. 20: 12), Dr Gray seems to have missed Yadin's study of Israelite and Aramaean strategy, including the possibility that Succoth is here the place-name (*Biblica* 36 (1955)).

P. 558 and n. a: despite its difficulties, 2 Kings 14: 28 is probably more meaningful and reliable than Dr Gray credits. For the power of Jeroboam II (and then of Uzziah) that it reflects, see Tadmor, *Scripta Hierosolymitana*, VIII (1961), 238ff., esp. 248ff., 266ff.; also *HHAHT*, Tables II, IV, V and ch. iii.

P. 570 and n. c: Emendation of Rezin to Rason (to suit the Greek) is mistaken. The Assyrian form is Rahianu (not Rasunu) as pointed out by Landsberger, *Sam'al I*, 1948, 66, n. 169 and others since. Rahianu = Aramaic Ra'yan, for which the Canaanite/Hebrew equivalent is R(a)syan, contracting to Resin (English: Rezin). (On the relations of Israel and Aram, note the instructive paper by B. Mazar, *Biblical Archaeologist* 25 (1962), pp. 98-120, apparently unused by Dr Gray.)

#### 16. 'SO, KING OF EGYPT' (cf. pp. 583, n. a; 584-5)

Professor Gray still persists in equating So of 2 Kings 17: 4 with 'Sib'e, *turtan* (army-commander) of Egypt', although the latter name is to be read Re'e not Sib'e (see Borger, *JNES* 19 (1960), 49-53). However, he wisely avoids dragging the later king Shabako into the problem. In 716 bc (not 715 — see Tadmor, *JCS*

12 (1958), 77, 78), Sargon II of Assyria took tribute from Shilkanni king of Egypt, *i.e.*, Osorkon IV ('*Akheperrē*'), last king of Dyn. 22, who ruled from c. 730/729 BC (at latest). Hoshea's conspiracy occurred c. 726/725 BC, and So could be an abbreviation for Osorkon; *cf.* Sese for Ramesses much earlier on, see *NBD*, 1201 on So. About 728/727 BC, Tefnakht of Sais in North West Delta proclaimed himself king. Goedicke (followed by Albright) now suggests reading in Kings '... to Sai(s), (to) the king of Egypt', *i.e.*, to Tefnakht (*BASOR*, 171 (1963), 64-66). This is also possible, although it requires an emendation (insertion of 'el', 'to', into the text). Thus, 'So' is either Osorkon IV or Tefnakht, but either way involves a pharaoh, and correctly despite Gray, p. 585. On this period in Egypt, see *ThIP*.

#### 17. TIRHAKA, HEZEKIAH AND SENNACHERIB (*cf.* pp. 37, 602-6, 616, 623/4)

Professor Gray claims that in 701 BC, Tirhaka (Taharqa) of Egypt was too young to lead troops (because aged 9 or 10) against Assyria, following Macadam's view of Taharqa in *Temples of Kawa I*, 1949, and joins his OT colleagues who naively assume that Macadam's is the last word on Taharqa. They are consequently up to twelve years out-of-date in ignoring the vital study by Leclant and Yoyotte, *BIFAO* 51 (1952), 15-27. Taharqa was a son of Piankhy who died at least 14 years before the accession of Taharqa's brother Shabataka; the latter then summoned Taharqa (aged 20) to Egypt from Nubia. It is possible that this occurred in 702 BC, so that in 701 Taharqa at 21 would be quite old enough to lead an army to defeat. The title 'king' given him in Isaiah and Kings is from after Taharqa's own accession in 690 BC, a proleptic reference by the Hebrew writers after that date. On this basis, there need be *no* Taharqa problem, no need or confusion of two campaigns of Sennacherib, no inaccuracy or anachronism (despite Rowley). There was no coregency of Shabataka and Taharqa (despite Gray, p. 624), as shown by Leclant and Yoyotte and by Schmidt in *Kush* 6 (1958), 121-130. Again, all this is being covered in *ThIP*.

#### 18. MERODACH-BALADAN (*cf.* pp. 606, 637/8)

Professor Gray's treatment of this king (Marduk-apal-iddina II) is neither ade-

quate nor accurate. He ruled in Babylon in 722-710 BC (not 720-709) until Sargon II expelled him. But in 703 (not 702), he regained the Babylonian throne and held it for nearly a year (nine months, in fact) and might well then have sought allies like Hezekiah. Sennacherib then expelled him, but he *again* made a bid for Babylon and was pursued by Sennacherib in 700 BC — within a year of Hezekiah's successful resistance to Assyria. It is not impossible that Merodach-Baladan sent to Hezekiah in 701/700, on hearing of Hezekiah's survival. He still claimed Babylonian kingship, a 'government in exile' as we say. These possibilities are also allowed for by Leemans, *Jaarbericht Ex Oriente Lux* 10 (1945/48), 452-3: '703 or still later' being termed possible. Even after 700, his family maintained their pretensions; his son and grandson continued to trouble Assyria. Even when out of Babylon, he and they still could use either their homeland Bit-Yakin as a base of operations (incl. for embassies) or intrigued from neighbouring Elam. All this escaped Gray, p. 606; see *HHAHT*, Table XVIII, § 5. On p. 638, we have a real howler in the claim that an Assyrian text proves the OT wrong by naming Merodach-Baladan's father as Yakin. No Assyro-Babylonian text has yet given us the name of his father. Dr Gray has simply lifted a mistranslation from Luckenbill where one should render: 'Merodach-Baladan of Bit-Yakin', lit. 'House of Yakin'. Any citizen of a land Bit-x is called *mar-x*, lit. 'son of x', meaning only 'person of Bit-x'. This is very well known in Akkadian, and has various developments, see *HHAHT*, Table IV, commentary. The OT patronymic could be a Bel-iddin or (god omitted-) Apil-iddin in Babylonian.

#### 19. OTHER POINTS

(a) *Jeroboam's Return from Egypt*. Here, Dr Gray (pp. 278/9) sees a contradiction between 1 Kings 12: 1-3, 12 and verse 20: that Jeroboam came to the Shechem assembly *versus* he was only invited after the break with Rehoboam of Judah. This seems unnecessary. When an assembly was decided, his supporters would call Jeroboam from Egypt when the assembly-summons was sent out. Jeroboam then came to Rehoboam with the elders of Israel during the vital three days' negotiations, ending in the initial break of verses 16-18. Meanwhile, the return and leadership of Jeroboam rapidly became known to the mass of Is-

raelite people, and he was made king. No contradiction or complication need be invoked.

(b) *The plundering of the Temple in 597 and 587/6 BC.* Here, Dr Gray (p. 692) suggests that 2 Kings 24: 13 'is flatly contradicted by Jer. 27: 19f'. This itself is nonsense. The emphasis in the looting of 597 BC (2 Ki. 24: 13; Jer. 27: 16, under Zedekiah) is on all the most valuable, portable gold and silver vessels and general treasure. In the final sack of 587/6 BC (2 Ki. 25: 13ff., as well as Jer. 27: 19-22), all the bronzework, more ordinary vessels and remnants of gold and silver were cleared out. Where is the contradiction here? None, unless the word 'all' be given a specially absolute force that it so often lacks in the Ancient Near East.

#### IV. CONCLUSION

What, then, should be said of Professor Gray's new volume? It does indeed represent a great deal of hard and meritorious work by Professor Gray, particularly on the Hebrew and Greek text. This latter point, subject to cautions already expressed (on Arabic and emendation) will probably be the book's main value. On the historical side (esp. Egypt) and use of Ancient Near Eastern background ('fresh archaeological evidence') this book is definitely inadequate; the preceding 19 paragraphs contain much of my evidence for that assertion. Most of the material used above could have been examined by Dr Gray down to 1960-61, even some of that which for brevity has been referred forward to *AO/AT*, *HHAHT* and *ThIP*. In any future edition, he would be well advised to take the extant text of Kings more seriously and to cover the Ancient Oriental background more thoroughly. Frankly, this book is likely to be widely used in colleges, etc., as it is the only recent English work in the field besides Montgomery, which is probably too technical in some respects for other than advanced students. It is for that reason that this review is so detailed, to enhance if possible the book's utility by filling some of the lacks. For this review has been conducted in no unfriendly spirit; the Aberdonian-born reviewer would dearly have liked to have given a far rosier verdict to this first British (and Scottish) contribution to its series. But there is, one hopes, a future, and second editions can sometimes outclass their originals. That closing good wish is mine for Dr Gray's work.

#### ABBREVIATIONS USED IN THIS REVIEW:

*ANET* — J. B. Pritchard (ed.), *Ancient Near Eastern Texts relating to OT*, 2nd ed., 1955.

*AO/AT* — K. A. Kitchen, *Alter Orient und Altes Testament*, 1965; English ed., *Ancient Orient and Old Testament* to follow it.

*BASOR* — *Bulletin of the American Schools of Oriental Research*.

*BIFAO* — *Bulletin de l'Institut français d'Archéologie Orientale*, Cairo.

*HHAHT* — K. A. Kitchen, *Hittite Hieroglyphs, Aramaeans and Hebrew Traditions*, delayed but forthcoming.

*ICC* — *International Critical Commentary*.

*IEJ* — *Israel Exploration Journal*.

*JCS* — *Journal of Cuneiform Studies*.

*JEA* — *Journal of Egyptian Archaeology*.

*JNES* — *Journal of Near Eastern Studies*.

*LAR* — D. D. Luckenbill, *Ancient Records of Assyria and Babylonia*, I & II, 1926/27.

*MN* — E. R. Thiele, *Mysterious Numbers of the Hebrew Kings*, 1951.

*NBD* — F. F. Bruce, J. I. Packer, R. V. G. Tasker, D. J. Wiseman, J. D. Douglas, ed. *The New Bible Dictionary*, IVF, 1962.

*PRU* — *Le Palais Royal d'Ugarit*, Vol. II by Ch. Vroilleaud, 1957; Vol. III by J. Nougayrol, 1955.

*SAP* — K. A. Kitchen, *Suppluliuma and the Amarna Pharaohs*, 1962.

*ThIP* — K. A. Kitchen, *The Third Intermediate Period in Egypt*, forthcoming.

The reviewer must again apologize for referring the reader to works not yet published (cf. his review of *A History of Israel* by John Bright, 1960, in *TSF Bulletin*, 29 (1961), p. 20 and 39 (1964), p. viii of Supplement). This has been owing to delays beyond his control. When issued (DV), their relevance will be evident.