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3. LATER HEBREW CHRONOLOGY

I. THE DATE OF THE EXODUS AND ISRAELITE INVASION OF CANAAN¹

Here also the biblical and other evidence is very complex, and is often considered to contain a variety of serious contradictions.² But the difficulties and supposed contradictions are less serious than they are often made to appear, if the available data is treated positively and in proper accord with known Ancient Near Eastern usage. The material is here dealt with under five heads.

(a) *Egyptian Evidence*

First, Exodus 1:11 links the oppression of the Israelites with the building of the store-cities of Pithom and Ra'amses, giving thereby an indication of date for the *end* of the oppression and for the Exodus.³ Ra'amses is most probably⁴ the Pi-Ramessē of

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Egyptian texts,⁵ founded by Sethos I and mainly built (and named) by Ramesses II. The Exodus, therefore, is best dated after the accession of Ramesses II (1304 or 1290 BC).⁶ There is no reason

¹ On the date of the Exodus, cf. earlier C. de Wit, *The Date and Route of the Exodus*, 1960 (good conspectus of previous studies), and Kitchen and Mitchell, *NBD*, pp. 214.-216 (brief outline of treatment used here).

² Cf. the detailed and painstaking study by H. H. Rowley, *From Joseph to Joshua*, 1950, who offers a rather too complicated reconstruction of the course of events.

³ Giving no hint as to how long the oppression lasted, Ex. 1:7-14 describes the oppression very briefly in general terms, esp. verse 14, a general summary of building and other field-work. The failure of this oppression to reduce the Hebrews led to the edict of Ex. 1:16 and the Hebrew evasion of it that sets the stage for the birth of Moses in Ex. 2. The narrator gives just *one* concrete example of the work done by the Hebrews, 'and they built for Pharaoh store-cities, Pithom and Ra'amses', 1:11b. We have *no* warrant to assume either that the Hebrews were employed exclusively on Pithom and Ra'amses (note 1:14, 'and in all manner of service...'), or that the oppression began only with this project. In fact, it is much more likely, that Pithom and Ra'amses were their *last* major taskwork before the Exodus itself, because (i) they actually set off from the vicinity of Ra'amses (cf. Ex. 12:37; Nu. 33:3, 5), and (ii) they would retain most vividly in memory and record the names and scenes of their last labours before leaving Egypt, not those of a generation earlier. In other words, it should not be lightly assumed that Moses' birth was later than the start of Hebrew labours on Pithom and Ra'amses (as did Rowley, *Expository Times* 73 (1962), pp. 366-367, thereby imposing artificial problems on Ex. 1, and *NBD*, pp. 214-216). For the literary usage in Ex. 1 of general terms and a specific isolated point, cf. the generalities and specific reference to the Shardana in Tanis stela II of Ramesses II, J. Yoyotte, *Kémi* 10 (1949), pp. 62, 63.

⁴ Recently, J. Yoyotte suggested that the biblical Ra'amses should perhaps be identified not with the great metropolis Pi-Ramessē, but with a smaller place Ramessē-pa-demi (Ramessē-the-town), cf. J. Sainte Fare Garnot, *Revue Historique*, fasc. 4.59 (1961), p. 118, n. 2; but this does not affect the chronological argument.

to doubt the Hebrew text at this point, and the possible sites of Pi-Ramesse - Tanis⁷ or Qantir,⁸ or both⁹ -

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were original foundations by Sethos I and Ramesses II,¹⁰ so that the Exodus can hardly be dated in the preceding Eighteenth Dynasty as was once thought by some scholars,¹¹ who argued that the name Ra'amses was original neither in the Hebrew text nor in the name of the city on Egyptian monuments.

Secondly, the so-called 'Israel Stela' commemorates a victory of Merenptah over the Libyans in his fifth year (c. 1220 BC), and mentions places and peoples in Syria-Palestine claimed as subdued by Merenptah - including Israel. This clearly suggests that Israel was already in Western Palestine by 1220 BC.¹² Some scholars, however, have doubted whether Meren-

⁵ References first collected by Gardiner, *JEA* 5 (1918), pp. 127-138, 179-200, 242-271; subsequent references and discussion in Gardiner, *Ancient Egyptian Onomastica*, II, 1947, A.410, pp. 171*-175*, 278*-279*. The treatment of Ex. 1:11, Pithom and Ra'amses by D. B. Redford, *VT* 13 (1963), pp. 401-418, is misleading and almost worthless; cf. the able refutation and corrections given by W. Helck, *VT* 15 (1965), pp. 35-48.

⁶ A combination of astronomical (lunar) and historical data makes it certain that the accession of Ramesses II occurred either in 1290 BC or fourteen years earlier in 1304 BC (cf. R. A. Parker, *JNES* 16 (1957), pp. 42-43). The 'high date' 1304 BC is advocated by Rowton (*JCS* 13 (1959), pp. 1-11; *JNES* 19 (1960), pp. 15-22), while the more commonly accepted 'low date' 1290 BC is methodically defended by E. Hornung, *Untersuchungen zur Chronologie and Geschichte des Neuen Reiches*, 1964. In point of fact, the available evidence is still indecisive; see my review of Hornung's valuable monograph in *Chronique d'Égypte* 40/Fasc. 80 (1965), pp. 310-322. Hence I retain, dates based on 1290 for the accession of Ramesses II merely provisionally; 1304 dates must also be reckoned with at present.

⁷ P. Montet, *RB* 39 (1930), pp. 5-28; *Géographie de l'Égypte ancienne*, I, 1957, pp. 194-197; *Revue Archéologique*, 1958-1, pp. 1-20; *L'Égypte et la Bible*, I 1959, pp. 54-55; H. Kees, *Tanis*, 1942, pp. 150-155.

⁸ M. Hamza, *ASAE* 30 (1930), pp. 31-68 (esp. 64-68); L. Habachi, *ASAE* 52 (1954), pp. 443-447, 500, 510-514, 545-559. Also W. C. Hayes, *Glazed Tiles from a Palace of Ramesses II at Kantir*, 1937, pp. 5-8; and *Scepter of Egypt*, II, 1959, pp. 332-333, 334-339. Good discussion of pros and cons (without final verdict) is B. Couroyer, *RB* 53 (1946), pp. 75-98.

⁹ Taking Pi-Ramessē in the broad (and administratively-correct) sense of 'Estate (not "House") of Ramesses', including Tanis and Qantir in one domain of scattered settlements and institutions. See Gardiner, *Anc. Eg. Onomastica*, II, p. 175*; C. F. Nims, *JNES* 9 (1950), p. 261; A. Alt, *FS Zucker*, 1954, pp. 7-8 (=KS, III, 1959, pp. 181-182); H. Kees, *Ancient Egypt, A Cultural Topography*, 1961, p. 201; W. Helck, *VT* 15 (1965), p. 41. This view is probably the most realistic at present.

¹⁰ Cf. Montet (note 7 above) and Habachi (note 8 above).

¹¹ As the official building-works of the Ramesside kings in the E. Delta are usually found to be the first original works there since the Hyksos period four centuries earlier, they are not mere usurpations of Eighteenth Dynasty structures as is sometimes suggested by advocates of a fifteenth-century date for the Exodus (e.g., J. W. Jack, *The Date of the Exodus*, 1925, pp. 22-32). This can be seen from (e.g.) the frequency of genuine Ramesside (and absence of Eighteenth Dynasty) monuments in Lower Egypt in B. Porter and R. L. B. Moss, *Topographical Bibliography of Ancient Egyptian Hieroglyphic Texts (etc.)*, IV, 1934, pp. 1-68 (esp. 1, 6-44, 52-58); so far, only Bubastis has both (cf. L. Habachi, *Tell Basta*, 1957), and this city is no candidate for identification with Ra'amses/Pi-Ramessē.

¹² The equation of the name *Ysr'r* (Egyptian for *Ysr'l*) on the Merenptah stela with Hebrew Israel is universally recognized by all competent philologists in Egyptian and Semitic, and likewise with the Israelites who left Egypt at the Exodus explicitly and uniformly commemorated in the Pentateuch and other OT writings. Yet Eissfeldt, without evidence, would claim that *Ysr'l* is ambiguous because it could well be for Jezreel (*CAH*², II: 26a (*Palestine in the Nineteenth Dynasty...*), 1965, p. 14). This is an incredible 'howler'; Heb. z appears as z or t

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ptah ever warred in Palestine, and so they suggest that the stela indicates merely that Israel left Egypt in Merenptah's first years.¹³ But independent proof that Merenptah *did* conduct at least one small campaign in Palestine is given by an inscription at the temple of Amada in Nubia - overlooked completely by most scholars - in which the title 'Binder of Gezer' (in Palestine) is given equal prominence with the strictly parallel title 'Seizer of Libya' that refers to his well-known Libyan war.¹⁴ Some Israelites must, therefore, have had a minor clash with Merenptah's troops in West Palestine before 1220 BC, and so the Exodus and first phase of the Israelite conquest must be earlier than 1220 BC as well as later than 1290 (or 1304) BC (earliest date for Ra'amses).

(b) Duration of the Wilderness Journeyings

Originally, Israel were to go from Egypt through Sinai directly to Canaan; the forty years in the wilderness was a punishment for disobedience, and its explicit purpose was to replace the rebellious generation by a new generation (Nu. 14:21-23; 32:9-13; Dt. 2:14). The forty years should not be dismissed as a meaningless round figure, because it is explicitly made up of

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thirty-eight years¹⁵ (Dt. 2:14) plus other short periods of time, totalling forty years altogether (Nu., *passim*; Dt. 1:3). There fore, this particular forty-year period must be taken seriously in dealing with this epoch. This means that if the Exodus is after 1290 BC, the start of the conquest under Joshua cannot then precede *c.* 1250 BC; similarly, if the conquest began before 1220 BC, the Exodus would not be later than *c.* 1260 BC. The limits for the date of the Exodus are then roughly 1290/1260 BC, and for the start of the conquest about 1250/1220 BC. A rough average date would

in Egyptian, not *s* (*cf.* Helck, *Beziehungen*, pp. 589, 554:18), and *Ysr'l* wholly lacks the 'ayin of Jezre'el! Noth's idea that *Ysr'l* could be some earlier entity than OT Israel bearing the same name (*History of Israel*², 1960, p. 3) is equally baseless. No evidence exists for such a 'double'; and why not also claim that Egyptian 'Moab', 'Edom' and 'Se'ir' are different from their OT homonyms? Why these evasions? Simply that the tribal Israel as an entity in W. Palestine in 1220 BC, pictured by the Old Testament and tacitly by the Merenptah-Sela (by determinative of 'people'), does not suit their particular theories about Israelite origins, and they prefer these theories to the first-hand evidence of the stela. No wonder that, in another context, Albright found himself writing that 'German [OT] scholars are inclined... to close their eyes' to archaeological and linguistic data (*History, Archaeology and Christian Humanism*, 1964, p. 267) - a situation distressingly close to obscurantism. *Cf.* THB 17 (1966), pp. 90-92 (where read *d, t*, in *ydr', *ytr', p. 91).

¹³ See E. Drioton, 'La Date de l'Exode' in *La Bible et l'Orient*, 1955, p. 45, and C. de Wit, *The Date and Route of the Exodus*, 1960, p. 10.

¹⁴ Published by U. Bouriant, *RT* 18 (1896), p. 159; H. Gauthier, *Le Temple d'Amada*, 1913, pp. 187-189 and pl. 41:B, and *Livre des Rois d'Égypte*, III, p. 118. New copy by J. Černý, *Amada: Stela of Amenophis II and Inscription of Menepthah*, Cairo, Centre of Documentation, n.d.; *cf.* A. Youssef, *ASAE* 58 (1964), pp. 273-280 and plate.

¹⁵ This thirty-eight years, a real and not 'round' or schematic figure, was occupied by wanderings as is clearly stated in Dt. 2:14, and was not simply spent at Kadesh as Rowley states (*From Joseph to Joshua*, p. 133), perhaps by a slip of the pen. His conclusion as to its artificiality is wishful thinking unsupported by any tangible evidence and therefore invalid; the same is true of Alt, *KS*, I, 1953, p. 163 n. 3.

be c. 1280 and c. 1240 BC respectively, or perhaps up to a decade later (*cf.* p. 67, below). (If Ramesses II's accession were in 1304 rather than in 1290, all these dates would then average some fourteen years earlier.)

(c) *Palestinian Evidence*

First, in Transjordan, Glueck's surveys show a renewed density of occupation from about 1300 BC, after a lapse of five centuries since the Patriarchal age,¹⁶ a situation reflected in Egyptian sources, where in the Nineteenth Dynasty we suddenly find references to the Transjordan lands and peoples lacking in the Eighteenth Dynasty - to Moab, Se'ir, Edom - and evidence for forces of Ramesses II having campaigned in Moab and Se'ir, presumably before the Israelites under Moses reached the 'plains of Moab'.¹⁷ As Israel were opposed by strong kingdoms in Edom and Moab and had to go round these (Nu. 20:14-21; Jdg. 11:17), the Exodus and wanderings of

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Israel are unlikely to have been earlier than c. 1300 BC, the approximate date of foundation of these kingdoms.

Secondly, let us look at the situation in Western Palestine. After the crossing of Jordan,¹⁸ the capture of Jericho and Ai, and the submission of the Gibeonites, Joshua had to conduct one campaign in Southern Palestine and then another in Galilee; then he and his contemporaries and successors had to try to occupy a Canaan defeated but not fully or finally conquered. Joshua continued the work of Moses in apportioning the land (Jos. 13). At Gilgal, he assigned land to Caleb, his tribe Judah, and to the Joseph-tribes (Jos. 14-18); and at Shiloh allotted land for the other tribes to occupy (Jos. 18-22). Before his death, Joshua exhorted the leaders and people privately (Jos. 23) and publicly (Jos. 24). Caleb was some forty years old when Moses sent him and others to spy out Canaan; he claimed his inheritance forty-five years later, after the campaigns of Joshua 6 to 11, aged eighty-five years (Jos. 14:6-11). As nearly forty of those forty-five years were spent in the wilderness journeyings, this suggests that the events of Joshua 1 to 12 occurred within five or six years of Moses' death. During what remained of the lifetimes of Joshua and the elders, the tribes had the task of beginning to occupy the territories assigned them at Gilgal and Shiloh.

Excavations at several sites in Palestine suggest that the Israelite conquest began during the second half of the thirteenth century BC, thus agreeing with the Egyptian evidence, although two sites have produced results which appear divergent at first sight.

¹⁶ *Cf.* notes 38, 40 to p. 43, above; also Glueck, *The Other Side of the Jordan*, 1940, pp. 125 ff., 128 ff., 134 ff., 140 ff. For a note of caution (but applying more to the Jordan valley), see also Albright, *BASOR* 90 (1943), note 77a to pp. 17-18.

¹⁷ For details and the new data on Moab and Dibon, *cf.* Kitchen, *JEA* 50 (1964) pp. 50, 53-55, 63-67, 69-70.

¹⁸ *Cf.* J. Garstang, *Joshua, Judges*, 1931, pp. 136-138 and plate 25.

At Jericho (Tell es-Sultan), the Late Bronze Age levels appear to have been almost completely washed away¹⁹ during the four centuries that the mound lay desolate from Joshua until Ahab's time;²⁰ in barely half that length of time (during *c.*

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1600-1400 BC), most of the Middle Bronze Age city had been eroded away,²¹ so that this is a real factor to be reckoned with and not just a harmonistic excuse.²² Excavations at Et-Tell have failed to produce any proper evidence of occupation there after the Early Bronze Age (*c.* 2400 BC),²³ apart from a small Israelite settlement (Iron I) of *c.* 1200-1050 BC. Despite assertions sometimes made to the contrary, this situation suggests that Et-Tell is not Ai but another ancient site (Beth-Aven?),²⁴ and that Ai must be looked for somewhere else in the area and not on Et-Tell. When mounds and literary records fail to agree in other cases, topographers and archaeologists do not panic but simply use their common sense, recognize that they were probably mistaken in their identification,²⁵ and proceed to search elsewhere

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in the region.²⁶ The problem of Ai should be regarded in exactly the same way. Jericho and Ai are lessons in negative evidence: the absence of the expected body of remains of Late

¹⁹ For what follows, *cf.* Kitchen, *NBD*, pp. 215-216, and esp. 612-613. For re-dating of Garstang's 'Late Bronze' wall as Early Bronze, see K. M. Kenyon, *Digging Up Jericho*, 1957, pp. 170-171, 176-177, and esp. 181.

²⁰ It is possible, also, that the Late Bronze Age inhabitants continued to use the Middle Bronze Age ramparts (oral suggestion by Prof. Y. Yadin); *cf.* also Albright, *The Biblical Period*⁴, 1963, p. 28, citing Tell Beit Mirsim where this is attested.

²¹ Kenyon, *op. cit.*, pp. 170-171; *cf.* pp. 45, 93, 259-260, 262-263; *PEQ* 92 (1960), p. 108. *Cf.* Albright, *op. cit.*, pp. 28-29.

²² Positive evidence that a settlement existed at Jericho in the thirteenth century BC comes from the tombs, these yielding Mycenaean pottery and imitations of such. The pottery is of thirteenth-century type (*cf.* Albright, *op. cit.*, p. 100 n. 59, and in *Bibliotheca Orientalis* 21 (1964.), p. 69). Its rarity merely reflects the inland location of Jericho, like Hama(th) which yielded only two such sherds from its thirteenth-century occupation (Kitchen, *NBD*, p. 216 top, after Hanfmann, *JNES* 12 (1953), pp. 206-207).

²³ G. E. Wright, *Biblical Archaeology*, 1957, p. 80; Albright, *op. cit.*, 1963, p. 29 and n. 60.

²⁴ See J. M. Grintz, *Biblica* 42 (1961), pp. 201-216; note how well the occupational history of Et-Tell corresponds to the literary evidence for Beth-Aven, and is utterly different from that for Ai (Grintz, *op. cit.*, pp. 213-215, with p. 207)! Ai may not be a full-sized tell, but only separate (if neighbouring) Middle Bronze, Late Bronze and Iron II settlements in this area, and not easily found. One thinks of the searches for Teiman or pre-Hellenistic Gerasa in Transjordan (*cf.* N. Glueck, *The Other Side of the Jordan*, 1940, pp. 21-26, 121-123).

²⁵ It is only the approximate geographical suitability and the presumed play on the name that has recommended Et-Tell as the site of Ai, and no scrap of positive *proof* (such as inscriptions *in situ* as at Gezer) exists to justify Noth's uncritical belief (*VTS*, VII, p. 273) that the equation of Et-Tell with Ai is 'beyond all doubt'. There are very serious doubts on both grounds cited, *cf.* Grintz, *op. cit.*, pp. 208-211 (name), 207 end (locus). New excavations at Et-Tell have as yet added little, but at least the neighbouring Khirbet Haiyan can apparently be ruled out as purely Islamic (*cf.* E. F. Campbell after J. A. Callaway, *BA* 28 (1965), p. 28).

²⁶ Thus, 'Agir was once thought to be Ekron, but no pottery-evidence could be found to support this, despite similarity of name; Ekron may rather be located at Khirbet el Muganna which shows a suitable history of occupation and is topographically acceptable (see J. Naveh, *IEJ* 8 (1958), pp. 166 ff.; *cf.* B. Mazar, *IEJ* 10 (1960), pp. 106 ff.). Similarly, Khirbet Tarrama with nothing earlier than Hellenistic pottery could not be Debir as Noth suggested; hence Albright and Wright suggested the more fitting Tell Beit Mirsim (*cf.* Wright,

Bronze Age date does not automatically imply that the biblical narratives are inventions or aetiological tales.²⁷ The circumstantial realism of the topographical allusions and of Joshua's leadership suggest otherwise, as does the analogy of archaeological failure to produce remains tallying with other - and indisputably original - Ancient Oriental written documents.²⁸

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The excavations at Gibeon²⁹ afford a further sharp lesson on the unreliability of negative evidence: the first three seasons of excavation found no trace of the Late Bronze Age city presupposed in Joshua 10:2.³⁰ But in the fourth campaign, the discovery of a few very fine tombs of that particular period has shown that there must in fact have been a Late Bronze Age settlement somewhere on the general site as required by Joshua 10:2.³¹

With the beginning of the conquest have been associated archaeological destruction-levels at Lachish (Tell ed-Duweir), Debir (if Tell Beit Mirsim), Bethel (Beitin), Tell el-Hesi (Eglon?) and Hazor (Tell el-Qedah or Waqqas).³² All of these show traces of catastrophic destruction in the later part of the thirteenth century BC, although the fall of Bethel has been thought to be earlier than the others. If one identifies these destructions at Lachish and Debir as resulting from Joshua's Southern campaign (Jos. 10), the earlier fall of Bethel (before Israel crossed the Jordan?) might seem a difficulty. To 'solve' it by postulating a separate history of the Joseph-tribes (as some do), or similar counsels of despair, would seem quite unnecessary, however.

First, the notion that Bethel perhaps fell earlier than Lachish and Debir is based on the superior quality of its pottery compared with that from the destruction-levels at the other two

JNES 5 (1946), p. 110, n. 12). Specially instructive is the case of Arad in S. Palestine, where the ancient town was apparently located at Tel Arad and Tell el-Milh in different epochs; both mounds together (and neither alone) would fit the whole history; see Y. Aharoni and R. Amiran, *IEJ* 14 (1964), pp. 144-147; cf. also B. Mazar, *JNES* 24 (1965), pp. 297-303; Yadin, *IEJ* 15 (1965), P. 180. There is no inherent reason for treating Ai as any more of a special problem than Ekron, Debir or Arad.

²⁷ As has been the tendency with Alt and Noth, for example (references, cf. Grintz, *op. cit.*, p. 205 and nn. 2-5); on their over-use of aetiology, cf. J. Bright, *Early Israel in Recent History Writing*, 1956, pp. 91 ff. The supposed standing still of the sun, or 'long day', of Jos. 10:12-13 sometimes causes difficulty, but it may rest on nothing more than mistranslation. Possibly cease *shining* rather than cease *moving* should be understood, and for 'about a whole day' one may definitely render 'as when day is done'; cf. provisionally R. D. Wilson, *Princeton Theological Review* 16 (1918), pp. 46-54.

²⁸ Thus in Egypt, for example, many stone temples are mentioned in documents (*e.g.*, the great Papyrus Harris I) or on monuments of their officials, but have never been found by archaeologists. Cf. lists in W. Helck, *Materialien zur Wirtschaftsgeschichte des Neuen Reiches*, I, 1961, pp. 137-139 (eight royal funerary temples never yet found); *ibid.*, II, 1961, pp. 157-190 (temples of provincial capitals, *etc.*). No trace of Ninth-Dynasty Heracleopolis has yet been found although that dynasty originated there, and so on.

²⁹ On the identification of El Jib as the site of Gibeon on the basis of inscribed jar-handles found there, cf. J. B. Pritchard, *VTS*, VII, 1960, pp. 1-2, and *Gibeon where the Sun stood still*, 1962, pp. 45-52 (topographical indications, *ibid.*, pp. 24-45 *passim*).

³⁰ Except for a single sherd of Cypriote ware (*VTS*, VII, p. 8 n. 2), but reported as two sherds of a bowl in *BA* 24 (1961), pp. 22-23.

³¹ *BA*, *loc. cit.*; *Gibeon....* 1962, pp. 135-138, 156-158; *The Bronze Age Cemetery at Gibeon*, 1963.

³² Wright, *Biblical Archaeology*, 1957, pp. 80-83; with references, *JNES* 5 (1946), pp. 110-111.

sites.³³ But this overlooks the fact that all the material culture at Bethel is of a high standard: well-built houses, paved or plastered floors, excellent drainage-system, exotic art ('Astarte

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cylinder-seal), and so on.³⁴ Thus, the Bethelites may simply have maintained a higher over-all cultural standard than did less important Debir, or Lachish subject to greater foreign (Egyptian) exploitation, and the chronological time-lag may be illusory.³⁵ The fall of Bethel to the Joseph-tribes is mentioned in Judges 1:22-26, but is given no explicit date; this incident could have been associated with the fall of Ai, or with the end of Joshua's Southern campaign, or with some other occasion.³⁶

Secondly, the final destruction of Canaanite Debir and Lachish probably represents the work of the Judah-tribe led by Caleb (Jos. 14:13-15; 15:13-19; Jdg. 1:10-15, 20) subsequent to the Southern campaign of Joshua (Jos. 10, esp. verses 31-33, 38-39) - It is clear from these references that Debir was smitten twice by the Hebrews; once during Joshua's flying campaign, and a second time by Caleb and the Judahites beginning a permanent settlement. The major destruction of Debir (if Tell Beit Mirsim, end of stratum C) should be associated with the second occasion (Caleb) - the first would leave little separate trace - as it was followed by an entirely different kind of occupation (stratum B₁) which is best identified as that of the newly settling Hebrews.³⁷ It is therefore possible that the major destruction of Canaanite Lachish also belongs to the follow-up campaigns of Caleb and Judah. Unlike Debir, however, the city was not immediately settled by Israel after its destruction; instead, the Philistines had a garrison there for a time.³⁸ The fall of Lachish may even perhaps be dated to the fourth or

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fifth year of the Egyptian king Merenptah (*i.e.*, c. 1220/1219 BC), for among the Late Bronze Age ruins was found an Egyptian hieratic ostracum dated 'Year 4' in the script of this period, relating to taxes (tribute for Egypt?) on the grain-harvest.³⁹ In other words, Caleb's campaign was perhaps not later than c. 1220/1219 BC, and may have begun a little earlier at Debir, while Joshua's Southern and Northern (and perhaps other, unmentioned) campaigns were earlier still, within about five years (see p. 62 above). This might put Israel's crossing of the Jordan at c. 1230/1225 BC, and the Exodus forty years earlier at about 1270/1265 BC, roughly. The fall of Bethel (if linked with Ai or the Southern

³³ Cf. Albright, *BASOR* 58 (1935), p. 13.

³⁴ Wright, *op. cit.*, p. 81: more detail, J. L. Kelso, *BA* 19 (1956), pp. 38-40; Albright, *Archaeology of Palestine and the Bible* 2, 1933, p. 101.

³⁵ Thus, metropolitan Ugarit in the thirteenth century BC enjoyed higher living standards than anything attested in Palestine then - but it was destroyed (by the 'Sea Peoples') later than any of the sites dealt with here, at c. 1200 BC (plus or minus a few years).

³⁶ Note that Jdg. 1:1, 'After the death of Joshua', does *not* refer to everything in Jdg. 1, only to verses 1-9 and 16-19. Jos. 14:13-15 and 15:13-19 show that Jdg. 1:10-15, 20 belongs in the lifetime of Joshua; the rest of Jdg. 1 (verses 21, 22-26, 27 ff.) is undated.

³⁷ Albright, *Archaeology of Palestine and the Bible* 2, 1933, pp. 101 ff.; cf. references in note 45 below.

³⁸ Possibly under Ramesses III of Egypt; cf. T. Dothan, *IEJ* 10 (1960), pp. 62-63.

³⁹ See Albright, *BASOR* 68 (1937), pp. 23-24, and *ibid.*, 74 (1939), pp. 20-22; and now, J. Černý in O. Tufnell, *Lachish IV: The Bronze Age*, 1958, pp. 132-133, cf. p. 36.

campaign) might also be about 1230/ 1225 BC.⁴⁰ These dates are realistic, but of course must not be pressed; the 'Year 4' on which they are based is most probably that of Merenptah, but not certainly so.

The main destruction of Canaanite Hazor (enclosure level 1a) would fall into the same period.⁴¹ Rowton's attempt⁴² to equate this fall of Canaanite Hazor (stratum XIII of the Tell; enclosure level 1a) with the campaign of Deborah and Barak in Judges 4 to 5 instead of that by Joshua in Joshua 11 (so pushing the Exodus and initial conquest back into the fourteenth or early thirteenth century BC) flies in the face of *all* other collateral evidence, and glosses over certain hints in Joshua I I and judges 4. It should be noted that in Joshua 11 all the emphasis is on Jabin I as king of *Hazor*, and on *Hazor* as 'formerly head of all those kingdoms', and it alone was burnt (Jos. 11:10, 13). It is therefore natural to associate this with the main end of Canaanite Hazor and its burnt remains. But in Judges 4, Jabin II is more often called king of *Canaan* (Jdg. 4:2, 23, 24 twice) than king of Hazor (Jdg. 4:2, 17), and

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his main strength is curiously not in Hazor but with Sisera in Harosheth. No emphasis whatever is placed on the city Hazor. This could simply mean that the later Jabin had a small fortified residence somewhere on (or near) Tell el-Qedah that has not yet been touched by the excavators, or else merely that he still ruled the state of Hazor but from a different town in the area.⁴³ (Only a small proportion of the Tell has been dug down to Canaanite levels; the examples of Gibeon and Arad should be a warning against too hasty an assumption that a further (but secondary) Canaanite occupation by Jabin II (or a residence nearby) is to be excluded.⁴⁴) The occurrence of two kings Jabin is, of course, no more of a doublet than two Niqmads (II and III) and two Ammistamrus (I and II) in Ugarit, two Suppiluliumas (I and II) and two Mursils (II and III) of the Hittites, and two Amenophis (III and IV), two Sethos (I and II) and two Ramesses (I and II) in Egypt - all in the fourteenth/thirteenth centuries BC.

⁴⁰ If 1304 BC be preferred for the accession of Ramesses II, then all dates in this section must be raised by about fourteen years (*cf.* above, p. 58, note 6).

⁴¹ Y. Yadin, *BA* 22 (1959), pp. 1-20, esp. 4-6, 13-15.

⁴² M. B. Rowton, *CAH*², 1:6 (*Chronology*), 1962, pp. 67-69; *cf.* F. H. Stubbings, *ibid.*, pp. 75-76, on difficulties of dating Mycenaean III pottery.

⁴³ *E.g.*, in Gn. 20:1, Abraham dwells between Kadesh and Shur, and sojourns 'in Gerar' - obviously, in the territory so named, not the walled city itself (probably Tell Abu Hureirah, Albright, *BASOR* 163 (1961), pp. 47-48 n. 59). City and state often have the same name in the Ancient Orient, although distinct entities. This applied to Carchemish in Hittite politics, for example, where city and land (same name) had distinct roles (*cf.* H. Klengel, *Geschichte Syriens im 2. Jahrtausend v.u.Z.*, I, 1965, pp. 41, 48 n. 54). Assur-uballit II, last king of 'Assyria', reigned in the West at Harran (outside his home territory) when Babylonians and Medes had destroyed the ancient capitals of Assyria proper, occupying the land (*cf.* D. J. Wiseman, *Chronicles of Chaldaean Kings*, 1956, pp. 17-19, 45, 61-63).

⁴⁴ J. Gray, *VT* 16 (1966), 26-52, compactly surveys the history and archaeology of Hazor (not using *Hazor III-IV*), but his treatment of the conquest marks no advance, while his sceptical view of Joshua's role rests on no tangible, objective basis.

Furthermore, the type of occupation found on several of these destroyed Canaanite sites is of a quite different and simpler kind, best explained as that of the occupying Israelites.⁴⁵ The biblical account cannot, therefore, be reduced to a peaceful, marginal infiltration by the Hebrews as required by Noth's

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arbitrary theories,⁴⁶ while the contrast commonly drawn between Judges 1 and Joshua 10 (usually to the grave disadvantage of the latter) is tenable only if one is content with a superficial view of the matter.⁴⁷

(d) Some False Trails

Some factors that have been supposed to have some bearing on the date or nature of the Exodus and conquest are actually irrelevant, and so can be eliminated.

I. *The Habiru or 'Apiru*. People so designated were a source of unrest in Canaan in the fourteenth century BC, as is shown by the Amarna tablets.⁴⁸ Their name is probably etymologically connected with that of the 'Hebrews', but it includes people scattered in place and time as far apart as Egypt, Anatolia and Mesopotamia from the eighteenth to twelfth centuries BC. There are too many differences in the data provided by Joshua/Judges and the Amarna tablets to identify the biblical and Amarna-period Hebrews. Thus, the Israelites were invaders from without and opposed the Canaanites, but the Habiru were native to Canaan and served under rival Canaanite princes who sometimes called *each other* 'Habiru' pejoratively.⁴⁹ In any

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⁴⁵ On this matter see G. E. Wright, *JBL* 60 (1941), pp. 27-42, esp. 30-33; *Biblical Archaeology*, 1957, pp. 81, and esp. 88-89; Albright, *op. cit.* in note 37, p. 66, above.

⁴⁶ Cf. M. Noth, *History of Israel*², 1960, pp. 68 top, 68-70.

⁴⁷ As is, for example, O. Eissfeldt, *CAH*², II:34 (*The Hebrew Kingdom*), 1965, pp. 4 (on Jos. 10 and Jdg. 1), 7, 9, 10-11, 12-13 (denial of Joshua commanding 'all Israel'), who seems to have learnt little or nothing from Wright, *JNES* 5 (1946), pp. 105-114. It must be remembered that Joshua's swift campaigns temporarily disabled a series of Canaanite city-states and were *not* (and not considered) an exhaustive conquest (despite Eissfeldt, *op. cit.*, pp. 10-11); when (Jos. 10) 'he left none remaining', common sense suggests that (like pedestrians on our roads) it is a question of 'the quick and the dead'; whoever had not got away perished. Eissfeldt has not allowed for Jos. 13 ff., where it is obvious that Joshua left very much land to be actually possessed, and not merely swept through or assigned by lot.

⁴⁸ Most of the data on the Habiru/'Apiru will be found in J. Bottéro (ed.), *Le Problème des Habiru*, 1954, and in M. Greenberg, *The Hap/biru*, 1955; for some later studies, see J.-R. Kupper, *Revue d'Assyriologie* 55 (1961), pp. 197-200, esp. 197 n. 2, and next note.

⁴⁹ So E. F. Campbell, *BA* 23 (1960), pp. 13-15 (esp. 15), and Wright, *Biblical Archaeology*, 1957, p. 75 (after Mendenhall); cf. earlier, E. Dhorme, *JPOS* 4 (1924), pp. 162-168 and Greenberg, *op. cit.*, pp. 70-76, 86-87. G. E. Mendenhall, *BA* 25 (1962), pp. 65-87, is interesting but in large measure dubious.

case, the larger number of South Palestinian city-states in Joshua as opposed to the Amarna tablets would indicate a later situation under Joshua than in the tablets.⁵⁰ The Amarna Habiru, therefore, have no direct bearing on the date of the Exodus or conquest (except indirectly to precede them) and so cannot support a date for these events in the fifteenth and fourteenth centuries BC as was once held. As has been said long ago, the Hebrews may have been Habiru - but not all Habiru are biblical Hebrews, nor can any particular group in the external data be yet identified as corresponding to the Hebrews.

2. *Asher in Palestine before the Exodus*. In Egyptian documents *Cf. c.* 1300-1250 BC,⁵¹ a place-name *I-s-r* in Palestine was identified by some with the biblical tribe of Asher, and it was then argued that this tribe was already in Palestine before the main Exodus took place: either they had a separate Exodus, or were never in Egypt.⁵² But recently, the proper Egyptian transcription of the name Asher has been recovered: it is *I-sh-r* (*i-š-r*) not *i-s-r* - so the references to *i-s-r* have nothing to do with the biblical Asher, and the theories based on this false equation must be abandoned.⁵³ The supposed references to Asher,

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Zebulon, etc., in the Ugaritic epics were proved to be non-existent long ago.⁵⁴

3. *More than one Exodus and Some Tribes Never in Egypt*. The uniform biblical tradition at all levels records that all of Jacob's sons entered Egypt (*e.g.*, Gn. 46:8-27; Ex. 1:1-5), and knows of only *one* Exodus by their descendants (*cf.* Ex. 24:4; Nu. 1, 2, 10:14 ff.). They were accompanied by a variety of heterogeneous elements (*cf.* Ex. 12:38; Nu. 11:4), and yet more were joined with Israel subsequently (*e.g.*, the Kenites, Nu. 10:29; Jdg. 1:16; 1 Sa. 27:10). There is not a scrap of clear, explicit evidence for more than one Exodus or for some tribes never going into Egypt. The supposed Egyptian and Ugaritic evidence is illusory (see previous paragraph). The events of Genesis

⁵⁰ Wright, *Biblical Archaeology*, 1957, pp. 75-76; note also the differences between the situation in Joshua and in the Amarna tablets mentioned by Wiseman, *NBD*, pp. 67-68 (Lachish and Gezer supporting Habiru, not destroyed by them; differing names of city-kings). The Joshua whom Meek compares with 'Joshua', without identifying him outright, in *Hebrew Origins*², 1960, pp. 21-22, is a lesser member of the Egyptian administration of Palestine (Amarna letter 256:18), not an invader. These differences invalidate Meek's general comparisons, *loc. cit.*

⁵¹ In topographical lists: J. Simons, *Handbook... of Egyptian Topographical Lists*, 1937, pp. 147 (XVII:4), 162 (XXV:8). In Papyrus Anastasi I: Gardiner, *Egyptian Hieratic Texts*, I, 1911, p. 25* note 12, and *Ancient Egyptian Onomastica*, I, 1947, pp. 191*-193* (No. 265); M. Burchardt, *Altkananäische Fremdworte und Eigennamen...*, 1911, No. 139.

⁵² For example, Rowley, *From Joseph to Joshua*, pp. 3, 33-35, etc.

⁵³ See Albright, *JAOS* 74 (1954), pp. 229-231 and n. 51, 232 and n. 58a (on Papyrus Brooklyn 35.1446 of eighteenth century BC). S. Yeivin, *Mélanges Bibliques Andri Robert*, 1957, pp. 98-9, preferring the old combination, dismissed Albright's view as hasty. But Yeivin has himself been too hasty. (i) Heb. Asher and Eg. 'i-sh-r' do not go with 'i-s-r' and the goddess Ashirat, because the latter has nothing to do with the root 'good fortune' but in Ugaritic is 'Athirat yammi', 'She who walks the Sea' or the like (*cf.* Albright, *Archaeology and the Religion of Israel*, 1953, pp. 77-8). (ii) Albright's *JAOS* equation is in line with the table *BASOR* 110 (1948), p. 15 n. 42, see remarks on Eg. equivalents before the table, and *cf.* table of F. M. Cross, *HTR* 55 (1962), p. 245 n. 95. Hence Albright's view must be retained.

⁵⁴ On this, see Albright, *BASOR* 63 (1936), pp. 27-32, and *ibid.* 71 (1938), pp. 35-40; R. de Langhe, *Les Textes de Ras Shamra-Ugarit...*, II, 1945, pp. 469-519.

34 belong explicitly to the time of Jacob, and have nothing to do with any later period (the Amarna age, for example; Labayu and his sons, not Hamor, ruled Shechem then).⁵⁵ The fact that Joshua and Judges do not record an Israelite conquest of the Shechem area of Palestine may show nothing more than the fact that our biblical records are not exhaustive sources for the period, and were never intended to be so read. One cannot (and should not) build theories on a void.⁵⁶ It should be evident from

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this whole chapter that assumptions of more than one Exodus, or of tribes not entering Egypt, or that the order of Moses and Joshua should be reversed⁵⁷ are wholly superfluous.

(e) Links with Other Periods

1. *With the Patriarchs*. As shown above (pp. 53-56), a four centuries' interval agrees very well with a date for the descent of the Patriarchs into Egypt about 1700 BC (round figure) and for the exodus of their descendants (and associates) in the early thirteenth century BC, each established on independent grounds.

2. *From the Exodus to Solomon*. Here, the evidence is rather more complicated. The primary evidence and biblical data used so far would indicate an interval of roughly 300 years from the Exodus to the early years of Solomon (c. 971/970 BC).⁵⁸ For the same interval, 1 Kings 6:1 gives 480 years, while addition of all the individual figures in the books from Exodus to 1 Kings gives a total of some 553 years plus three unknown amounts which will here be called 'x'.⁵⁹ Furthermore, David's genealogy of five generations in Ruth 4:18-22 can hardly easily extend over the 260 years or so between him and the Exodus, and so it is probably a selective one; but that of the priest Zadok (I Ch. 6:3-8) of ten generations would about cover the 300 years. The

⁵⁵ Both Rowley, *From Joseph to Joshua*, pp. 113-114, 124, etc., and Eissfeldt, *CAH*², II: 26a (*Palestine in the Nineteenth Dynasty...*), 1965, pp. 13, 24 with p. 8 (date of patriarchs) and *CAH*², 11:34 (*The Hebrew Kingdom*), 1965, pp. 7-8, put the patriarchs and the events of Gn. 34 in the Amarna age; as noted, Labayu, not Hamor, was at Shechem then, and as shown in §II of chapter 2, the patriarchs must be dated much earlier than the Amarna age.

⁵⁶ This is not to deny that some Hebrews could have left Egypt long before *the* Exodus; but if so, we have no explicit biblical record of such – the Patriarchs are people, not tribal personifications. The silence on central Palestine could reflect a rapid link-up with Hebrews already there, but of itself does not constitute direct evidence and so proves absolutely nothing except the incompleteness of the data. The assumption is possible but lacks proper evidence.

⁵⁷ As assumed by A. T. Olmstead, *History of Palestine and Syria*, 1931, pp. 197, 248; T. J. Meek, *Hebrew Origins*², 1950 (repr. 1960), pp. 43-46.

⁵⁸ The date 971/0-931/0 BC for Solomon's reign rests initially on Thiele's date for the beginning of the Divided Monarchy (E. R. Thiele, *Mysterious Numbers of the Hebrew Kings*, 1951, pp. 42-54); 961-922 BC is preferred by Albright and his associates (e.g., *BANE*, pp. 209-210). I prefer 971/0-931/0 BC for detailed Near Eastern and Egyptian reasons to be published in my *Hittite Hieroglyphs, Aramaeans and Hebrew Traditions* and *The Third Intermediate Period in Egypt* respectively (both forthcoming).

⁵⁹ Cf. the convenient table in Rowley, *From Joseph to Joshua*, pp. 87-88; he gives 554 + (etc.) years, where I have taken only three complete years of Solomon up to his fourth year.

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genealogies need be no problem; but what shall we make of the 480 and 553-plus-x years, as compared with the roughly 300 years' interval required by our primary evidence?

In principle, this problem is not quite so contradictory as it may appear, if we remember that the Old Testament is also a part of the Ancient Near East, and therefore that Ancient Oriental principles must be applied. Thus, in ordinary king lists and historical narratives, ancient scribes and writers did not usually include synchronistic tables and cross-references as we do today. Synchronisms were the subject of special and separate historiographic works.⁶⁰ In biblical terms, Judges as a narrative with a historico-religious purpose does not deal with synchronisms (except with oppressors as part of its story), while Kings is a synchronous history of Israel and Judah (while also a selective religious writing) in some degree comparable with the so-called 'synchronous histories' of Assyria and Babylonia.⁶¹ Here, an Egyptian example will be instructive as a parallel problem. For the five Dynasties Thirteen to Seventeen (the so-called Second Intermediate Period in Egyptian history), the Turin Papyrus of Kings⁶² records - or did when it was com-

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plete - some 170 kings who reigned at least 520 years altogether. Now we also know that they all belong inside the period 1786 to c. 1550 BC,⁶³ a maximum period of only about 240 years at most - a hopeless contradiction? No. We know, too, that these dynasties were all *partly contemporary*: the 520 or so years are genuine enough, but were partly concurrent, not all consecutive. This may prove equally true of some of the Judges in early Israel, so that the 553-plus-x years would then fit into the roughly 300 years, just like the 520 or so into the roughly 240 in Egypt. Now in the Ancient Orient, chroniclers and other writers often used excerpts from fuller records, and this might explain the 480 years - a total of selected figures (details now unknown) taken from the larger total. The various figures are therefore not so refractory in principle, when relevant principles are applied. To work this

⁶⁰ For example, the synchronous lists of Assyrian and Babylonian kings, especially Assur 14616c. See E. F. Weidner, *Die Könige von Assyrien*, 1921 (=MVÄG 26:2), pp. 10 f., and S. Smith, *Early History of Assyria*, 1928, pp. 349 ff. For Assur 14616c, cf. Weidner, *op. cit.*, pp. 15 f., and *AfO* 3 (1926), pp. 66 ff., and A. Poebel, *JNES* 2 (1943), pp. 60, 61. Cf. also next note. English version, *ANET*, pp. 272-274.

⁶¹ Cf. H. Winekler, *Altorientalische Forschungen*, I, 1893, pp. 297 ff.; F. Delitzsch, *Die Babylonische Chronik*, 1906, pp. 43 ff. 'Babylonian Chronicle' is given complete in R. W. Rogers, *Cuneiform Parallels to the Old Testament*², 1926, pp. 208-219; large extracts in English, also in *ANET*, pp. 301-307, plus D. J. Wiseman, *Chronicles of Chaldaean Kings*, 1956, and A. R. Millard, *Iraq* 26 (1964), pp. 14-35. The so-called 'Synchronous History' *par excellence* may just possibly be the preamble to a treaty (Oppenheim, *Ancient Mesopotamia*, 1964, p. 146, 284); W. G. Lambert regards this as a propaganda-text, not a treaty-preamble, *Bibliotheca Orientalis* 21 (1964), p. 182 end. No full translation is available, but snippets are given in P. van der Meer, *The Chronology of Ancient Western Asia and Egypt*², 1955, and H. Tadmor, *JNES* 17 (1958), p. 131 (further chronicle, *ibid.*, p. 134).

⁶² Text, Sir A. H. Gardiner, *The Royal Canon of Turin*, 1959; contents excerpted in Gardiner, *Egypt of the Pharaohs*, 1961, pp. 429-443 (Dyns. 13-17, pp. 440-443). On Egyptian lists, cf. also W. Helck, *Untersuchungen zu Manetho and den altägyptischen Königslisten*, 1956.

⁶³ For 1786 (end of Twelfth Dynasty), see R. A. Parker, *The Calendars of Ancient Egypt*, 1950, Excursus C; for about 1550 BC for start of the Eighteenth Dynasty, see E. Hornung, *Untersuchungen zur Chronologie and Geschichte des Neuen Reiches*, 1964, pp. 15-23, 108, and my review in *Chronique d'Égypte* 40/Fasc. 80 (1965), pp. 310-322, esp. 311.

out in practice within the book of Judges is not easy, simply because we need more detailed information on the period than is available there or from elsewhere. But neither is it beyond possibility (as is evident from an unpublished preliminary study). The problem of the book of Judges is chronologically rather less complicated than other celebrated problems of Near Eastern chronology - such as the Second Intermediate Period in Egypt, or the date of Hammurapi of Babylon, where a similar situation obtains.

Finally, in Judges 11:26, Jephthah (c. 1100 BC ??) speaks of Israel occupying Transjordan for 300 years before his time, *i.e.*, back to about 1400 BC if this is treated literally on modern reckoning, which does not fit a conquest at somewhere near 1240/1220 BC. But here again, *we do not know* the basis of Jephthah's figure - it could, again, be an aggregate of partly concurrent periods (*e.g.*, for Reuben, Gad and East Manasseh?), but we have no indications on which to build. Mesopotamian

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monarchs sometimes give long-range dates (like this 300, or 1 Kings 6:1, 480 years) which are invariably too long in absolute years, and probably represent some kind of aggregate; these are not yet understood despite apparently plentiful information. Empty speculation is profitless, and sound method would counsel one to await fresh light on matters of this type. No-one is compelled to produce a complete answer when there is simply not enough information to do so.

When treated positively, then, nearly all of the relevant data fits together reasonably well within the context of Ancient Near Eastern studies, considering its complex nature; more than this, no-one can demand in the current state of knowledge.⁶⁴

II. THE MONARCHY AND LATER

Here the problems are rather matters of small detail, than questions of wholesale divergences affecting centuries at a time.

(a) *The United Monarchy*

As something has happened to the Hebrew text of 1 Samuel 13:1, the length of Saul's reign can only be estimated.⁶⁵ But the round 'forty years' of Acts 13:21 must be quite near the truth. The

⁶⁴ As hinted already above, other Near Eastern chronological problems are just as intractable as anything in the Old Testament, but this does not inhibit Orientalists from seeking constructive solutions (real or provisional) covering all the data. For example, no one solution for the date of Hammurapi will satisfy *all* the astronomical data (*cf.* A. Parrot, *Archéologie Mésopotamienne*, II, 1953, pp. 428-429) as at present understood; the later long range Assyro-Babylonian figures do not fit any solution" (*ibid.*, pp. 363-365, 430-431), as already noted; and the Assyrian kinglists cause difficulties (*ibid.*, pp. 360-363). Other examples are plentiful.

⁶⁵ A figure may have fallen out of the text during the course of transmission. Mr. A. R. Millard reminds me of the omission of the year-date in Babyl. Chronicle B.M. 21901, 1. 66 (Wiseman, *Chronicles of Chaldaean Kings*, 1956, pp. 62/63, 82); and 'Babylonian Chronicle', col. 1:25, the reign of Tiglath-pileser III (R. W. Rogers, *Cuneiform Parallels to the Old Testament*², 1926, p. 209; *cf.* Grayson, *Bibbia e Oriente* 6 (1964.), p. 205).

biographical data available for Saul's fourth son, Ishbosheth (2 Sa. 2:10), implies that Saul was about sixty at death; he was anointed leader and king while still a 'young man' (1

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Sa. 9:2; 10:1, 17 ff.) and so he must have reigned thirty or forty years. The reigns of David and Solomon at forty years each need not be doubted; the first is thirty-three + seven years (1 Ki. 2:1 1), and Solomon was a younger son of David.⁶⁶

(b) The Divided Monarchy

For the 350 years from Rehoboam of Judah to the fall of Jerusalem in 587 or 586 BC, some ninety-five per cent of the long series of reigns and cross-datings in Kings and Chronicles have been brilliantly worked out by E. R. Thiele - and that not by arbitrary juggling but by full use of proper Ancient Near Eastern procedures, objectively documented.⁶⁷ At only two main points have difficulties persisted: the interpretation of certain data from the reigns of Ahaz and Hezekiah,⁶⁸ and certain dates linked with the capture and fall of Jerusalem in the period 609-587/6 BC.⁶⁹ New data and close study may well eliminate even these quite limited problems.

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One must remember that ancient methods of reckoning were not the same as ours. In the Ancient Near East two main kinds of regnal year are attested. By one method, a king reckoned the interval between his accession and the next New Year's Day as his accession-year (in effect, attributing that year to his deceased predecessor) and began his first regnal year with New Year's Day. This system was current in Mesopotamia. By the second method, a king counted the interval between his accession and New Year's Day as his first regnal year (in effect, attributing that whole year to his own reign), and began his second regnal year on New Year's Day. This was the Egyptian method. The official years of a king on the first system are his real total reign; but on the second

⁶⁶ See first work cited in note 68, below.

⁶⁷ E. R. Thiele, *Mysterious Numbers of the Hebrew Kings*, 1951 (2nd ed., 1965), following on *JNES* 3 (1944), pp. 137-186, and supplemented in *VT* 4 (1954) pp. 185-195, in E. C. Hobbs (ed.), *A Stubborn Faith* (FS W. A. Irwin), 1956, pp. 39-52, and in *AUSS* 1 (1963), pp. 121-138, *AUSS* 2 (1964), pp. 120-136. The rival scheme of Albright (*BASOR* 100 (1945), pp. 16-22; cf. *BANE*, pp. 208-213 and pp. 226-228, notes 20-50) is inferior in both its methods and results (cf. Thiele, *Mysterious Numbers...* 1951 ed., pp. 244-267, *A Stubborn Faith...*, pp. 39-41, and *AUSS* 1 (1963), pp. 132 ff.). The same may be said of V. Pavlovsky and E. Vogt, *Biblica* 45 (1964), 321-347, cf. 348-354, and Jepsen in A. Jepsen and R. Hanhart, *Untersuchungen zur Israelitisch Jüdischen Chronologie*, 1964.

⁶⁸ See commentary to Table II in my *Hittite Hieroglyphs, Aramaeans and Hebrew Traditions* (forthcoming). Recent discussions include those of H. Tadmor, *Scripta Hierosolymitana VIII* (*Studies in the Bible*), 1961, pp. 232-271; C. Schedl, *VT* 12 (1962), pp. 88-119; S. H. Horn, *AUSS* 2 (1964), pp. 40-52; and E. R. Thiele, *VT* 16 (1966), 83-103, 103-7.

⁶⁹ Recent discussions include: W. F. Albright, *BASOR* 143 (1956), pp. 28-33; E. R. Thiele, *ibid.*, pp. 22-27; H. Tadmor, *JNES* 15 (1956), pp. 226-230; J. P. Hyatt, *JBL* 75 (1956), pp. 277-284; D. N. Freedman, *BA* 19 (1956), pp. 50-60; E. Vogt, *VTS* IV, 1957, pp. 67-96; E. Kutsch, *ZAW* 71 (1959) pp. 270-274; E. Auerbach, *VT* 9 (1959), pp. 113-121; *VT* 10 (1960), pp. 69-70; *VT* 11 (1961), pp. 128-136; and C. Schedl, *ZAW* 74 (1962) pp. 209-213.

system, the official regnal years of a king will always be one higher than his real total (unless he died exactly at midnight on New Year's Eve, an unlikely event). Thus, if two kings were exactly contemporary but used different systems, their regnal years would always show a difference of one year at any given time.⁷⁰ Proper understanding and application of these methods is the main key to the detailed figures in Kings and Chronicles.

(c) Exile and Later

The main framework of chronology from 600 BC onwards is on the whole well fixed,⁷¹ and the only biblical question of note is the dispute over the relative order of Ezra and Nehemiah. Suffice it to say here that the biblical order is factually no more objectionable than the often advocated reverse order.⁷²

⁷⁰ When neighbouring states also begin their calendar years at different seasons (*e.g.*, in spring or autumn), or when civil and calendar years do not coincide (as in New Kingdom Egypt), then further complications enter into our attempts to unravel Ancient Near Eastern chronology, biblical or otherwise.

⁷¹ For Mesopotamia, *cf.* R. A. Parker and W. H. Dubberstein, *Babylonian Chronology, 626 BC-AD 75*, 1956, giving tables to turn Mesopotamian dates into Julian dates; for Egyptian dates in the Twenty-sixth Dynasty, *cf.* R. A. Parker, *Mitteilungen des Deutschen Archäologischen Instituts, Abteilung Kairo*, 15 (1957), pp. 208-212; and E. Hornung, *ZÄS* 92 (1965), pp. 38-39 (solar eclipse).

⁷² General survey (reversing order of Ezra and Nehemiah) is given by H. H. Rowley, 'The Chronological Order of Ezra and Nehemiah' in *The Servant of the Lord and Other Essays*, 1952, pp. 131-159. A modified 'reversal' view (owing much to Albright) with further references is presented by J. Bright, *Yehezkel Kaufmann Jubilee Volume*, 1960, pp. 70-87 [English part], and *A History of Israel*, 1960, pp. 375-386. For a critique of the Albright/ Bright date (428) for Ezra (but favouring 398), *cf.* J. A. Emerton, *JTS* 17 (1966), 1-19.

In favour of the biblical order, see J. S. Wright, *The Date of Ezra's Coming to Jerusalem*², 1958, and *The Building of the Second Temple*, 1958; J. Morgenstern, *JSS* 7 (1962), pp. 1-11; K. A. Kitchen, *TSF Bulletin* 29 (1961), pp. 18-19, reprinted as *Supplement to TSF Bulletin* 39 (1964), pp. vi-vii. On this age, *cf.* K. Galling, *Studien zur Geschichte Israels im Persischen Zeitalter*, 1964 (noting P. Lapp, *JBL* 84 (1965), pp. 297-300).

