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The Egyptian Middle Kingdom at Megiddo	<i>John A. Wilson</i>	225
The Origin of the Name "Hebrews" - -	<i>E. G. Kraeling</i>	237
The Date of the Byblos Temples	<i>Robert J. Braidwood</i>	254
Women and the State on the Eve of Islam	<i>Nabia Abbott</i>	259
<u>Persian and Egyptian Chronology</u> -	<i>Richard A. Parker</i>	285
An Aramaic Journal Page - -	<i>Raymond A. Bowman</i>	302
<u>Darius and Xerxes in Babylonia</u> - -	<i>George G. Cameron</i>	314
Two Inscriptions of Ashurnasirpal - - -	<i>Tom Jones</i>	326
Book Reviews - - - - -		327
<i>Walter C. Klein, Al-İbānah 'an Uşûl ad-Diyānah (L. V. Thomas)</i>		

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THE EGYPTIAN MIDDLE KINGDOM AT MEGIDDO

JOHN A. WILSON

An extreme simplification of the history of ancient Egypt might confine itself to action and counteraction in the play of forces between Egypt and its neighbors. Thus the late predynastic age showed strong Asiatic influences coming into the land of the Nile. Then the Old Kingdom exploited Sinai, Phoenicia, and perhaps Palestine economically. In the First Intermediate Period Asiatics "invaded" the Egyptian Delta. The Middle Kingdom moved again into Asia in some measure and with some authority. The Second Intermediate Period saw the Hyksos invasion of Egypt. The New Kingdom set up an Egyptian empire in Asia. The balance swung again with attempted invasions of Egypt in the thirteenth and twelfth centuries B.C., etc. The scheme of things becomes clearer in these later phases; the nature of the empire under the New Kingdom is fairly well known. We know less about the outreaching of the Old and Middle kingdoms into the areas beyond their normal frontiers. What was the nature of Egyptian "imperialism" under the Middle Kingdom?

The Middle Kingdom did not spring into being fully armored. It took time for the pharaohs of the Eleventh and Twelfth dynasties to establish their authority within Egypt. When that was accomplished they were ready to reach out toward regions beyond the frontiers. Sesostri III established his authority solidly at the Second Cataract, and trading-posts reached as far south as Kerma near the Third Cataract. Was there a similar situation in Syria-Palestine?

Oriental Institute A 18622 (Pls. I-III) is the lower portion of an Egyptian seated statuette, found by the excavations of the Oriental

Institute at Megiddo in Palestine. Gordon Loud, field director of the Megiddo Expedition, makes the following statement on the discovery of the statuette (Field No. A 1199) out of its normal context:

This and fragments of three uninscribed statuettes constitute an Egyptian group found obviously out of place in and about the Stratum VIIB phase (13th–12th century B.C.) of the eastern temple, which itself is contemporary with the Late Bronze Age palace. Three of these pieces, including the one under discussion, were incorporated into the rubble of which the temple platform was built. The fourth had reached an equally low status in the rubbish supporting the pavement just outside the temple door. In material they vary from the coarse, gray, granite-like stone of the one found outside the temple to a fine, black, polished stone, possibly diorite (e.g., see *Illustrated London News*, June 20, 1936, p. 1108, Fig. 2, where—as also in A. Rowe, *A Catalogue of Egyptian Scarabs . . . in the Palestine Archaeological Museum*, p. xlvii—the names on our statuette are incorrectly given on the basis of preliminary study).

Investigation has shown that the foundations of the temple originated not later than Stratum VIII (15th–14th century B.C.) and were so deep-set that they rested at one point no more than a meter above a standing wall of Stratum XV (20th–19th century B.C.), reused in XIV (19th century B.C.), to which level or levels we should attribute these statues. It is not improbable, therefore, that they were first encountered in the excavation for the foundations of the temple, and being of a useful building material were thus incorporated in the structure of the building.

The extant fragment of the statuette is approximately nine and a half inches high (24 cm. high by 17 cm. long by 13.5 cm. wide). The material is a hard, black stone of the diorite or basalt type. The individual depicted was represented as seated upon a chair, with his left palm upon his left knee and his right hand holding a kerchief the ends of which are visible against his right thigh. He wears a pleated skirt with a folded forepiece. Musculature visible on the left leg suggests a sculptor of ability.

Four columns of hieroglyphic inscription cover the left side of the base and four columns the right. The supporting column at the back of the statuette probably once ran up to the individual's head, with a single column of text. The inscriptions present conventional appeals for a certain Thuthotep, with little more than names and titles. The following translation must be provisional, as certain broken passages and certain titles puzzle me. Fortunately, the essentials of the inscriptions are clear. I am indebted to Dr. Keith C. Seele for collation, correction, and corroboration.

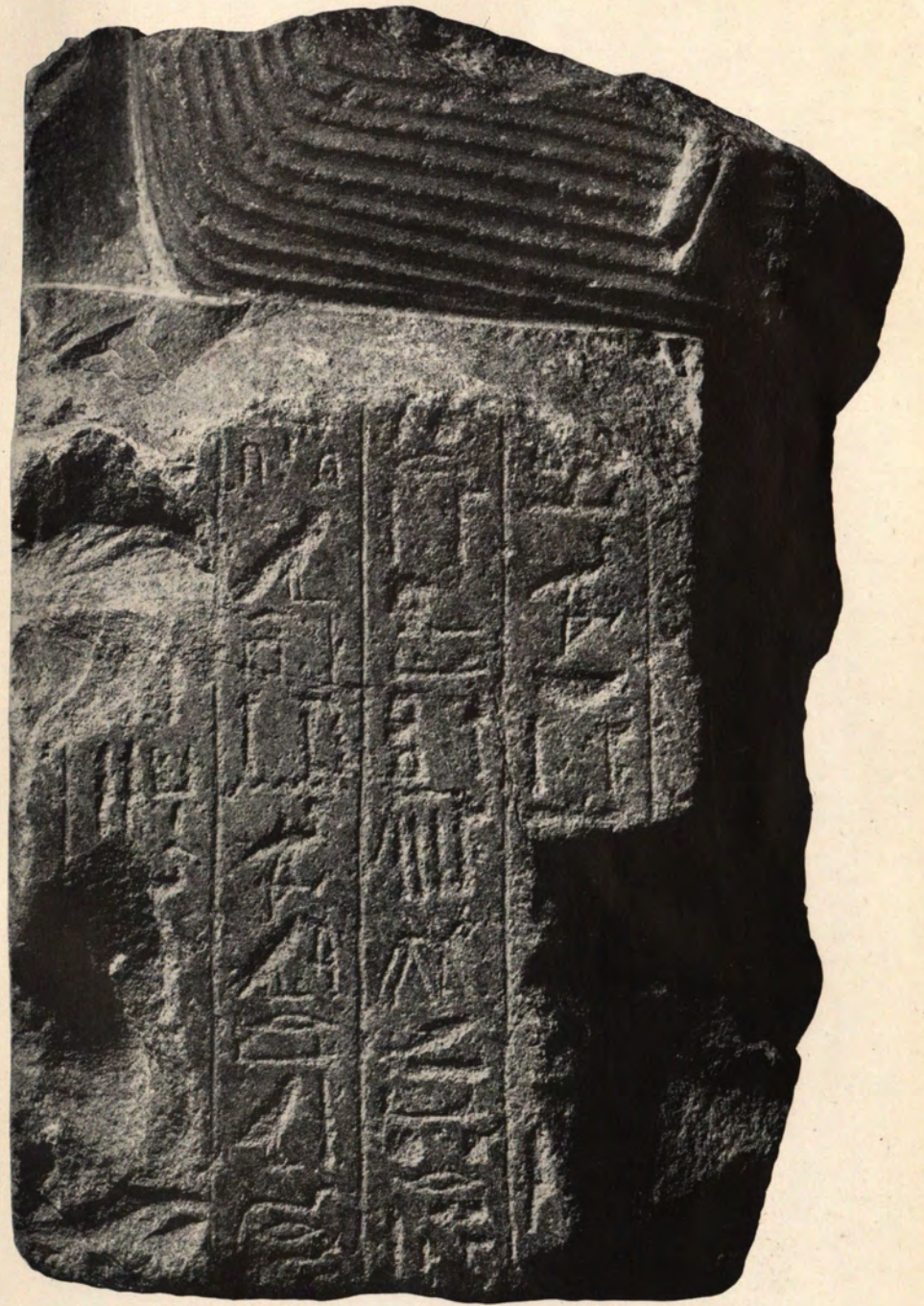
PLATE I



EGYPTIAN STATUETTE FOUND AT MEGIDDO, GENERAL VIEW AND REAR



EGYPTIAN STATUETTE FOUND AT MEGIDDO, LEFT SIDE



EGYPTIAN STATUETTE FOUND AT MEGIDDO, RIGHT SIDE

TRANSLATION

LEFT SIDE

(1) An offering which the king gives (to) Khnum, Lord of the Foreign-Country-of-the-God,^(a) that he may give an invocation-offering (consisting of) bread, beer, [cattle], fowl, etc.,^(b) to the *ka* of the 'revered one',^(c) (2) the Count, Controller of the Two Thrones, Overseer of Priests, Chief of Five,^(d) Royal Intimate, he who sees the mysteries [of]^(e) (3) the King and exalts the courtiers, the Great Overlord of [the Hare Nome], 'of the Royal Favorite^(f) (4) at the head of' , [Thut]hotep, born to Si[t-Kheper-ke].^(g)

RIGHT SIDE

(1) An offering which the king gives (to) Thoth, Lord of Divine Words,^(h)⁽ⁱ⁾ (2) 'the one revered in the presence of' the great god,⁽ⁱ⁾ the Count, Controller of the Two Thrones, Overseer of Priests, Magistrate and Administrator of Buto,^(k) 'Mouth of Hierakonpolis, priest^(l) (3) twenty in the palace',^(m) High Priest of Thoth, 'sem-priest',⁽ⁿ⁾ who has the text read,^(o) (4),^(p) Kay's son, 'Thuthotep',^(q)

REAR

. . . . 'in the House of Thoth,^(r) Great of,^(s) Ruler of^(t) in the House of Khnum, Kay's son, Thuthotep.

NOTES ON THE TRANSLATION

(a) "The-Foreign-Country-of-the-God" is unknown to me. We have here a sanctuary of the ram-god Khnum. The outline of the signs as they stand on the stone is beyond doubt. There is a question as to whether the land sign may be read *h3st* ("foreign country") or *zmyt* ("highland" or "desert"), but the implications would be essentially the same for the contrast of the upland foreign regions to the flat Egyptian plain. More important is the use of the city-sign determinative, involving the question whether this sanctuary can be in or out of Egypt.

On the face of it, the city sign should mean a town in Egypt, a hitherto unknown sanctuary of Khnum. That may be the safest hypothesis, but I do not wish to discard the possibility that this may be an early and hitherto unknown designation of Megiddo, with a local ram-god of pastoral Palestine assimilated to the Egyptian god Khnum.

In defense of the rare possibility that a non-Egyptian town might

be written with the city sign as determinative, I point to the Empire writing of Beth-Shan so determined, in "Mekal, Lord of Beth-Shan" (A. Rowe, *Topography and History of Beth-Shan*, Pl. 33), and to a Nineteenth Dynasty writing of the name Nebet-Kepen, "Lady of Byblos," with Byblos so determined (Turin 166, *Recueil de travaux*, IV, 140). It must be admitted that this determinative for Asiatic names is highly exceptional. The Empire permitted itself to determine the names of Nubian towns with the city sign, as in the case of Buhen (D. Randall-Maciver and C. L. Woolley, *Buhen*, Text, *passim*) and of Anibeh (G. Steindorff, *Aniba*, Text, *passim*). It may well be that cities which were assumed to be Egyptian, as belonging integrally to the Egyptian empire, might be so treated, especially when an Egyptian deity was lord or lady of the town. If so, Megiddo is here a possibility, and the question of the type of Egyptian control of the town comes into play.

A related problem, also not susceptible of solution, concerns the identity of the god in the name "Foreign-Country-of-the God." One thinks of the analogy of the Egyptian term *ḥ-ntr*, "Land of the God," which applied to regions east of Egypt, including Syria-Palestine, and in which the god was the sun-god (Ch. Kuentz, *Bulletin de l'Institut français d'archéologie orientale*, XVII, 178 ff.). But it is not necessary that the two names be psychologically related. The "god" may have been the ruling pharaoh. Further, Khnum in the Nineteenth Dynasty had a sanctuary at or near Elephantine called *ḥ.w-ntr*, "Lakes of the God," and on this analogy may himself be the god in question (A. Mariette, *Abydos*, Vol. I, Pl. 45, No. 29).

We emerge from the discussion without established results. We may have here a new name for Megiddo, indicating Egyptian control. It is safer to assume that we have a name for a sanctuary in Egypt, otherwise not yet attested.

(b) No certain trace of the ox's head of the word "cattle" could be isolated. Further, the oval sign under the goose's head may be abbreviated to the right of center. There is some abnormality in the usual formula. See the next note.

(c) The apparent spacing of the *ka*-sign to the right of center is inexplicable. The traces of *imḥ*, "revered," at the end of the line are probable but not certain.

(d) Thuthotep's titles "Controller of the Two Thrones, Overseer of

Priests, Chief of Five," as well as the "High Priest of Thoth" (right side, l. 3), are those of the High Priest of Thoth at Hermopolis (R. Anthes, *Zeitschrift für ägyptische Sprache*, LIX, 100 ff.; see also K. Sethe, *Amun und die acht Urgötter von Hermopolis*, §§ 73 ff.). The restoration of "the Hare Nome" in line 3 is certain from Thuthotep's tomb at el-Bersheh.

(e) About half a square is lacking at the end of the line. The translation is grounded on P. E. Newberry, *El Bersheh*, Vol. II, Pl. VII, pp. 23 f.: *mḥ sḥḥ . . . ny-sw, sds[r] ḥnywt*, where the writing fortunately brings the word *ḥnywt*, "courtiers," beyond question. Newberry's restoration of *pr*, "house," in the lacuna, resulting in the translation "he who sees the mysteries of [the House of] the King," seems to be an unsupported guess, although a reasonable one.

(f) A diligent search of Middle Kingdom titles has provided no resolution of the titles or epithets of Thuthotep at the end of this line and at the beginning of the next. A full square is lost at the end of line 3.

(g) The mother's name is known from Thuthotep's tomb. The *t* over the back of the duck cannot be established with certainty. The remainder of the line provides just room for the remainder of the name. For the writing of *ms*, "born," with horizontal *s*, see J. Polotsky, *Zu den Inschriften der 11. Dynastie*, page 21.

(h) Is this the earliest noted occurrence of the epithet of Thoth, *nb mdw-ntr*? A. Erman and H. Grapow, *Wörterbuch der ägyptische Sprache*, II, 181, gives it as beginning in the Eighteenth Dynasty.

(i) The presence of a reed leaf toward the end of the line makes restoration of the customary mortuary formula difficult here.

(j) At the beginning of the line the *imḥ* sign is probable but not certain. The *ḥ* sign in "great god" is certain.

(k) Note the abnormal writing of *Dp*, "Buto," with the *di* sign.

(l) The arrangement of signs and the visible traces at the end of the line are puzzling. If this be *rḥ Nḥn*, "Mouth of Hierakonpolis," why is the *Nḥn* sign to the right of center? The last group shows the top of a *ntr* sign, followed by a stroke (lower end visible), followed by a tall sign with rounded top. If this be *ḥm-ntr*, "priest," the writing is extraordinary.

(m) The "twenty" seems to stand isolated, with a possible, but improbable, horizontal trace faintly visible above it. In any case, "thirty" (for the title *wr mdw ḥm-w*, "Chief of Southern Tens") seems

difficult to introduce here. The initial sign of the assumed *izt*, "palace," is not certain.

(n) A clean-looking cut diagonally across a poorly cut *s* makes the reading *sm* uncertain. But what else could it be?

(o) This *rdi mdw drf*, later common as an epithet of Thoth, may be a characterization of humans in the Middle Kingdom (K. Sethe, *Historisch-biographische Urkunden des Mittleren Reiches*, I, 62:17; 18:19, occurring in the latter shortly before the title *sm*). For *d* used for *d*, cf. Polotsky, *op. cit.*, page 21.

(p) Unintelligible traces are visible near the top of the line and just above the *k3* of "Kay."

(q) The beak of the Thoth ibis and the right end of the *htp* sign may be discerned, chiefly because one knows where to look for them.

(r) Perhaps "[Chief of Five] in the House of Thoth." The peculiar writing with *m* last might be justified on the theory that the Thoth sign, carrying with it the house sign, had been given honorific precedence. But the uncertainty about the following titles makes any hypothesis difficult.

(s) The first of two unknown titles, which may be only one title. The striding bird looks most like the *gm* bird ("Great of Finding"? "Chief of those who Find the Two *hk3* Cords in the House of Khnum"?). But there are other possibilities for the bird, such as *snm*, "feeding"; *h3m*, "fishing-fowling"; or *d3r*, "red."

(t) On the face of it, *hk3 rk.wy*, "Ruler of the Two Cords." Could it possibly be "Ruler of the Last Two Days (of the Month)"? And one notes a late priestly title of Hierakonpolis and elsewhere, *hk3 rk* (Erman and Grapow, *op. cit.*, III, 172). This and the preceding title probably involve priestly functions in some sanctuary of Khnum.

GENERAL CONCLUSIONS

This, then, is the statuette of the Egyptian official and priest Thuthotep, the son of Kay and of a woman whose name begins Si. . . . From the names and titles there can be no doubt that this Egyptian was Thuthotep, nomarch of the Hare Nome, whose tomb at el-Bersheh is well known.¹ In his tomb we have the statement that he

¹ P. E. Newberry, *El Bersheh*, Vol. I: *The Tomb of Tehuti-hetep*; K. Sethe, *Historisch-biographische Urkunden des Mittleren Reiches*, I, 44 ff.; J. H. Breasted, *Ancient Records of Egypt*, Vol. I, §§ 688 ff. The tomb is famous for the scene showing the transportation of a colossus from the quarries. The Hare Nome was the fifteenth nome of Upper Egypt, with its chief city at Hermopolis Magna, across the river from el-Bersheh.

was a "royal child" under Amenemhet II (1938-1903 B.C., Breasted) and was still an active functionary under Sesostri III (1887-1849 B.C., Breasted). His father Kay invested him with the office of nomarch of the Hare Nome. His mother's name was Sit-Kheper-ke.

It is safe to take it for granted that this small piece actually belonged in Megiddo at some time during Thuthotep's life. But what was a statuette of Thuthotep, high priest of Thoth at Hermopolis and nomarch of a nome in Middle Egypt, doing in Megiddo? I assume that he was resident there in some capacity. It is improbable that he was a member of a merchant colony. His titles and the indications of his career under three successive pharaohs make it unlikely that he was an exile like Sinuhe. An ambassadorial post is a distinct possibility, although we do not know enough about Egyptian envoys at this time to affirm that a man of the rank and responsibilities of Thuthotep would be sent as ambassador to a town like Megiddo.

Other Middle Kingdom objects will be summarized below. Some pieces found in Asia carrying the names of Egyptian individuals may be argued to be those of merchants. But this statuette and that of Sesostri-enekh at Ras Shamra give me the impression of an Egypt which posted men of commanding authority abroad. If so, I must revise my ideas about the foreign relations of the Middle Kingdom. I have assumed that its Asiatic imperialism was commercial and cultural but not military and administrative. But the presence of Thuthotep at Megiddo, added to other accumulating evidence, makes me feel that I have been wrong. Perhaps the Middle Kingdom, in Asia as in Nubia, did extend its administrative control beyond the frontier of Egypt and thus by power held the main arteries of trade.

SOME MIDDLE KINGDOM CONTACTS WITH ASIA

The following notes are not intended to be exhaustive. I enumerate some of the more significant objects or blocks of material bearing on the problem of Egyptian relations with Syria-Palestine under the Middle Kingdom. I can draw no binding conclusions from this material; I do draw a strong presumption in favor of a type of Egyptian empire.

A. The biographical evidence on Thuthotep himself is slight, and his tomb provides no evidence that he resided outside of Egypt. An isolated title is *3 n h3st nb(t)*, "Door of Every Foreign Country,"

(Newberry, *op. cit.*, p. 16). I have not noted this title elsewhere. Does it connote frontier or customs or consular responsibility? Newberry's Plate XVIII provides A. M. Blackman with an interesting suggestion in *Journal of Egyptian Archaeology*, II, 13 f. In this scene "cattle of Retenu (Syria-Palestine)" are addressed with the words: "(Once) you trod sand, (but now) you walk on herbage." Understanding that the cattle have been moved from Asia, Blackman points out that this may be indirect evidence of Egyptian military campaigns into Syria-Palestine. On the basis of our statuette these Asiatic cattle may be given further consideration.²

B. A stela now in Manchester mentions Sesostris III's conquest of an Asiatic country named *Skmm* (Shechem?), "together with the wretched Retenu" (T. E. Peet, *The Stela of Sebek-khu*; Breasted, *op. cit.*, §§ 676 ff.). This may have been a raid, a conquest of territory, or a punitive expedition against rebels within the Egyptian domain. No other direct statement of Middle Kingdom military activity in Asia is known.³

C. The presence in Egypt of *3mw*, "Asiatics," as traders or as slaves need not indicate anything but peaceful relations with foreign lands. There is the famous scene of the arrival in Middle Egypt of thirty-seven Asiatics bringing eye paint (P. E. Newberry, *Beni Hasan*, Vol. I, Pls. XXX-XXXI). There are passing references to the transfer of Asiatics within Egypt as slave property (F. Ll. Griffith, *Hieratic Papyri from Kahun and Gurob*, 12:10-11; 13:15-17; 30:35) and to Asiatic dancers at Egyptian feasts (*ibid.*, 24:4-6, 13-14).

Equally inconclusive is such evidence as the Asiatic nature of some of the Tôd treasure of Amenemhet II ("Tôd [1934 à 1936]," *Fouilles de l'Institut français du Caire*, Vol. XVII, Pls. XV-XVII, pp. 113 ff.). Trade or royal gift is a sufficient answer, without assuming tribute.

D. On a wider scale there is a variety of evidence covering an extended period. There was the incursion of Asiatics into the Egyptian

² Corroborative evidence from a different tomb group of the Middle Kingdom may lie in the scene of cattle labeled as the "cattle of the Asiatics (*3mw*), brought from . . ." (or "brought as . . ."), discussed with caution by Blackman, *The Rock Tombs of Meir*, II, 18 n. In each case the cattle may have come to Egypt by trade rather than conquest.

³ But note the reported Twelfth Dynasty blocks at Karnak bearing names of Palestine tribute-bearers (R. M. Engberg, *The Hyksos Reconsidered*, p. 33, n. 38). In the same note, the twenty ships of cedarwood sent by Amenemhet I "to meet the Setetyu"-Asiatics rests on a dubious reading; K. Sethe, *Historisch-biographische Urkunden des Mittleren Reiches*, I, 12, reads the destination of these ships as probably "Upper Egypt."

Delta in the First Intermediate Period, and there is the construction of the "Walls of the Ruler" on the eastern frontier (probably a chain of fortresses), built by Amenemhet I "to ward off the *Sttyw*-Asiatics and to crush the Sand-Farers" (Sinuhe, R43). The Middle Kingdom execration texts, whatever their precise date, indicate a very real threat to the Egyptian throne, active relations with Asiatic towns, and a fairly detailed knowledge of the personnel of such towns.⁴

The Middle Kingdom relations with Sinai, for the exploitation of the mines there, are not conclusive evidence on the nature of Egyptian imperialism. For example, in one case Amenemhet III dispatched an expedition of 734 soldiers to the Sinai mines (Breasted, *op. cit.*, § 713). So large a force of soldiers was not necessary from a military standpoint to guard the mines against the Beduin. Rather the soldiers were used as the active Egyptian workers on the mines, just as Ramses IV later sent 5,000 soldiers to the Wadi Hammamat quarries (*ibid.*, Vol. IV, § 466). In other words, such a force does not set the effective Egyptian frontier in Sinai.

Further, no general consideration of the period would be complete without an understanding of the gradual crystallization of the Hyksos movement, beginning before the Twelfth Dynasty and culminating after the Middle Kingdom power had disintegrated.⁵

E. Egyptian scarabs and seals found in Palestine and Syria must be brought into the picture (see especially A. Rowe, *A Catalogue of Egyptian Scarabs . . . in the Palestine Archaeological Museum*). They may be treated in two classes: those containing the titles and names of Egyptians resident in Asia and those containing the titles and names of Asiatic princes. Thus there is the "Scribe of the Vizier, Senebef," found at Jericho (Rowe, S.5), and the "Guard of 110 Asiatics, User-

⁴ K. Sethe, *Die Achtung feindlicher Fürsten, Völker und Dinge, etc.* For more recently found documents cf. W. F. Albright, *Bulletin of the American Schools of Oriental Research*, No. 81 (1941), pp. 16 ff. On the basis of my own attempt to enter the hieratic signs of the *Achtungstexte* into Möller's *Hieratische Paläographie*, Vol. I, I am inclined to agree with W. F. Edgerton (*JAOS*, LX, 492, n. 44) that these texts "cannot be earlier than Sesostris III and are more probably to be placed in the Second Intermediate Period." Since their date cannot be fixed with precision, they may be used for this argument only in a general way: Asiatic princes ruled Asiatic towns, but the contacts with Egypt were arguably more direct than those permitted by mere trade and diplomatic relations.

⁵ In Engberg, *op. cit.*, and in Albright's studies in *Journal of the Palestine Oriental Society*, VIII, 223 ff.; XV, 194 ff., there are useful treatments of Egyptian-Asiatic relations in the periods in question.

khepesh," of which the Palestine provenience is unknown (Rowe, No. 15—but is it actually of the Twelfth Dynasty?). From Syria come the "Lady of the House, Sit-User" (*Syria*, VIII, 85 ff.) and the "Hereditary Prince and Count, Impi" (*ibid.*). These arguably had temporary posts in Asia. On the other hand, the scarabs of the "Counts of Byblos" carry the Asiatic names ²*Intn*, ²*Ibšmw*, and *Ypšmwib* (*Syria*, X, 12 ff.; *Kêmi*, I, 90 ff.; *Journal of Egyptian Archaeology*, XIV, 109; XIX, 54). These Asiatics ruled Byblos as princes. But the repeated title *h3ty-c*, conventionally translated "Count," is of importance. In Egypt this title was conferred upon an individual by the king. (Cf. Breasted, *op. cit.*, Vol. I, § 385, or cf. Hepzefi's inability to transmit property from his count's estate in *ibid.*, § 551.) In other words, even though we do not know that the same situation applied outside of Egypt, this title presents the argument that the Asiatic rulers of Byblos were confirmed in their rule by the king of Egypt, implying a measure of Egyptian control.⁶

To carry on the evidence, at the end of Rowe's catalogue there is a summary graph of Egyptian objects found in Palestine. Ignoring difficulties and taking the evidence in its sweep, this graph provides the following rough ratio of materials: Middle Kingdom, 3; Second Intermediate Period, 7; New Kingdom, 10; and post-New Kingdom, 3. Such a ratio does not argue for an Egyptian empire in the Middle Kingdom of the same intensity as in the New Kingdom. On the other hand, it does indicate a moderately active beginning for the spread of Egyptian influence in Asia on the basis of physical evidence.⁷

F. We come next to objects of Egyptian sculpture bearing hieroglyphic inscriptions and found on Asiatic soil. Inscriptions of the

⁶ W. F. Edgerton objects that I overstate my case. The title "Count" was conferred by Egyptian kings, when those kings were strong enough. Perhaps such strength did not reach Byblos under the Middle Kingdom. Edgerton points out that the use of the uraeus to ornament a Byblite scimitar and of the cartouche (turned backward) around a princely name do not suggest that these princes were "loyal," even though they may have been friendly. Such words of caution are necessary in our present knowledge of these times. However, such considerations do not modify my argument, which is not a sweeping claim of an aggressive imperialism.

⁷ The materials listed above are not all of the Twelfth Dynasty; some of them belong to the period following that dynasty. In this summary survey I forbear to list uninscribed materials such as pottery or such scarabs as do not contain clear names. It would be instructive to deal with such evidence. See, e.g., Table V in P. L. O. Guy and R. M. Engberg, *Megiddo Tombs*, pp. 190 f., for a survey of foreign relations as indicated by the material found in the tombs at Megiddo.

"Butler Heka-ib" and of the "Citizen Dedu-Amon" were found at Gezer (R. A. S. Macalister, *The Excavation of Gezer*, II, 311 ff.). A sphinx of Amenemhet IV was found at Beirut (*British Museum Quarterly*, II, 87 ff.; *Syria*, IX, 300). A sphinx of the princess Ita, daughter of Amenemhet II, was found at el-Mishrifeh = Qatna (*Syria*, IX, 10 f.). From Ras Shamra come a sphinx of Amenemhet III (*Syria*, Vol. XIV, Pl. XV opp. p. 120), a statuette base of Khnumit-neferhedjet, the wife of Sesostri III (*Syria*, Vol. XIII, Pl. XIV, p. 20), and the statuette group of the vizier Sesostri-enekh (*Syria*, Vol. XV, Pl. XIV opp. p. 116, and pp. 131 ff.). The latter is a most important document. Commenting on the phrase found on this monument, "[to whom was given] the gold of honor," Breasted points out that this was a reward for distinguished service abroad.⁸ The vizier of Egypt might reside in an important foreign town as an ambassador or as a governor. Perhaps these terms draw too much from modern situations, and we might think of Sesostri-enekh as an Egyptian high commissioner, keeping a watchful eye on a Syrian state which was nominally independent but subject to Egyptian "protection."

There is no need to linger over objects bearing the names of Amenemhet III and IV from the tombs of the princes of Byblos (P. Montet, *Byblos et l'Égypte*, Text, pp. 155 ff.). These were royal gifts to friendly or loyal princes, a remark which applies also to the sphinxes listed above.

On a different footing are two statuettes found in Anatolia, that of the "Nurse Sit-Snefru," found at Adana (*Bulletin of the Metropolitan Museum of Art*, XVI, 208 ff.), and that of an untitled Kerey, found east of Ankara (*AJSL*, XLIII, 294 ff.). At such a distance we can hardly think in terms of firm imperial control. Sit-Snefru may have been an Egyptian governess engaged by some Anatolian prince, and Kerey may have been a merchant. Their presence so far from the land of the Nile is a measure of the range of Egyptian cultural influence under the Middle Kingdom.

On the fragment of an Egyptian ivory wand found at Megiddo the inscription invokes magical protection "over the Lady of the House

⁸ *Syria*, XVI, 318 ff. I cannot follow Breasted in his restoration "who satisfies the king as his deputy [in Khajru and in the royal cabinet]." This would involve a strange spacing and lack of determinative for *h3rw*, "Syria."

'B³twmw', by night during the night and by day during the day" (*Illustrated London News*, November 25, 1939, p. 795). This was found in Stratum VIII (fifteenth-fourteenth centuries B.C.) but in such a context that it may have been carried over from earlier levels. Such magic wands may be of the Eighteenth Dynasty but are most common in the Middle Kingdom. The assumed 3 sign in the lady's name is highly doubtful, but the name seems to be non-Egyptian in character.

G. Finally, we turn to the story of Sinuhe, a political refugee, who fled from the sphere of Egyptian control at the death of Amenemhet I. The geography of his flight and sojourn in Asia is not clear, but he went as far north as Byblos on the Phoenician coast and then apparently cut to the east, where he was received by a prince of Upper (mountainous) Retenu, in a land of fruits, vines, grains, and cattle. Although he lived near a route which saw the passage of travelers to and from Egypt, he was out of the reach of Egyptian police or legal power. Such a region as the Buqaa, with the great road running north and south between the Lebanon and the Anti-Lebanon, might serve the purposes of the story.

In this case we might suggest that the Egyptian effective control applied to Palestine and to Phoenicia with greater force than to the hinterland of Syria. Or we may have an early Twelfth Dynasty situation, before the later pharaohs had been able to set up a tighter control of Asia. I incline to the latter as a working theory.

No clear-cut conclusions have been won. However, the delegation of the vizier Sesostri-enekh to residence in Ugarit and of the high priest of Hermopolis Thuthotep to residence in Megiddo is significant. This was something more than cultural and commercial empire. If men of such standing were sent to posts in Asia, there must have been a measure of administrative and military empire. On the basis of present evidence a working theory would be that Egypt in the nineteenth century B.C. confirmed the rule of local princes but held them in control by resident high commissioners, possibly backed by garrison troops. Perhaps Egypt of the nineteenth century A.D. provides an analogue for Egypt's Asiatic empire in the nineteenth century B.C.

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THE ORIGIN OF THE NAME "HEBREWS"

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Since the discovery of the Amarna letters much ink has been spilled over the Ḫa-bi-ru and their relationship to the Hebrews.¹ Unfortunately, the problem of who the former were has become more complicated all the while. Since the discoveries of Chiera it has become increasingly clear that the word is not ethnic but rather has something to do with the sphere of social legislation.² The vexing problem of the correct transliteration of the cuneiform word Ḫa-bi-ru³ as Ḫabiru or Ḫapiru, and the identity of the Semitic root (whether ḪḪḪ, ḪḪ, עבר, עבר, עפר, עפר) seems now to have been decided by the Ras Shamra texts. Virolleaud announced on June 30, 1939, that these texts proved that the transliteration should be Ḫa-pi-ru.⁵ In a letter to the present writer⁶ he quoted the passage on which this claim was based:

alu Ḫal-bi amelūti^{SAG-GAZ} = Ḫlb ḫprm

It associates the Syrian city of Ḫalbi with the SAG.GAZ people (usually written SA.GAZ, another name for the Ḫa-bi-ru) and transcribes this ideogram into Ugaritic ḫprm (עפרים), thus showing that the root is עפר. In a letter to J. W. Jack⁷ several other references to Ḫalbi are

¹ The most circumspect of the numerous discussions of the problem prior to the discovery of the Nuzi materials was that of Böhl, "Kanaanäer und Hebräer," *Beiträge zur Wissenschaft vom Alten Testament*, Vol. IX (1911). It is still worth reading, as are the basic remarks in Knudtzon, *Die El Amarna Tafeln* (1915), pp. 46 f.

² Chiera, "Habiru and Hebrews," *AJSL*, XLIX (1932/33), 115 f. To this must be added Speiser, "Ethnic Movements in the Near East in the Second Millennium B.C.," *AASOR*, XIII (1933), 13 f., and Lewy, "Habiru and Hebrews," *Hebrew Union College Annual*, XIV (1939), 587 f., where the new source materials are subjected to further amplification and examination. In addition, cf. *ibid.*, XV (1940), 47 f. A good résumé is given by Meek, *Hebrew Origins* (1936), pp. 6 f., where additional literature will be found cited.

³ The sign *bi* stands for both *bi* and *pi*; the special sign for *pi* alone occurs only by exception in the Amarna letters. Cf. Böhl, *Die Sprache der Amarnabriefe* (1909), p. 9a n. My spelling Ḫa-bi-ru thus emphasizes the ambiguity of the middle syllable.

⁴ This was the etymology adopted in my *Aram and Israel* (1918), p. 34. It was recently favored also by Dhorme, *Revue biblique*, 1924, pp. 12 f., and, with changed interpretation, in *Revue de l'histoire des religions*, 1938, pp. 170 f.

⁵ Académie des Inscriptions et des Belles-Lettres, *Comptes rendus*, May-June, 1939 p. 329.

⁶ Kraeling, "New Light from Ugarit on the Habiri," *BASOR*, LXXVII (February 1940), 32 f.

⁷ Jack, "New Light on the Habiru-Hebrew Question," *PEQ*, July, 1940, p. 97.

cited, and Goetze⁸ draws the conclusion that they all refer to certain parts of the city—the last *Hlb prm* being the quarter where the *Hapiru*, a distinct class of the population, were living.

The question has, however, been raised whether the reading carries the weight that Virolleaud (and myself in the article aforementioned) have attributed to it when we considered it as proving that the connection with the name "Hebrews" was impossible. Albright expressed doubts in a postscript to my article and toned down its final sentence so that, as Rowley⁹ observes, it ends on a less certain note than that on which I began it; and Jack, too, expresses doubts in quoting his letter from Virolleaud. It is to be noted that Jack approaches the subject with the firm conviction that there is a relation between the Hebrews, on the one hand, and the *Ha-BI-ru*, on the other, so that it is for him only a question of explaining the divergent Ugaritic reading *prm*. He makes much of the confusion or interchange of *b* and *p* in the languages, dialects, and scripts of the ancient Near East; a somewhat similar line is taken by Rowley, who, however, is more open minded with respect to the general problem at issue. Their approach here is rather "external," and many of these examples prove little or are themselves uncertain. Of decisive importance, of course, would be the claim that the Ras Shamra texts also spell *prm* as *brm* if that were true.¹⁰ This rests on Virolleaud's communication, some years ago, of the following passage from an unpublished text:

k ksp l brm zt
hrš l brm kš

"Like silver is the olive tree to the *brm*
Gold is the *kš* (-tree) to the *brm*."

But Virolleaud's earlier identification of the *brm* with the *Ha-BI-ru* (and the Hebrews) in this passage was hasty.¹¹ There is no reason here to expect the name of a people or a social group. The only natural rendering is "wanderers," "passers-by" (cf. Ps. 129:8, etc.), for one can readily see how a fruit tree would be very desirable to wanderers.

⁸ Goetze, "The City Khalbi and the Khapiru People," *BASOR*, LXXIX (October, 1940), 32 f.

⁹ Rowley, "Ras Shamra and the Habiru Question," *PEQ*, July, 1940, pp. 90 f.

¹⁰ *Ibid.*, p. 93, n. 5; Jack, *op. cit.*, p. 100.

¹¹ Virolleaud, "La Mort de Baal," *Syria*, XV (1934), 317; *La Légende de Keret* (1936), p. 74.

whereas there is no reason why it should be more precious to the "Hebrews" than to anybody else. In view of the agreement of Ugaritic, Egyptian (Eperu), and doubtless also the Assyro-Babylonian that the consonant is *p*; in view, furthermore, of the fact that Ugaritic often preserves words more closely corresponding to the primitive Semitic form than Hebrew does, it would seem difficult to defend Jack's hypothesis that the Ugaritic form with *p* is secondary. The shakiness of his position is even further revealed by his introduction of a secondary line of argument. He suggests that the Ras Shamra scribe may have been an Egyptian or have stood under Egyptian influence because he observes a widespread Egyptian inclination to write *p* for *b*. But the rule in Egyptian is just the opposite,¹² and the exceptions he lists are few and, owing to the possibility of errors of hearing on the part of Egyptian scribes, do not prove much. In the case of the Eperu (people who were present in Egypt itself)¹³ we may assume that the writing is accurate and does not reflect an error of hearing. At all events it would seem that Jack's conclusion that "in view of the fresh light from the Ras Sharma texts there seems to be decided proof at last that the *Habiru* were Hebrews" is without foundation.

Far more valid than Jack's approach is that of Albright, already mentioned.¹⁴ Here there is no trifling with the fact that *Hapiru* is primary. For him the new reading vindicates a suspicion that he had entertained previously that "*Habiru*" or "Hebrew" derives from an earlier *Hapiru*, because stop sounds may become voiced in the presence of *r* (or, as Goetze suggests, only in immediate contact with *r*). Of course, if it is now to be accepted that the word is to be read *Hapiru* wherever *Ha-BI-ru* occurs in the Amarna texts and elsewhere, the change from *p* to *b* would have taken place in the period when South Canaanitic vocalism was modified, leading from *Apiru* > *Ipr*. But, while such a change is a possibility, it can certainly not be said to be a necessity, since it did not occur, for example, in the name of the Midianite clan Epher < *Ipr* < *Apir*, long since (e.g., Böhl) considered as a possible equivalent of *Ha-BI-ru* (if it was to be read *Hapiru*), or in words like

¹² Erman, *Ägyptische Grammatik* (3d ed., 1911), p. 102.

¹³ Wilson, "The Eperu of the Egyptian Inscriptions," *AJSL*, XLIX (1932/33), 275 f.

¹⁴ *BASOR*, LXXVII, 32 f. Cf. *Archaeology of Palestine and the Bible* (1932), pp. 206 f.

sepher and *kopher*. If this is true, then the assumption of such a change remains a highly precarious theory. The burden of proof for the actuality of the occurrence of the transition in this word lies on those who advocate the identification of Ḥapiru and Hebrews; the purely theoretical possibility that such a change might have taken place can carry only slight weight over against the solid fact that the two forms must be considered different until proven identical.

Here a point made by Rowley¹⁵ might well claim attention. He asserts that "the view that the Ḥabiru or Ḥapiru of the Amarna letters represent one of the waves of Hebrew or Israelite immigrants does not rest merely on this [philological] equation, nor would it fall if the equation were definitely ruled out." The only other thing beside etymology that can underlie the conviction of a relationship between the Hebrews and the Ḥa-bi-ru is the assumption that the latter are foreign invaders who are conquering the country after the manner of the Israelitic tribes of the Book of Joshua. But that assumption is itself very doubtful in the light of the new information concerning the Ḥa-bi-ru.¹⁶ It seems to us far more likely that we are concerned with social revolution within the land than with invasion from without. Such revolutions took place repeatedly in Egypt and elsewhere. Why not in Palestine? If we find the statement in Amarna 67:17 that "Aziru is an escaped dog like the SA.GAZ" this seems to us to suggest quite clearly that the SA.GAZ and Ḥa-bi-ru in the Amarna period are runaway or emancipated servants (many of them doubtless men who were in military service) who band together and seize control of local towns and communities.¹⁷

But whether this view is correct or not, our intention in presenting it is merely to show that the other main prop on which the identification of Ḥa-bi-ru and Hebrews rests involves an "assumption" that itself now requires further proof. It is not our intention here to enter into that Ḥa-bi-ru problem in its full range. It is a problem in the oriental history of the second millennium B.C. and will have to be settled in the light of inscriptional and archeological data. The Israelitic sources cannot be used as "historical" source material of the first

¹⁵ *Op. cit.*, p. 93.

¹⁶ Lewy (*op. cit.*, XIV [1939], 615) says "no passage in the Amarna letters shows that they are nomadic invaders."

¹⁷ Cf. now Lewy, *ibid.*, pp. 616 f.

rank in dealing with such a problem, any more than Herodotus carries the weight for Assyro-Babylonian history that a cuneiform text carries. The problem of the name "Hebrews" is, in our opinion (as will be set forth below), a problem of the early part of the *first* millennium and for the present at least, or until more inscriptions having bearing on this age come to light, will have to be investigated as a biblical problem. When the proper limitations are not observed, only confusion results, and the entire Ḥa-bi-ru—Hebrew discussion has been tinged with that sort of thing. In the following pages we shall investigate the name "Hebrews" exclusively on the basis of the Old Testament. This is the more necessary since the only recent study, that of Parzen,¹⁸ reaches what we believe to be erroneous conclusions. We are particularly concerned with one point which everybody seems to take for granted, viz., that the name "Hebrews" is old and was applied to the Israelites from the very beginning of their history. This is an assumption which certainly ought to be tested.

The chief proof for the supposition that the name is ancient is its use in the narratives concerning Israel's beginnings. It is rather naïve, however, to suppose that this usage accurately reflects that of the times concerning which these narratives speak. In the light of our present insight, the nation called Israel crystallized from various groups which came to unite or were united as a result of forces or circumstances that need not be further discussed in this place. All evidence points to the fact that "Israel" was the common name for this union of tribes from the very beginning. There is no evidence and very little likelihood that the separate groups previously had a common name for themselves or were designated by such a name, except this usage of sources many centuries younger than the events they purport to relate. The only actual fact at our command is this—that documents dating from the period of the divided monarchy, presumably the ninth and eighth centuries, employ the term "Hebrew" or "Hebrews" in a few connections as a designation for an Israelite or the Israelites. What occasions surprise is not so much the existence of an alternate name as the spotty nature of its occurrence. We must make clear to ourselves that this name plays no discernible role in the historical life of the people. Nowhere in Joshua, Judges, II Samuel,

¹⁸ Parzen, "The Problem of the Ibrim in the Bible," *AJSL*, XLIX (1933), 258 f.

I or II Kings, or in the great stream of prophetic literature (cf. below on the seeming exception in Jeremiah) is this name employed. Its actual use is restricted to a few clusters of passages (where it occurs repeatedly) and to several isolated instances. A little reflection will show that under these circumstances the term cannot be regarded as very deeply rooted in the national tradition.

We must first of all glance at the tradition itself to see if we can find any principle or set of principles governing the choice of the word where it is used. The first "cluster" of passages is that found in the Joseph stories, which dwell on Egyptian-Israelite relations. Joseph is referred to by the Egyptians as a "Hebrew man," "Hebrew slave," "Hebrew boy, slave of N. N." (Gen. 39:14, 17; 41:12). Joseph speaks of Palestine as the "land of the Hebrews" (40:15), and the narrator himself contrasts Egyptians and Hebrews on a point of custom (43:32). The second "cluster" of passages is found in the Moses cycle, which also dwells on Egyptian-Israelite relations. We hear of "Hebrew midwives" and of "Hebrew women," who in one case are contrasted with Egyptian women (1:16, 17, 19; 2:7), and of Hebrews in general (2:6). In all these instances the word is used either by Egyptians in speaking to or about Israelites or by Israelites in speaking of themselves to Egyptians. This is true also in the case of all the occurrences of the plural masculine except two. In 2:6 we hear of Hebrews in the mouth of the Egyptians and in 3:18, 5:3, 7:16, and 9:3 of Hebrews in the mouth of Israelites. But it is to be noted in 2:11 and 13 that the narrator himself uses the word in speaking of his own people.

The third "cluster" of occurrences is composed of passages which are found in I Samuel. They are concerned with Philistine-Hebrew relations. We hear the Philistines speaking of the Israelites as Hebrews in 4:6 and 9, 13:19, 14:11, and 29:3. In 13:3 Saul is supposed to have said, "Let the Hebrews hear," but we share the opinion of many critics that this verse must be rearranged and that it originally spoke of the Philistines hearing that the Hebrews had rebelled (cf. the commentaries and Kittel's *Biblia Hebraica*), so that the word was used by the narrator. In 13:7 the word "Hebrews" must probably be gotten rid of entirely; the subject of the verb "and they crossed over" (emend to impf. cons.), is still the men of Israel in verse 6. In

14:21, finally, we have the word used in the mouth of the narrator. Thus the same mixed situation occurs here as in the Joseph and Moses cycles.

There is also a fourth group of occurrences, which, though not in proximity, form a cluster from a material point of view—Exod. 21:2, Deut. 15:12, and Jer. 34:9 and 14. It is obvious that Jer. 34:9 and 14 are closely related to Deut. 15:12 and do not have any independent value. But Deut. 15:12 is merely a restatement of the law of Exod. 21:2 for a different age. Except as attestations of the continued existence of the word "Hebrews," the secondary passages are without significance for the purposes of this inquiry. Exod. 21:2 is their fountainhead and need alone concern us.

Finally, there are the two isolated occurrences in Gen. 14:13 and Jonah 1:9. The former would, of course, be of great importance if the document were an ancient one. Since we cannot accept that view and see no strong arguments lending support to it, we feel that the reference to "Abram the Hebrew" carries no historical weight. The Book of Jonah being admittedly late, its allusion to the hero as a Hebrew does not lend much light to the subject.¹⁹

The first question awakened in the mind as one scans this material is this: Is the word "Hebrew" an ethnic term?

Under the influence of the Ḥa-bi-ru -Hebrew equation and in the light of the new materials given to the world by Chiera it became clear in recent years that Ḥa-bi-ru could not be ethnic; therefore, there has been a tendency to read this result also into the Old Testament use of "Hebrew." The strongest argument in this direction was put forward by Alt, who claims that in Exod. 21:2 *ʿIbrī* signifies a debt slave (cf. below).²⁰ But we doubt the probability of that exegesis, for would not *ʿebed* be unnecessary and tautological if *ʿIbrī* per se meant debt slave? Surely the idea would not have occurred to Alt if it had not been for the texts from Nuzi. Meek, because of the same

¹⁹ We will accept as correct the statement of Parzen that the name is not used extensively in post-biblical sources, the exceptions being Judith and II Maccabees. In the former we have mere imitation of biblical usage; in the latter, Alexandrian atmosphere, where the term "Jew" aroused hatred and contempt, so that "Hebrew" was artificially revived. It is interesting to note that Paul calls himself a Hebrew with obvious pride (II Cor. 11:22; Phil. 3:5).

²⁰ Alt, *Ursprünge des israelitischen Rechts* (1934), pp. 19 f. Cf. also Gordon, *Biblical Archaeologist*, III (1940), 12.

general line of thought, would even deny the ethnic use in Gen. 14:13 and finds justification for this in the fact that the LXX renders *περάτης*.²¹ But, as Noth rightly says, this LXX translation proves nothing except the understandable need felt by the Greek translator to make clear what was thought to be the significance of this designation at the first place in which it occurs.²² Since *ʿIbrī* is nowhere used as a word for a transient or nomad, we have scant grounds to regard it as an appellative of that meaning in Gen. 14:13.²³ If the nonethnic rendering cannot be defended in these two places, it is impossible to prove it anywhere else. Curiously enough, some of the scholars who start out with the claim that "Hebrew" is originally nonethnic of necessity end up by theorizing as to how it became ethnic in time (e.g., Speiser and Lewy). Noth, however, alluding to the inconsistency of such a procedure, frankly admits²⁴ that, if *ʿIbrī* really were an ethnic designation, one would have to separate *Ḥa-bi-ru* and *Eperu* from it entirely.

We must now ask: Why did the writers who employ the term "Hebrew" choose to use it when they did? Since *ʿIbrī* pl. *ʿIbrīm* < *ʿIbriyyīm* is a gentilic form,²⁵ Landsberger has suggested that it serves as a gentilic substitute for "Israelite."²⁶ For some reason not quite clear a *nisbe* of "Israel" is found only in II Sam. 17:25 where, however, it is an error for "Ishmaelite" (cf. I Chron. 2:17) and in the late passage Lev. 24:10 f. (cf. also the N.T. John 1:47, etc.). In our older texts, Alt asserts "Israelite" is always expressed by the phrase

²¹ *Op. cit.*, p. 7.

²² Noth, "Erwägungen zur Hebräerfrage," *Festschrift Otto Procksch* (1934), p. 107.

²³ The LXX translator seems to have etymologized the term as "wanderer." The word appears again only in Philo i. 349. Parzen suggests that the translator probably read *hā-ʿōbēr* in Genesis, chap. 14, but that can hardly be taken seriously.

²⁴ *Op. cit.*, p. 101. Noth's theory is that *Ḥa-bi-ru*-Hebrew is a self-designation of the nomads who have entered a settled region and tent there without property rights. But this falls with the *Ḥa-bi-ru* equation and the disproof of their nomadism.

²⁵ This fact by itself should have discouraged comparison with the word *Ḥa-bi-ru*. The Nuzi fem. *Ḥa-pi-ra-tū* makes it especially clear, for the *nisbe* would be *ḥapiraitu* > *ḥapiritu*. The *nisbe* of the masculine *Ḥapiraiu*, as pointed out by Knudtzon (*op. cit.*, p. 47) occurs only in two isolated instances of the Cassite period, and there with the writing *b/pir* instead of the usual one. On these passages see Weidner, *Archiv für Orientforschung*, X (1937), 2 f.; Landsberger, *Archiv für Orientforschung*, X (1937), 140 f. Such isolated occurrence scarcely warrants the weight given it by Meek, *op. cit.*, p. 13. Böhl's attempt to link these instances with a "Ḥapirtu" connected with Elam (*Kanaaner . . .*, p. 86) is no longer possible; the name is *Ḥa-tam-tu*. Cf. Poebel, *AJSL*, XLVIII (1931), 20 f.

²⁶ Landsberger, "Habiru und Lu-la-hu," *Kleinasiatische Forschungen*, I (1929), 329.

"Man of Israel" (Judg. 7:14, Num. 25:8 and 14; cf. also "a man from the house of Israel," Lev. 17:3, etc.).²⁷ Since *nisbe*'s are formed from similar names like Ishmael and Jerachmeel, one wonders why this situation should exist. Apparently foreigners did hesitate to form one, for Shalmaneser speaks of Ahab as "Sir'lai," using a *nisbe* form. One suspects editorial elimination of this form in the Old Testament literature for some reason that can only be guessed. But this much is clear—that, if "Hebrew" had actually served as gentilic for "Israel," we should find it used more consistently and not merely in several clusters of passages. It appears necessary, then, to seek for a different explanation.

It seems to us that where the name is employed it is an alternate name for Israel and is used purposely in place of the latter name. A certain nuance of feeling which the true national name conveyed to those who bore it was to be ruled out; or, conversely, a different nuance of feeling was to be expressed when the word "Hebrew" was introduced. It is wrong to go as far as Parzen does when he asserts that there was something derogatory in the term "Hebrew." But it is a fact that the term is chosen (when chosen at all) in situations where the Israelite is not a free citizen in a free community or on free soil. If one will assume for the moment that, in contrast to the rather exalted term "Israelite," "Hebrew" was more objective, one can readily understand why the Israelites in the house of slavery, Egypt, would be spoken of as Hebrews and, above all, why an Israelite man who became a slave would preferably be called a "Hebrew slave" in Exod. 21:2. This would also fit I Sam. 14:21, where the writer differentiates "the Hebrews who belonged to the Philistines" as vassals from "the people of Israel who were with Saul and Jonathan." We think it totally unnecessary to separate these Hebrews tribally from the Israelites, as is so often done. That the Philistines in all the other passages are made to speak or think of "Hebrews" rather than of "Israel" may well be due to the fact that the story-tellers consider them as subjects—now in a state of rebellion, it is true—but theoretically Philistine dependents. Even in Genesis, chapter 14, Abraham is a so-

²⁷ *Op. cit.*, p. 21. One wonders, however, whether the phrase does not have a slightly different nuance than the use of the gentilic would convey—referring not so much to derivation as to present membership in the national religious community?

journer who pays tribute to Melchizedek, and in Jonah the prophet is a helpless man, away from his land and his people.

Having found at least a provisory explanation for the use of the name where it is used, we must now attempt to approach the matter of its origin. Was it a designation which outsiders actually used of Israel or is it a self-designation? The Egyptians had their own terms for the Asiatics of Palestine, and if one rejects the link with the Eperu-Ḥapiru, Hebrew is not found among them. "Israel" is directly mentioned in the stele of Merneptah. Mesha of Moab speaks of Israel, evidently meaning the northern kingdom, and also refers to one tribe of the Israelites as "men of Gad" (גַּד אֲנָשִׁים). This suggests that the Philistines, too, were wont to use the national name or the names of specific tribes with whom they came into contact. It seems to us, therefore, that "Hebrew" is purely a self-designation. But while this conclusion, if correct, relieves us of the expectation to find the "Hebrews" referred to in foreign inscriptions, it still leaves the origin of the term in obscurity.

To account for the name, it might be well to take into consideration the possibility that it is the name of an early group which was absorbed by the Israelites. We today speak of Britons and Anglo-Saxons as though they were alternate names, whereas historically they represent totally different peoples. Might not the alternate name "Hebrews" also be the real name of an earlier stock? The possibility cannot be ruled out, but it seems strange that there is no mention of a tribe of "Hebrews" in our earliest documents, even though they reflect such matters as the existence of a tribe of Makir and the early importance and subsequent decline of such tribes as Reuben and Simeon. It would seem, therefore, that there was no actual Israelite group with the tribal name of Hebrews involved in the early settlement of Canaan.

Another possibility seems more attractive, viz., that we here have a designation that came into vogue some time in the early history of the monarchy as a result of Israelite self-orientation in the world in which it had become a power. The question who are we and who are our next of kin must inevitably arise in youthful peoples that come to have a national consciousness. One answer that, as we shall see, impressed itself enough to gain widespread acceptance at a certain time

was this: we are *ʿIbrīm*. That per se has a touch of racialism in it and implies separation from certain near-by elements and a linking-up with other more remote elements. For the antithesis to the near-by elements (Canaanites, Philistines) is a vital part of the national consciousness.

At this juncture one might well argue that "Hebrews" was the name of a distant group, contemporary or well remembered from former days, like the Aramaean *Aḥlamē* of whom we hear so much in the Assyrian inscriptions. Tradition (or speculation) asserted: we went forth from them. At a much later time, one might continue, it was no longer so clear who the *ʿIbrīm* were, and the deuteronomic passage (26:5) gives a modernized answer to the same question of origin and kinship when it says "a perishing Aramaean was my father."

It can hardly be doubted that both Eber > *ʿIbr* and *ʿIbrīm* are possible tribal names. J. J. Hess has adduced some close parallels to both when he points out²⁸ that it is said in the Tāj el ʿArus, "And *al ʿIbr* is a tribe that stretches along the Euphrates to the Syro-Arabian desert, and which Aṣ-Ṣaḡānī mentions, and *Banu ʿl ʿIbr* is (also) a tribe but another than the aforementioned." He also cites from E. C. Ross's list of the Beduin tribes of Oman one that is identical with *ʿIbrī = ʿIbrīm*: "Name of tribe, adjective form: *ʿIbrī*. Collective plural form *el ʿIbrī-īn*" and says that "tradition of Oman asserts that these tribes came from the west, and may once have dwelt along the Euphrates." The latter point would seem to imply reflection on the etymology of the name, as having something to do with the shore of a river. It should not be overlooked, however, in the case of the names *al ʿIbr* and *Banu ʿl ʿIbr* that the use of the article betrays consciousness of the appellative character of the name, whereas it is not clear that biblical Eber was anything more than a name.

But, unfortunately, we are unable to prove the existence of a contemporary tribe of Eber or *ʿIbrīm*: hence it seems advisable to look for a different solution. Now the explanation of national relationships in genealogical form was an especially easy and vivid way of dealing with the need of self-orientation in the world. Not only the Hebrews but even the Greeks followed much the same plan.²⁹ Various construc-

²⁸ J. J. Hess, "Beduinisches zum Alten und Neuen Testament," *Zeitschrift für die alttestamentliche Wissenschaft*, XXXIII (1915), 120 f.

²⁹ Cf. Meyer, *Geschichte des Altertums*, III (2d ed., 1937), 290.

tions arose at different times. The theory that there was an ancestor Eber may be one of these.

Our Book of Genesis gives an interesting glimpse of this rivalry of theories which resulted eventually in harmonistic compromises. Thus in the early poetic lines of Gen. 9:25-27 Shem stands as Israel's ancestor and representative, with no thought of larger horizons; the outlook is purely Palestinian—Shem (=Israel), Canaan, and Japheth (Philistines, Thekel, etc.) are three groups descended from the aboriginal vintager Noah. In the later Jahwistic stratum where the vintager Noah has been identified with the Babylonian flood hero of the Gilgamesh epic, Shem is no longer identified with Israel but moves up into a more remote position as ancestor of a group of peoples. Now in J's system we find Eber referred to in a manner showing that he has been demoted: "To Shem, moreover, the father of all the sons of Eber was born (offspring)" (10:21). The artificiality of this statement is patent; the father of all the sons of Eber can only be Eber. Shem has crowded him into the background owing to the larger panorama the author-editor desired to obtain by connecting up with the (newly elevated) flood hero and his sons. P in his re-working of J's system makes Eber the son of Arpachshad whom J had put in the group Elam, Ashur, Arpachshad, Lud, and Aram, and provides for a direct line of descent leading from Shem through Arpachshad and Eber to the ancestors of Israel. But perhaps Shem and Eber were not the only rival ancestors. In his mention of Peleg, J says that under him "all the earth was divided" (vs. 25); unless it is a secondary element suggested by the desire of punning on the name, this statement alludes to a system in which Peleg was the father of sons representing various groups of men. It is not clear, however, who the (two?) sons of Peleg were, though we can infer from P (11:18) that one was Reu. Peleg himself has now been demoted to the rank of one of two sons of Eber, the other being Joktan. In P's final system we hear of only one son of Eber, Peleg, and the latter is the father of Reu, from whom the line leads via Serug and Nahor to Terah. But another observation must be made. The system that put Eber at the head and made the sons of Eber ancestral to a line leading down to Israel, on the one hand, and to certain Arab tribes as far down as Hadramaut, on the other hand, is basically similar or even parallel to that which makes Abra-

ham father of both Isaac as ancestor of Israel and Ishmael as ancestor of an Arab group, or the other which makes Isaac father of both Jacob, rival ancestor of Israel, and Esau, ancestor of the Edomites. We thus get a glimpse of the fact that historical speculation, availing itself of the genealogical principle, attempted to establish a considerable variety of systems which eventually resulted in certain compromises.

The possibility that the name "Eber" is secondarily personalized from a geographical name is one that now needs to be investigated. This is particularly recommended by the fact that so many other geographical names are found in genealogy. In J's list of Shem's sons we doubtless have geographical names, though Arpachshad is still obscure.³⁰ P makes Eber the son of this Arpachshad and the progenitor of the line Reu, Serug, Nahor, Terah. Of these names, Serug and Nahor are certainly personalized geographical names.³¹ I once explained Terah as derived from the city name Til-ša-Turaḥi, likewise in the vicinity of Harran, and still think that, owing to the geographical "environment," this is not entirely ruled out;³² but the discovery of a personage named Trḥ in an obviously lunar role in the Ras Shamra texts³³ and the vocalization of the biblical Terah like *jérah*, "month," as well as the appropriateness of the idea that the moon-god should live at Harran (Gen. 11:32) makes it more likely that the name is to be explained from that quarter.³⁴ An exception, however, only helps to prove the rule. In view of the large use made in this list of personalized geographical names, and also in view of the fact that the most natural etymology strongly suggests some connection with the other shore of a river,³⁵ we have very good cause to look for the origin of the ancestor Eber in a geographical term of some sort.

³⁰ This name is particularly vexing. I expect to return to it in another connection. Meanwhile cf. my *Aram and Israel*, p. 18.

³¹ Cf. Schrader, *Keilinschriften und das Alte Testament*², pp. 477 f.; my *Aram and Israel*, pp. 18, 24.

³² ZAW, XL (1922), 153 f.

³³ Dussaud, *Les convertes de Ras Shamra et l'ancien testament* (1937), p. 81, n. 4, and p. 102, n. 3.

³⁴ The "lunar" interpretation of the figure Terah, hinted at by Jeremias, *Das Alte Testament im Lichte des Alten Orient*², p. 259, thus has received unexpected confirmation.

³⁵ Another possible etymology is "one who has passed over." Cf. Aescoly, "Falasha-Ibrim," *AJSL*, LI (1935), 127 f., for a parallel. But in that case one would have to assume the name to be a designation given by others and not a self-designation. The analogy quoted above from the tribes of *al 'Ibr*, etc., points more in the direction of connection with the shore of the river. Cf. my *Aram and Israel*, pp. 31 f.

A clue to such a geographical term seems to be given to us by the passage from the Balaam oracles referring to Eber (Num. 24:24). Now it is quite true, as Dillmann first pointed out, that Num. 24:20-24 do not seem integral in the connection,³⁶ but this does not prove that the words are necessarily a very late fabrication; it merely suggests that they are of separate origin. It is difficult to see what interest late times could have in Amalek or Cain or an otherwise unknown Eber. The oracle predicts the end of Amalek, considered a highly important nation, and the deportation of Cain by Ashur in spite of its impregnable position on a high rocky plateau, and then closes with the words

And ships (will go forth) from the side of the Kittaeans
And will humble Ashur and will humble Eber
And moreover it (?) perishes for aye.

Mention of the Kittaeans has suggested to some scholars that this is an interpolation from the Greek period because "the land of Chittim" in I Macc. 1:1 is Greece, whence Alexander went forth.³⁷ Meyer rejects that idea entirely because of the presence of the verses in the Samaritan Pentateuch.³⁸ I am not sure that this particular argument is valid, for it might conceivably be shown that the Samaritanus was re-edited in the light of the text set up as authoritative by the Jews. But allusion to the isles of the Kittaeans in Jer. 2:10 certainly shows that the term could be used in pre-Exilic times. Furthermore, I can see no connection between Num. 24:24 and the history of Alexander; the latter merely used his fleet to ferry his army across the Dardanelles and then marched overland to his wars with the Persians, for whom Ashur and Eber would have to be a sort of cryptic designation. Num. 24:24 presupposes a situation in which ships come, apparently via Cyprus (to which the name Kittaeans, derived from Kition, primarily belongs), and land troops that attack Ashur and Eber. Dan. 11:30 sees the prophecy fulfilled in the coming of the Romans at the time of Antiochus Epiphanes, but that very fact seems to us to prove that Num. 24:24 is much older. We must reject as unthinkable the idea that it is so late an interpolation.

³⁶ Cf., in general, Mowinkel, "Der Ursprung der Bilaamsage," *ZAW*, XLVIII (1930), 233 f. Eissfeldt, "Die Komposition der Bilaamerzählung," *ZAW*, LVII (1939), 212 f.

³⁷ Cf. Baentsch, *Handkommentar: Exodus-Leviticus-Numeri*, ad. loc.

³⁸ Meyer, *Die Israeliten und Ihre Nachbarstämme* (1906), p. 321.

Before proceeding further a word must be said about the method of interpreting this prophecy. Dare we use in this instance the principle of the post-eventum prediction? Meyer doubts the necessity of always historically interpreting words of a soothsayer, especially words that may be intentionally obscure.³⁹ He asserts that some of these words will never be satisfactorily interpreted. But since the post-eventum principle regularly gives us the key elsewhere in this type of prophetic passage, one is reluctant to back down in this instance without trying to make use of it. If one employs it, it would seem to necessitate our assuming (1) that the Assyrians carried off the Kenites; (2) that "Eber" and Ashur were humbled by troops landed by ships from the west. We would then have allusions to historical events not otherwise known. The matter leaves the historian somewhat helpless. Why should the Assyrians have troubled themselves to deport the Kenites and when could they have done it? How was it possible for ships from Kition to humble Ashur and Eber? The classical philologist, Dornseiff, takes the phrase "from the side of Kition" literally and says this means ships of Kition, the Phoenician city on Cyprus.⁴⁰ He interprets this with the help of a list of the sea rulers from the fall of Troy to 480 B.C., which a historian of the time of Caesar, Castor of Rhodes, put together and which reports a sea rule of the Cyprians 880-846 B.C. But unfortunately this period coincides with an era of great Assyrian power under Ashurnasirpal and Shalmaneser, so that an actual humiliation of Ashur by invaders from the sea seems out of the question. Furthermore, the great importance of Tyre at this period precludes a Cyprian sea rule. Meyer is doubtless right in dismissing the list of Castor of Rhodes as a worthless fabrication.⁴¹

Under the circumstances, then, it seems as though we have actual prophecy in Num. 24:24 spoken at a time when Assyria had become known as a factor of history for the West and when Amalekites and Kenites were still enjoying an independent existence and seemed formidable to Israel.⁴² The information that we have concerning both of these tribes is rather meager, but we may hazard the guess on the basis

³⁹ *Ibid.*

⁴⁰ Dornseiff, "Antikes zum Alten Testament," *ZAW*, LV (1937), 135.

⁴¹ Meyer, *Geschichte des Altertums*, II², Part II (1931), 62.

⁴² Cf. Meyer, *Israeliten*, p. 392. He, too, considers the passage ancient.

of what we know that the situation fits the early period of David's rule, prior to his subjection of Edom. The name "Eber" can hardly have referred to Israel, as Dornseiff assumes. Its proximity to Ashur in the prophecy and its place in the genealogies of Genesis, chapter 10, all point to Mesopotamia. It was apparently used at a certain time to describe the ancient Mitanni—territory which included the region that lay "across" the river (Euphrates)—the district around Harran to which Jahwistic tradition points as the home of Abram.

The establishing of a connection of "Hebrew" with a geographical "Eber" is ancient. It was already suggested by Aquila in his revised rendering of LXX in Genesis, chapter 14, as *περάτης*, a word used by Josephus *BJ* ii. 20. 4 for "one of the country across the water," i.e., Peraea. The narrow Palestinian viewpoint of the Roman period is not to be confused, however, with the broader international horizon of the Israelite era. In those days *hannahar*, "the river," was not the Jordan but the Euphrates. It is not likely that Eber had anything to do with Transjordan in Num. 24:24 or in the personalized ancestor of Genesis, chapter 10, for special district names prevailed for that area; a man from thence was a Gileadite, etc. Connection with the river Euphrates seems the only reasonable one, if a geographical link is to be sought.

Jensen recently raised the question of the connection of Eber with the term Eber-hannahar that we meet in Ezra 4:10 as the name for the province west of the Euphrates.⁴³ This is obviously a formulation from the viewpoint of the Assyrians or Babylonians (and Persians); for them Syria is Transeuphratensian. Jensen asks whether it is possible that the term "Hebrew" could have come up among the Jews in the Babylonian exile? We frankly think that it is impossible to make our references so late. But it is quite possible that prior to the rise of a province of that name, the people living in Syria or Palestine should have described the territory lying east of the Euphrates (from their point of view) as Eber-hannahar, and then abridged that term further to "Eber." That they used the phrase "other side of the river" in thinking about that region is obvious (Josh. 24:2, etc.), though we cannot prove that a name Eber-hannahar had an official

⁴³ Jensen, "Alttestamentlich Keilinschriftliches," *ZAW*, LII (1934), 123 f. Cf. also my *Aram and Israel*, p. 31.

status for that territory. But we think that Num. 24:24 shows that Eber was a familiar geographical term for it during a certain period—no doubt the very time when the vogue came up to describe the Israelites as "Hebrews." After that geographical term ceased to be used (presumably owing to the definite absorption of the territory by the Aramaeans which led to the use of the designation *Aram Naharaim* in Gen. 24:10 etc.), Eber became merely the name of an ancestor, as in Genesis, chapter 10.⁴⁴

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⁴⁴ A word may be added here on the supplementary article of Lewy, "A New Parallel between Habiru and Hebrews," *HUCA*, XV (1940), 47 f. In his note on the Ugaritic spelling of the word he takes in effect the same line as Rowley and Jack. He seeks to bolster the reading *Ḥa-bi-ru* by referring to the god *Ḥa-bi-ru* at Ashur (Schroeder, *Keilschrifttexte aus Assur verschiedenen Inhalts*, Pl. 37, col. II, l. 9; cf. Gustavs *ZAW*, XL [1922], 313 f.). But the spelling may be traditional and the god imported (cf. Albright, *BASOR*, LXXXI [1941], 20). It is also possible, however, that the name of the god is of an entirely different derivation. Even the god Amurru is not necessarily to be associated with the Amorites (cf. *Reallexikon für Assyriologie*, I, 99). And in the case of the *Ḥa-bi-ru* we are dealing with a social class rather than with an ethnic group, making it even more difficult to believe that it could lend its name to a divinity! If Lewy says that I overlook the fact that the discussion of the *Ḥa-bi-ru*-Hebrew problem no longer centers about a chiefly linguistic question but rather on the Nuzi analogies, he will find that the above article takes the basis from underneath his own discussion. Proof of the survival of Hurrian or any other line of law or practice in the Palestine of the first millennium is interesting and valuable but can show nothing as to the origin of the Israelites.

THE DATE OF THE BYBLOS TEMPLES

BUILDINGS II, XVIII, AND XL¹

ROBERT J. BRAIDWOOD

In his recent *Fouilles de Byblos* (Paris, 1939), Volume I, Maurice Dunand publishes three buildings of monumental character and the evidence for his interpretation of their dates. The buildings in question were set on sloping ground, and their excavation, in view of the differential levels involved, made for exceedingly complicated architectural digging. The success which M. Dunand achieved in the excavation and presentation of these buildings deserves only the highest praise. The matter in hand is merely concerned with his interpretation of the evidence for dating these buildings. Were it not for the specific invitation made to the reader of his volume to make original interpretations, I should hesitate to present mine publicly, for I am firmly convinced that no opinion can bear so much weight as that of the excavator himself.

Fully realizing the huge responsibility with which the excavations of Byblos charged him, M. Dunand dug entirely by a system of 0.20 m. *levées*, each *levée* peeled off one after the other, and each kept "*rigoureuseusement horizontale*." The system has both advantages and disadvantages. It is only pertinent here that objects are generally noted by *levée* rather than by relation to a specific occupational floor. The plan presented here (Fig. 1) was made by assembling on one sheet the tracings of the individual buildings published in the *Fouilles de Byblos*, Volume I. The section was constructed on the basis of the assembled plan and with the aid of various statements in the text (pp. 290-308).

The buildings and their dates as published by M. Dunand are:

1a. Building II (*dernier état du corps principal*)—Middle Kingdom, on the basis of the Twelfth Dynasty materials found in the jars of foundation offerings, underneath pavements of the building.

1b. Building II (*premier état*)—Old Kingdom to as early as the offerings of Khasekhemui (pp. 298, 304).²

¹ I am obliged to George R. Hughes and Richard A. Parker for checking the pertinent Egyptian inscriptions, and to Harold D. Hill for the finished drawing.

² Since the Khasekhemui inscription was found on the surface, the dating is by implication only and need not apply specifically to Building II.

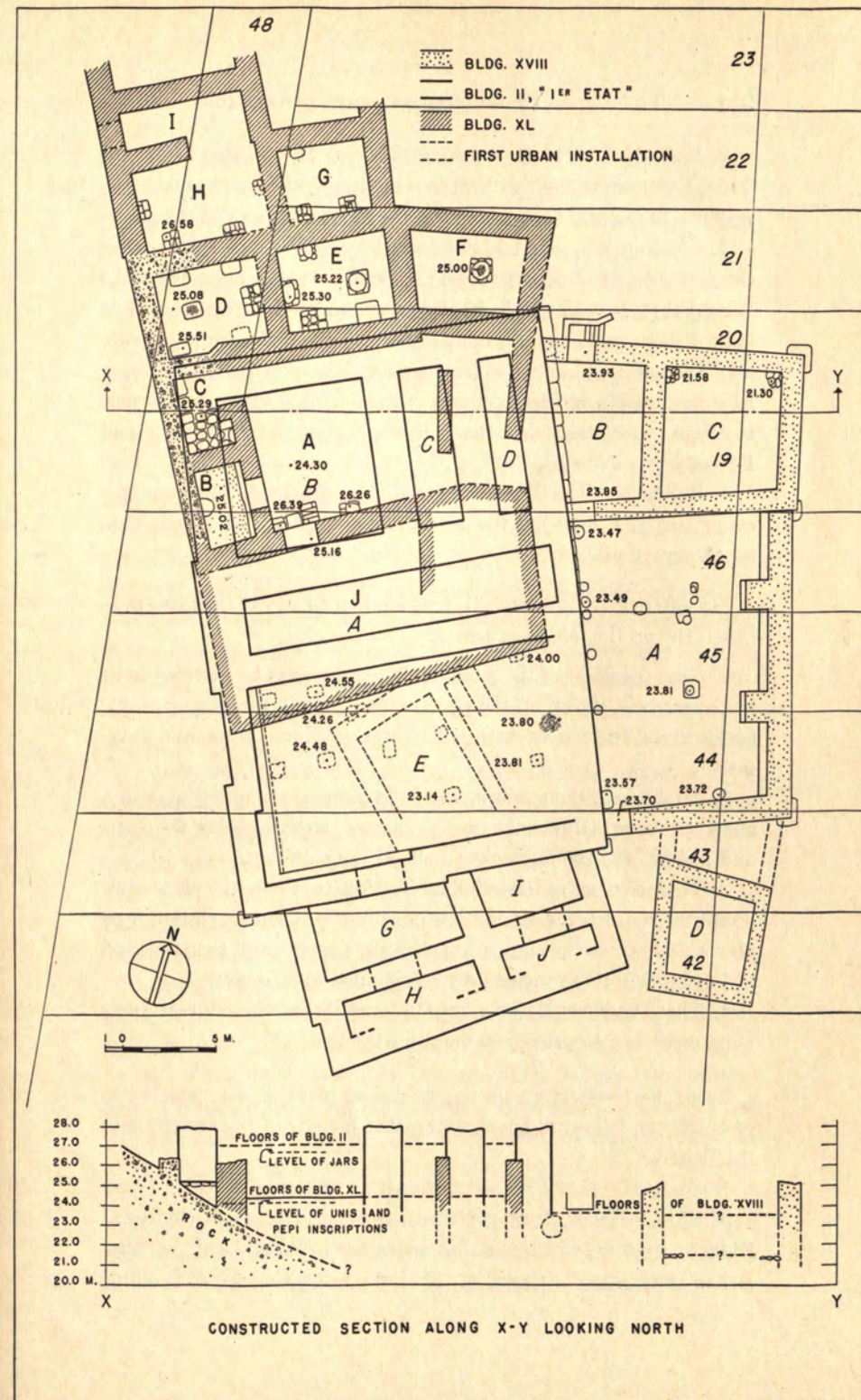


FIG. 1.—Plans of the Byblos Buildings Assembled and Superimposed, and a Constructed Section

2. Building XVIII—built against Building II, probably before the Third Dynasty (p. 304), with various additions, of which a great stairway is said to have been added toward the end of the Old Kingdom.

3. Building XL—overlaid in fair part by Building II; this is called the first important construction to be built above the buildings of the "first Urban Installation." M. Dunand says of Building XL: "Elle était déjà en ruine quand furent construits les plus anciennes parties du Bâtiment II, temple que les pharaons Khasekhemoui, Chéops, et leurs successeurs dotèrent de leurs offrandes." He makes no attempt to assign a more exact date than this implication that it is pre-Second Dynasty (pp. 295-96).

4. Buildings of the "Première installation urbaine," which underlie everything at Byblos but the aëneolithic necropolis. No precise date is attempted, of course.

My difference in interpretation is based on a study of the plans published and on the following assumptions:

1. That the floors of the buildings in question can be assumed to lie at those levels, shown on the plans, which contain such occupational architectural features as doors, sills, sockets, column bases, and pavements.

2. That where the stone walls of the buildings in question do not show the above features, the excavation has proceeded below the floors and has exposed the *foundation* walls of the buildings.

3. That in rooms of the buildings in question, where the plans show no intrusion of later walls or other possibilities of contamination from above, and, where the occupational features are present, then the level of the floors in those rooms may be assumed to be intact.

4. That the latest datable objects found below these intact floors must serve as a *terminus post quem* for the building.

From the levels given on the published plans, it was possible to establish the following floors, within the bounds of the assumptions listed above.

Building II—shows no occupational architectural features below ± 27.50 m. contour (from sea-level), save for a patch of cinders at 23.80 m. contour, which must belong rather to the walls of the "first Urban Installation" exposed at that level, below Court E of Building

II. The foundations of Building II descend as low as 22.60 m. contour, and the north run of the main east wall of the building rests on a wall of much larger stones which bears slightly eastward and is overlaid by the wall of Building XVIII, Room B, northwest corner, at least below 23.93 m. The change in masonry noted in *Fouilles de Byblos*, Volume I (i.e., Pl. XVI), as well as the cross-walls which exist within Room E just under 25.10 m. (Pl. CCVII), are strong arguments for M. Dunand's division of Building II into two phases, but *no* occupational features or floors below those at ± 27.50 m. are evidenced in the plans. One more point which assures the height of the floor of Building II in its northernmost portion is the fact that the walls of Building XL are preserved as high as 26.67 m. (p. 297).

Building XVIII—shows a number of doors, sockets, pillar supports(?) at ± 23.75 m. contour, but also two clumps of stones in the corner of Room C at ± 21.40 m. The preponderant number of occupational features at the higher level, however, makes the assumption of the floor at ± 23.75 m. more likely. The fact that the floors of Building XVIII are so much lower than those of the associated Building II must be accounted for by the general west-east pitch of the mound at this point. Even some clearance of older debris from along the east side of Building II may have been made in preparation for Building XVIII. Whatever the reason for the depth of the floors of Building XVIII (with levels comparable to those of the "first Urban Installation" just west of it under Court E, Building II), there can be little doubt that Building XVIII was annexed to Building II, as Dunand proposes, especially since its walls so plainly cover the basal wall of Building II, in Building XVIII, Room B.

Building XL—shows a considerable number of occupational features, in fact, it is practically possible to establish its floors room by room, between ± 25.25 m. and 24.30 m. contours. The difference in floor level is probably due to the proximity of rock below (the western part of Building XL is cut out of rock). How deeply Building XL is founded otherwise is not published, but its walls were preserved above floor level to as high as 26.67 m. contour.

With these floors once set out as indicated above, it is merely necessary to go through the inventory of objects in the *Fouilles de Byblos* to discover which are the latest pieces to underlie the floors of the various

buildings. These, especially in Building XL, have been chosen from areas where there is no published evidence to make one expect intrusion. The dating pieces are:

Building II—the Twelfth Dynasty jar deposits, said to have been buried under the pavement in Court E, 27.40–26.80 m. contours.

Building XVIII—fragments of inscriptions: No. 5136, with Old Kingdom titles similar to those of the Hesire panels of the Third Dynasty, in Room A, 23.0–22.8 m.; No. 5141, either Pepi, Room F (cf. Pl. CCVII), 23.0–22.8 m.; No. 5191, Pepi I, Room E, 22.8–22.6 m.

Building XL—fragments with cartouches: No. 3860, a Pepi, rectangle 48 ("Salle B"), 24.4–24.2 m.; No. 3980, Unis, rectangle 48 ("trouvé vers le centre du rectangle," i.e., must be under floors of Building XL, Rooms D, H, or I), 24.2–24.0 m.; No. 3981, Unis, rectangle 48 ("trouvé à côté du no. précédent"), 24.2–24.0 m.

On the basis of the evidence given above, my interpretation of the dating of Buildings II, XVIII, and XL would differ from the interpretation published by M. Dunand as follows:

1. Building II was built some time after Building XL (V–VI dynasties), which underlies it, probably quite close to the date of the Twelfth Dynasty jar deposits buried under its pavements. Whether there was an earlier stage of Building II or not, there is certainly no earlier fixed dating evidence published for it.

2. Building XVIII can be no earlier than the two Pepi inscriptions. On the other hand, if it must be considered an annex of Building II (for which the architectural evidence is excellent), then we must assume that there was no contamination of the earlier levels with contemporary materials when it was founded.

3. Building XL can be no earlier than the Fifth-Sixth Dynasty inscriptions found below its floors. Its latest limit is, of course, fixed by Building II above it.

In conclusion, the reader must again be reminded that this interpretation is based on such knowledge as can be gleaned from the published report. M. Dunand had the very evidence in his hands, hence his interpretation must be given first consideration.

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WOMEN AND THE STATE ON THE EVE OF ISLAM

NABIA ABBOTT

We have seen in a preceding article* that ancient Arabia had its queens and that some of these were also the priestesses of their local gods. They thus paralleled the priest-kings so familiar in the history of the entire ancient Orient. We have also seen that after the astonishing careers of the Emesan Julias and the Palmyrene Zenobia, Arab queens receded more or less into the background. Thus, for the last few centuries of the pre-Islamic period, characterized by the pious and proud Moslems as *al-Jāhiliyah*, or the "Age of Ignorance," we can point at best to a shadowy Ḥimyārite Balqis, a half-forgotten Ghas-sānid(?) Mawia, and a humiliated and bereaved Lakhmid Hind. The great majority of the royal women of these dynasties and of that of Kindah figure little or not at all in the available records. This may be due partly, as already pointed out, to the paucity and poverty of these records, if not indeed to the prejudice of the second- and third-century Moslem recorders. On the other hand, the situation may be reflecting some loss in public position suffered by the women in the centuries immediately preceding Islam. Changing social conditions, due in part to contacts with neighboring peoples and kingdoms, may have deprived the Arab woman of this period of some of the public prestige and privileges enjoyed by her earlier sisters.

However, it must not be inferred that the influence of the Arab woman had become negligible in the various phases of both private and public life. In her home the free Arab woman of all classes in her time-honored role of legal wife and mother expressed herself freely and forcefully. In poetry, the major literary passion of pre-Islamic Arabia, the Arab woman figured large. Not only did the romantic poets sing her praises in passionate verse but the chivalrous Arab, as yet not too civilized, coveted and prized her opinion as literary critic. The story is told of how the Kindite "vagabond prince" and greatest of Arab poets, Imrū al-Qais, during his wanderings settled for a while

* See "Pre-Islamic Arab-Queens," *AJSL*, LVIII (1941), 1–22.

among the Banū Ṭayy and married one of their women known as Umm Jundab. One day Imrū and that other famous poet, ʿAlqamah ibn ʿUbaidah, fell to arguing about their respective merits as poets, and neither would give preference to the other. Finally ʿAlqamah suggested that Imrū's wife should decide the question, and to this Imrū readily agreed. Umm Jundab then called on each to compose, in the same meter, a poem on the qualities of the horse, and, when this was done, she decided in favor of ʿAlqamah. Imrū, annoyed at the decision, divorced his wife whom ʿAlqamah then married.¹ Though this particular story may well be legendary, since the rivalry between the two famous poets has been questioned,² the presence of a woman literary critic in a land and at a time when poetesses received full recognition according to their merits is not at all improbable. For the pre-Islamic Arab accepted, as a matter of fact, woman's poetic contributions for general circulation.³ In this connection one need only mention that greatest of a long line of pre-Islamic Arab poetesses, Tumaḍir al-Khansā, who witnessed the advent of Islam and accepted the new faith, and whose poetry won the approval and praise of Mohammed.⁴

In the religious life, which generally linked up with the economic and political development and welfare of the people, a certain class of women played definite and well-recognized roles. Among them were the *kāhinah* or woman-seer and soothsayer, the *rabbat al-bait* or temple priestess, and, now and again, the more pretentious *nabīyah* or prophetess. They seem to have exercised their functions in the same way as did their masculine counterparts, the *kāhin*, *rabb al-bait*, and *nabī*. The known references to the *kāhinah* in Arabic literature are too numerous to list.⁵ The traditions associate one of these women with almost every major move of tribal policy or migration. There is, for instance, the well-known story of al-Zarqā, whose visions and pronouncements guided the movements of the Yamanite Tanūkhids

¹ *Aghāni*, VII, 126-28.

² Cf. Clément Huart, *A History of Arabic Literature* (New York, 1903), p. 16.

³ Lewis Cheikho, *Riyāḍ al-Adab fī Marāthī Shawāʿir al-ʿArab* (*Les Poétesses Arabes*) (Beyrouth, 1897), a work devoted to pre-Islamic Arab poetesses.

⁴ Ibn Ḥajar, *K. al-Iṣṣābah* (Calcutta, 1873), IV, 550; cf. Cheikho, *Commentaries sur le Diwan d'al-Ḥansā* (Beyrouth, 1896), pp. 19-23.

⁵ *EI*, II, 624-26; Wellhausen, *Reste arabischen Heidentumes* (Berlin, 1887), p. 130; Anastase Marie de St. Elie, "La Femme du desert autrefois et aujourd'hui," in *Anthropos*, III (1908), 60; Lammens, *L'Arabie occidentale avant l'Hégire* (Beyrouth, 1928), Index, "Kāhina"; Buhl, *Das Leben Muhammeds*, trans. Schaefer (Leipzig, 1930), p. 82.

northward to Ḥīrah.⁶ A similar and equally important role is assigned to the *kāhinah* Ṭuraifah, who accompanied other Yamanite tribes into the Ḥijāz.⁷ *Kāhinah*'s play their parts in the war of Basūs between Taghlib and Bakr and in the Battle of Dhū Qār⁸ between the Arabs and the Persians. A *kāhinah* consulted by the Quraish is said to have foretold Mohammed's prophetic mission by some twenty years.⁹ Several of them are mentioned in Mohammed's time: There is the Quraishite al-Ghailah,¹⁰ there is the *kāhinah* of Banū Ḥadas,¹¹ there is Hudhaim the *kāhinah* of the Banū Saʿd, there is Fāṭimah bint al-Nuʿmān at Makkah at the time of the Hijrah, and there is an unnamed *kāhinah* at Madīnah of about the same time.¹² Neither did they quite disappear¹³ with the coming of Mohammed, who was himself dubbed a *kāhin* by his opponents. The prophetess Sajāh, as we shall presently see, is likewise said to have started her role as a *kāhinah*.

The *kāhinah*, like the *kāhin*, was usually not restricted in action or movement. Not infrequently the *kāhin* performed also the duties of the *ḥākim*,¹⁴ or judge-arbiter. Whether the *kāhinah* as such performed this function is hard to trace, but the *ḥākimah*, or woman judge-arbiter, is met with in the traditions.¹⁵ Sometimes the *kāhinah* became associated with the shrine of a specific goddess or temple and was then called *rabbat al-bait*, or the mistress of the temple. Just what her specific functions were does not seem to be clear, though their politico-religious nature is hardly to be doubted. She does not appear in the traditions as frequently as does the simple *kāhinah*; but this may be due to a change in her position in the few centuries before Islam. Or again it may be due to the change in meaning that the word *rabb*, and

⁶ *EI* (Suppl.), art. "Tanūkh."

⁷ *Aghāni*, XIII, 110; Masʿūdī, *Murūj al-Dhahab*, ed. Meynard and Gourteille, III (Paris, 1864), 352, 379 ff.; for others see *ibid.*, pp. 364, 394 f.

⁸ Lammens, *op. cit.*, p. 122 f.; cf. also *ibid.*, p. 109, n. 6, for other incidents.

⁹ Aḥmad ibn Ḥanbal, *Masnad* (Cairo, 1313), I, 332; Ibn ʿAsākir, *Tārīkh al-Kabīr*, I, 367.

¹⁰ Ibn Hishām, *Sīrah*, ed. Wüstenfeld (Göttingen, 1859), pp. 132 f.

¹¹ *Ibid.*, p. 797.

¹² Ibn Saʿd, *Ṭabaqāt*, I, 49 f., 110, and 126.

¹³ Cf. *Aghāni*, XXI, 275.

¹⁴ Cf. Lammens, *op. cit.*, pp. 109, 135, 158.

¹⁵ *Aghāni*, XXI, 206, lists a number of these; cf. also Anastase Marie de St. Elie, *op. cit.*, p. 60 and Freytag, *Arabum proverbialia* (Bonn, 1838), I, 56, n. 1.

therefore also *rabbah*, underwent with the coming of Islam. For, whereas before Islam these words were applied to human beings, after Islam *rabb* was limited to the deity alone,¹⁶ and *rabbah* was left with no further strictly religious Islamic use. Nevertheless, there seems to have been some two or three of these temple priestesses in Mohammed's time,¹⁷ and one of them, Sarrā bint Nabhān, is said to have been converted to Islam.¹⁸ Arab prophetesses appear in the Moslem traditions even more rarely than do the priestesses. In fact, we know only of one active prophetess, the well-known Sajāh, whose prophetic career, to be dealt with below, ran its course largely after Mohammed's death.

We have seen how frequently the queens of ancient Arabia led their armies in person or accompanied their husbands on their campaigns. And what a queen does other women frequently do. It is, therefore, not surprising to find that the Arab woman on the eve of Islam played several roles in Arab warfare.¹⁹ At times she was the cause, if not the prize, of intertribal warfare; at others she used her wits in providing a sort of intelligence service for the benefit of family and tribe. Frequently as not she accompanied her men "to the front," both to inspire and to help. Many a woman urged brother, husband, and son to heroic action; and there was no title that an Arab woman coveted more than that of *munjibah*, or "mother of heroes."²⁰

Still this all important business of inspiring the warriors to courage, even unto death, was not left on major occasions to the inclination or patriotism of the individual woman. It was instead organized around a well-recognized institution that may well be called the cult of the Lady of Victory.²¹ A woman of outstanding social position would be placed within or associated with the portable *qubbah*, or sacred pavilion, of the tribal or local deity. Other women, varying in number, would accompany her. The sacred group, within sight and hearing

¹⁶ Lammens, *op. cit.*, pp. 134, 138f., 152, 154.

¹⁷ *Ibid.*, p. 152 and n. 4.

¹⁸ Ibn Sa'd, VIII, 227; cf. Gertrude H. Stern, "The First Women Converts in Early Islam," *Islamic Culture*, XIII (1939), 298.

¹⁹ Cf. Ilse Lichtenstädter, *Women in the Aiyām al-ʿArab* (London, 1935), pp. 13 ff.

²⁰ Caussin de Perceval, *Essai sur l'histoire des Arabes* (Paris, 1847-48), II, 417; Nicholson, *A Literary History of the Arabs* (Cambridge, 1930), p. 88; cf. also Aghāni, XVI, 20.

²¹ Cf. R. Geyer, "Die arabischen Frauen in der Schlacht," in *Mitteilungen der anthropologischen Gesellschaft*, XXXIX (1909), 148.

of the warriors, if not indeed in the actual fight from its start, urged and incited the men with their stirring war songs sung to the accompaniment of their lutes. The leader of the group was the Lady of Victory herself who, with hair flowing and body partly exposed, embodied an appeal to valor, honor, and passion. Around her and her women the battle raged until the day was lost or won. The practice no doubt had some age-long, though now perhaps somewhat dimmed, religious significance.²² Its complex psychological influence on the warriors is not to be underestimated. For according to their military code the capture of the chief lady meant the loss of the battle to her side and the consequent disdain of the women for the vanquished fighters. For the captured women themselves it might mean slavery and dishonor. On desperate occasions, as, for instance, in the Battle of Dhū Qār,²³ the Arabs either hamstringed the camels carrying the women or severed their saddles and litters so that the women fell to the earth. This device of thus incapacitating the women at a time when they were exposed to extreme danger was meant to banish from the minds of the men any thought of retreat or flight. The warriors had to fight or die.

In still another role the women played an important part both during and after a battle.²⁴ They formed a sort of Red Cross base behind the immediate line of action, while the bolder ones ventured into these lines with water to quench the thirst of the fighters and with simple supplies with which to dress their wounds. After the battle the women, going out on the field now strewn with the fallen, the wounded, and the dying, concentrated on their ministering services to their own at the same time that, with the added equipment of a club, they dispatched the wounded enemy soldiers to their final rest. In a few instances some of these women, thirsting and literally hungering for revenge, would give vent to their emotions by barbaric mutilation of a corpse.²⁵

With this brief sketch of woman's position and participation in public life, let us turn our attention to the women of the leading tribes

²² Lammens, *op. cit.*, section on "Le Culte des Bétyles," esp. pp. 120-25.

²³ *Naqā'id*, II, 643; for treatment of and references to several other incidents see Geyer, pp. 150 f.; Lichtenstädter, pp. 42 f.

²⁴ Geyer, pp. 152 f.; Lichtenstädter, p. 43.

²⁵ Geyer, pp. 152 f.

and families of Ḥijāz. Here again the Moslem traditions give us but bare glimpses of most of these women. But here and there an outstanding woman demanded and received their attention, though not infrequently the picture they give us of her is highly colored by the ideals and politics of her day and is sometimes even redrawn to harmonize with the policies of a later political party.

The first of the women of the Ḥijāz to come before us is Ḥubbā, the daughter of Ḥulail, the last Khuzā'ite priest-king of Makkah. The traditionally accepted story is that her father married her to the aggressive young Quṣayy whose Quraishite descent he accepted. To this couple were born four sons and two daughters, Takhmur and Barraḥ. Of the girls little is known, except that Takhmur is mentioned as mourning her father in verse.²⁶ The sons were 'Abd al-Dār, 'Abd Manāf, 'Abd al-'Uzzā, and 'Abd Quṣayy—names famous in Quraishite genealogy and in early Islamic history. As Ḥulail advanced in age he turned over the guardianship of the Ka'bah to his daughter Ḥubbā and handed her the key which she sometimes intrusted to her husband. Before he died he is said to have acknowledged his son-in-law, Quṣayy, and his own grandsons as his successors and to have left them his spiritual and temporal powers by a will, which, however, was repudiated or denied by the Khuzā'ah who thus forced Quṣayy and the Quraish to fight for their rights.²⁷ Other details, in contradiction with part of the above, are also woven into the story. Thus, when Ḥulail gave Ḥubbā the guardianship of the Ka'bah, she pointed out to her father her inability to open and close the gates. Whereupon he appointed Abū Ghubshān (who according to some was his own son, but according to others not so) to do that for her. Later, evidently after Ḥulail's death, Quṣayy made Abū Ghubshān drunk and purchased his office from him for a skin of wine and some camels.²⁸

Whichever version of this story we accept, the part played by Ḥubbā herself seems to be secondary. She seems to be willing to hand over the key to either her husband or her assistant, and she then dis-

²⁶ Ibn Sa'd, I, 39, 42.

²⁷ Ibn Hishām, 68, 75 f.; Ibn Sa'd, I, 37; Wüstenfeld, *Die Chroniken der Stadt Mekka* (Leipzig, 1857-61), I, 59 and 62 f., III, 44; Tabarī, I, 1092 ff.; Ibn Duraid, *Kitāb al-Ishtiqāq*, ed. Wüstenfeld (Göttingen, 1854), p. 276; cf. also Muir, *Life* (London, 1861), I, cc ff.; Caetani, *Annali dell'Islam*, I (1905), 99 ff.; Lammens, *op. cit.*, p. 112.

²⁸ Ibn Sa'd, I, 37; Tabarī, I, 1094; Mas'ūdī, III, 117 f.

appears from the records. If either of the versions is to be accepted, Ḥubbā is only a means of transferring the control of the Ka'bah from the tribe of Khuzā'ah to that of Quraish. On the other hand, if we do reject the story as a whole, there is nothing so improbable about its several details so far as Ḥubbā is concerned. As the daughter of a priest-king any woman might well be associated with some temple function; an ambitious and aggressive politician might well aspire to her hand and, having won it, use it to establish his own power and line. History is replete with the political marriage motif, and among the pre-Islamic Arabs themselves several instances are reported where political power was transferred from one house or dynasty to another through such marriages. There is, for instance, the case of the semi-legendary Balqis of the later Ḥimyārites and the Tanūkhid Jadhīmah of Ḥīrah who married his sister to a descendant of the Abgarids who then succeeded him in power at Ḥīrah.²⁹

Aside from this connection of the office of guardianship of the Ka'bah with Ḥubbā, who ought to be looked upon as a successor to or transmitter of her father's powers (and these, therefore, not necessarily limited to the guardianship of the Ka'bah), the office itself is not associated with any other woman. Some modern scholars have drawn attention to the fact that at the time of the conquest of Makkah the key of the Ka'bah was in the possession of a woman. They leave the impression that the sources themselves represent her as holding the key in her own right.³⁰ Actually, however, the traditions make 'Uthmān ibn Ṭalḥah, a descendant of Quṣayy through his son 'Abd al-Dār, the one who really held the office of the guardianship. According to some, he seems to have intrusted the key to his mother, Sulāfah, for safekeeping. It was from him that Mohammed demanded the key and it was to him, 'Uthmān, that his mother delivered it as it was again he, 'Uthmān, who handed it over to Mohammed.³¹ Sulāfah's part, so far as the traditions go, was apparently no more and no less than a mother's natural reluctance to see the key, and with it a lucrative office, pass from her son and family. When Mohammed, in ac-

²⁹ Ibn Hishām, *Kitāb al-Tijān* (Hayderabad, 1928-29), pp. 144 ff.; cf. Nabia Abbott, *The Rise of the North Arabic Script and Its Qur'ānic Development* ("OIP," Vol. L [Chicago, 1939]), p. 4.

³⁰ E.g., Lammens, *op. cit.*, p. 112; Stern, *op. cit.*, p. 298.

³¹ Ibn Hishām, *Sīrah*, p. 821; Wüstenfeld, *op. cit.*, I, 67, 184-87; Ya'qūbī, *History*, II, 61.

cordance with his policy of leniency and reconciliation, returned the key and the office to ʿUthmān ibn Ṭalḥah, his mother, Sulāfah, did not figure in the scene at all. When one recalls that *kāhīnah*'s, priestesses, and even a prophetess were on the scenes in the first years of Islam, one suspects that both Ḥubbā and Sulāfah have probably suffered considerable effacement at the hands of the Moslem traditionist.

So far as can be directly gathered from these same traditionists, the women do not seem to have been given any part in the government that Quṣayy eventually evolved at Makkah. The deliberations that were to be conducted in the *Dār al-Nadwah*, or council hall, were, with few exceptions, to be participated in only by the Elders or men of forty years and over. More often than not when the women of this period are mentioned in the traditions it is in connection with genealogy, since for this and the early Islamic period the mother's descent was as carefully investigated and as much valued as that of the father's. Here and there a woman is vaguely associated with some political affair. ʿĀtikah bint Murrah, wife of ʿAbd Manāf ibn Quṣayy and mother of several of his children including Hāshim, Muṭṭalib, and ʿAbd al-Shams, is credited with having a part in the *ḥilf al-Aḥābīsh*³² or a confederacy bringing together the Quraish and the Abyssinian and negro elements in Makkah. The circumstances and date of this confederacy are more or less obscure.³³ It is, therefore, not surprising that ʿĀtikah's part in it is also obscure. The text can mean either that she brought about the alliance or that she participated in the concluding ceremonies. The former alternative seems hardly probable, and the nature of the latter can perhaps be guessed at by the part attributed to a daughter of ʿAbd al-Muṭṭalib in the later *ḥilf al-Mutaiyabīn*, or the "Confederacy of the Perfumed."³⁴ This confederacy was organized by the Banū ʿAbd Manāf against the party of the Banū ʿAbd al-Dār, when the latter refused to give up their prerogatives in the Kaʿbah. In the concluding ceremony the parties to the confederacy, having first dipped their hands into a common bowl of perfume, placed them next on the Kaʿbah to sort of sanctify the common pledge. Two of ʿAbd al-Muṭṭalib's six daughters are associated with this occasion. According to some, it was Umm Ḥakīm, but, according to others, it was her sister, ʿĀtikah (of whom more presently), who

³² Yaʿqūbī, I, 279; cf. *EI*, I, 307 f.; art. "Ḥilf."

³³ Cf. Lammens, *op. cit.*, pp. 264-66. ³⁴ Cf. *EI*, I, 307 f., Arabic sources there cited.

prepared the bowl of perfume from which the confederacy took its name.³⁵ To whichever of the two sisters we concede the honor, the part itself so far as the traditions go seems to have been incidental.

We know next to nothing of Fāṭimah bint ʿAmr, one of several wives of ʿAbd al-Muṭṭalib, but mother of five of his daughters and three of his boys, including ʿAbd Allah and Abū Ṭalīb, and therefore grandmother of both Mohammed and ʿAlī.³⁶ Four of her daughters, aunts though they were of Mohammed, are little more than names to us.³⁷ The fifth was ʿĀtikah, of whom the following story is told.³⁸ Three days before the arrival of Abū Sufyān's messenger at Makkah bringing the unexpected news of the threatened danger to his caravan she dreamed of this messenger and of the unwelcome message he had to bring. She was much alarmed and confided her dream to her brother ʿAbbās, who in turn confided it to a few others. Presently the dream was known all over the city and became a lively topic of conversation. Abū Jahl, Mohammed's inveterate enemy, seized the opportunity to taunt ʿAbbās with, "Is it not enough that your family has produced a prophet? Now you have a prophetess too! If nothing happens within three days, we shall be obliged to testify that you, the Banū Hāshim, are the greatest liars among the Arabs!" But the events, so the story goes, came to pass just as ʿĀtikah had dreamed them. She was converted, presumably soon after, and migrated to Madīnah. We hear little more of her, although she outlived Mohammed.

Yet another daughter of ʿAbd al-Muṭṭalib and aunt of Mohammed has received some considerable notice at the hands of the traditionists. She was Ṣafīyah, full-sister of Uncle Ḥamzah and mother of Zubair ibn al-ʿAwwām and therefore the grandmother of the future rival caliph ʿAbd Allah ibn al-Zubair. She seems to have been among the small group of early converts at Makkah and to have migrated early with the rest to Madīnah. At the Battle of Uḥud (3/625) when Mohammed's forces were forced to retreat, Ṣafīyah, spear in hand, rushed among the soldiers, striking them and crying out scornfully, "So you desert the messenger of Allah!"³⁹ When the battle was over, Fāṭimah, we are told, dressed her father's wound, and Ṣafīyah searched for her

³⁵ Yaʿqūbī, I, 288, II, 16.

³⁶ Ibn Hishām, pp. 69 f.

³⁷ Cf. Ibn Saʿd, VIII, 27-31. Ibn Hishām (pp. 108-11) tells how all six daughters composed elegies on the death of their father.

³⁸ Ibn Saʿd, VIII, 29 f.; Ibn Hishām, pp. 428-30.

³⁹ Ibn Saʿd, VIII, 28.

brother Ḥamzah, to whom she was much attached. Mohammed, anxious to spare her the sad sight of Ḥamzah's mangled remains, told her son Zubair to head her off. But she would not be denied. "I will not go back," she cried, "until I see him." Leading her to the body, Mohammed left her to her anguished grief. Then to comfort her he told her that Ḥamzah's name was listed in paradise as the "Lion of Allah and of his Apostle."⁴⁰

We find her again in an aggressive part during the siege of Madīnah at the time of the Battle of the Trench (5/627). The women and children had been placed for safety in a fortlet belonging to Ḥassān ibn Thābit, Mohammed's court poet. Ḥassān, who fought for Islam with the pen but not the sword, was in the fort with this group. The Banū Qurayzah, Jewish allies of Mohammed, were being courted by Abū Sufyān and were therefore becoming suspect to the Madīnese, who were presently to accuse them of treachery, for which the unhappy tribe was later mercilessly massacred. While Mohammed still had his hands full with the enemy, Ṣafiyah, back in the fort, noticed a Jew prowling about. Suspecting him of being a spy, she asked Ḥassān to attack and kill him. But Ḥassān had no taste for the undertaking. So Ṣafiyah, taking a club, according to some, but a sword according to others, lay in wait for the Jew and, slipping through the gate, stealthily struck and killed him.⁴¹

In another and later scene she appears not quite in character with the fighting Ṣafiyah of the preceding incidents. The occasion was that of the conquest of Khaibar. In the usual challenge to single combat before the battle Zubair rushed forth to meet a Jewish challenger, whereupon Ṣafiyah, much alarmed, ran up to Mohammed and expressed her fears for her son's life. Mohammed assured her he would be victorious, Allah willing. Allah indeed so willed.⁴² Allah's messenger then gave Aunt Ṣafiyah forty camel-loads of the produce of the newly acquired Khaibar while a similar gift went to his Aunt Umamah, who was also his mother-in-law, being the mother of his cousin-daughter-in-law and wife, Zainab bint Jaḥsh.⁴³ The last glimpse the traditions give us of Ṣafiyah is her presence, together with Fāṭimah, with the sick

⁴⁰ *Ibid.*, III, 7-9 and VIII, 28; *Aghāni*, XIV, 23; cf. Muir, *Life*, ed. Weir (Edinburgh, 1923), p. 264.

⁴¹ Ibn Hishām, p. 680; Ibn Sa'd, VIII, 27 f.; *Aghāni*, IV, 16; *Iṣṭabāh*, IV, 671.

⁴² Ibn Hishām, p. 761.

⁴³ Ibn Sa'd, VIII, 27 and 31. Cf. Ibn Hishām, pp. 773-75, for the long list of both men and women who received gifts on this occasion.

and dying Mohammed just before he was transferred to the house of Aishah. "Oh Fāṭimah, my daughter and thou Ṣafiyah, my aunt! Work ye out that which shall gain acceptance for you with the Lord; for I verily have no power with Him to save you in anywise."⁴⁴ She, with her sisters Arwā and ʿĀtikah, composed several elegies on Mohammed⁴⁵ as did other women and men, the latter including Abū Bakr and the poet Ḥassān. Ṣafiyah herself died in the caliphate of ʿUmar ibn al-Khaṭṭāb.⁴⁶

Two women of the family of Abū Sufyān, chief of the Quraysh at the time of Mohammed, have received considerable attention at the hands of the Moslem traditionists and historians. They were his wife, Hind bint ʿUtbah, who opposed Mohammed and the new faith, and his daughter (not by Hind), Ramlah, who early accepted Islam and later married its prophet.

We do not know when Hind was born or when and to whom she was first married.⁴⁷ She was descended from the ʿAbd al-Shams' branch of the Quraysh, and we first meet her as the wife of Fākih ibn al-Mughīrah the Makhzūmite, the uncle of Khālid ibn al-Walīd⁴⁸ the future "Sword of Islam." Fākih, suspecting Hind's fidelity and claiming to have seen a stranger leaving their private apartment, repudiated his wife and sent her back to her parents, thus exposing her to the active gossip of the community. Her father, ʿUtbah, distressed at this development, was willing to go to any length to save his daughter's reputation. He, therefore, informed her that he had two alternative plans of action. If she was guilty, he would have Fākih assassinated and thereby silence his accusations; but, if she was innocent, then he would demand that Fākih submit the case to trial before some Yamānite *kāhin*'s. Hind vigorously protested her innocence, and the second plan was then followed with due publicity and ceremony. The *kāhin* pronounced Hind not guilty; furthermore, he prophesied that she would be the mother of a king named Muʿāwiyah. Fākih, accept-

⁴⁴ Ibn Sa'd, II, 17, 46; cf. Muir, *op. cit.*, p. 494.

⁴⁵ Ibn Sa'd, II, 93-97.

⁴⁶ *Ibid.*, VIII, 28.

⁴⁷ Ibn Sa'd, VIII, 170, does not mention her husband Fākih but says she was married to Ḥafṣ ibn al-Mughīrah, to whom she bore Abān; her marriage to Ḥafṣ is mentioned also by Ibn Qutaibah, *ʿUyun*, I, 283. Chronologically, this marriage seems to belong to the earlier career of Hind before her marriage to Fākih, unless there has been some confusion between Fākih and Ḥafṣ.

⁴⁸ *Aghāni*, VII, 26.

ing the decision, was more than willing to take her back, but she now would have none of him.⁴⁹

How much of this story is fact and how much fiction can be left to the judgment of the reader. However, in its essentials this, the earliest story we have of Hind, characterizes her as a woman holding to the heathen practices of Arabia, a wife whose virtue was not above suspicion, and a proud lady with a mind of her own, a mind that was quick to decisive action. All three characteristics will cling to her throughout her career as we have it in the traditions.

Hind, thus parted from Fākih, had no lack of suitors. There was Musāfir ibn Abi ʿAmr, a minor Quraishite poet and a relative, who had developed a passionate love for her, and who now wished to marry her. Though he was well known for his poetry and generosity and held the respected title of "the traveler's provider," indicative of his great hospitality, Hind was satisfied with neither his position nor his wealth. She refused to marry him, though gossip again had it that she received him as a lover and that, fearing another scandal, she asked him to leave. Musāfir, still much enamored and hopeful, wended his way to Ḥīrah to the court of the famed ʿAmr ibn Hind with the express purpose of seeking there fame and fortune with which to win his beloved to wife. Fate granted him the first of his wishes but ironically denied him the last for which alone the first was sought. For at Ḥīrah Musāfir won ʿAmr's favor and prospered. But Hind either knew not of this or cared not to await the return of a distant lover. Two other suitors were at hand, and she was free to choose either, having first heard her father's description and estimate of them.⁵⁰ The one, Suhail ibn ʿAmr, she refused because, though he was noble, generous, and good tempered, he was withal a weak man. The second and successful suitor described in glowing terms as a man strong in character and in leadership, was none other than Abū Sufyān Ṣakhr ibn Ḥarb ibn Umayyah, the Quraishite, a wealthy and influential merchant if not the chief of his tribe and the actual leader of the city republic. The unfortunate Musāfir did not long survive the shock of this news casually imparted to him by none other than Abū Sufyān himself while on a trading trip to Ḥīrah.⁵¹

It was soon after that Hind gave birth to Muʿāwiyah, the future

⁴⁹ *Aghāni*, VIII, 50 f.; Ibn ʿAbd Rabbihi, *ʿIqd al-Farid* (Cairo, 1293), III, 273 f.

⁵⁰ Ibn Saʿd, VIII, 171; *ʿIqd*, III, 274 f.

⁵¹ *Aghāni*, VIII, 49 and XIX, 105; Caussin de Perceval, I, 336-38.

caliph and founder of the Umayyad dynasty. The year of this event is not known; it is estimated as falling within the first decade of the seventh century of our era.⁵² She bore Abū Sufyān one other son, ʿUtbah, and two daughters, Juwairiyah and Umm al-Ḥakam.⁵³ She was, of course, one of several wives of Abū Sufyān, though we hear little of her relationship with the other wives. Ibn Qutaibah relates how she constantly nagged Abū Sufyān until he divorced one of these, Ṣaʿbah, the daughter of ʿAbd Allah ibn Mālik.⁵⁴

The traditions, so far, afford us only one definite reference to Hind for the entire period covering Mohammed's ministry at Makkah. Abū Lahab, the disapproving and actively antagonistic uncle of Mohammed, had already drawn a Qurʾānic curse on his head; so also had his wife, referred to as the "carrier of wood" in *Sūrah* 111, which is devoted entirely to the curse on this couple. Abū Lahab's family relations to Mohammed were further complicated by the fact that his son had married one of Mohammed's daughters, either Ruqaiyah or Umm Kulthūm. Abū Lahab's own wife, Umm Jamīl, the sister of Abū Sufyān, disliked the girl and her father Mohammed and incited her husband against them. Abū Lahab forced his son to divorce Mohammed's daughter and when the sharp break came between Mohammed and his Makkan persecutors, who placed a ban and a boycott on the new movement, Abū Lahab deserted the Hāshimites and joined the opposition. Among the chief leaders of the opposition were several of Hind's relatives—including her father ʿUtbah and her uncle Shai-bah and, of course, Abū Sufyān. Hind's own part at this stage of the opposition is not stated. That she must have been keenly interested in the current events is hardly to be doubted. Abū Lahab, meeting her on the street during the period of the ban, asked and received her approval of his conduct in the cause of Allāt and al-ʿUzzā and in the cause of those who followed them.⁵⁵

But not all of Hind's family were opposed to Mohammed. Her brother, Abū Ḥudhaifah, was among the first converts to Islam. At the Battle of Badr (2/623) he challenged his father, ʿUtbah, to single combat; and Hind was quick to satirize him for this unfilial conduct.⁵⁶

⁵² Lammens, in *EI*, III, 617.

⁵³ Ibn Qutaibah, *Kitāb al-Maʿārif*, ed. Wüstenfeld (Göttingen, 1850), p. 175; Ibn Saʿd, VIII, 174 f.; *Iqābah*, IV, 854.

⁵⁴ *ʿUyūn*, IV (Cairo, 1930), 101; for Ṣaʿbah see *Iqābah*, IV, 664.

⁵⁵ Ibn Hishām, 231. For Abū Lahab's family situation see *ibid.*, p. 465.

⁵⁶ Ibn Saʿd, III, 59 f.; cf. Muir, *Life*, pp. 60 and 228.

After the battle she satirized her cousin, Ramlah bint Shaibah, who had become Moslem and was married to ʿUthmān ibn ʿAffān.⁵⁷ There was also later her stepdaughter, Ramlah bint Abī Sufyān, who married Mohammed.

It is in connection with the aftermath of the Battle of Badr that we get our first full view of Hind. But first we must take note of a curious story that connects her with the flight of Mohammed's daughter, Zainab.⁵⁸ Hind is here cast in the unexpected role of Zainab's helper, the story being traced back to Zainab herself. Hind, so the story goes, hearing of Zainab's plans to join her father at Madinah, offered to help her with supplies for the journey, justifying her unexpected action to the suspicious Zainab by a statement to the effect that the affairs of the men did not concern the women. When Zainab, starting on the journey, met with rough treatment at the hands of those who would prevent her departure, it was Hind who rebuked the ruffians with, "Ah! in time of peace ye are very brave and fierce against the weak and unprotected, but in battle ye are like women with gentle speeches." Hind's ready tongue may have uttered the gibe against Zainab's assailants; but it is difficult to account for her offer of help to Zainab. The account states that Abū Sufyān and the Quraish, having but recently suffered defeat and comparatively heavy losses at Badr, were lying low and were therefore ready to connive at Zainab's departure. Hind may, therefore, have been acting in accordance with this policy, but even then she would hardly need go to the length of offering direct help to Zainab. Further, if we accept this story, we would have to credit Hind with an amount of outward diplomacy that is incompatible with her general character, for her conduct in this instance is in conflict with the rest of her activities in this same period.

Hind, having lost father, uncle, and brother at Badr, took the affairs of men very much indeed to her woman's heart. Tradition credits her with several dirges on her father's death, though those learned in poetry are skeptical. However, her grief for her dead and her determination to avenge them are not to be questioned. Makkah, humiliated, was burning for revenge; but pride stifled the expression of natural grief. "Weep not for your slain," cried Abū Sufyān, "bewail not their

⁵⁷ Ibn Sa'd, VIII, 173 f.; Balādhurī, *Ansāb*, V, 106.

⁵⁸ Ibn Hishām, 466-68; Ṭabari, I, 1348 f.; Muir, *Life*, pp. 345 f.; but see Henri Lamens, *Fātima et filles de Mahomet* (Rome, 1912), pp. 5-7, where Zainab's very existence is questioned.

loss; neither let the bard mourn for them. Show that ye are men and heroes! If ye wail and lament, and mourn over them with elegies, it will ease your wrath and diminish your enmity toward Mohammed and his followers. Moreover, if that reach our enemies' ears, and they laugh at us, will not their scorn be the severest calamity of all? Perchance ye may yet obtain your revenge. As for me, I will touch no oil, neither approach any woman, until I go forth to war against Mohammed." A month passed before their pent-up grief broke out with double force, and for another month all Makkah wept for its dead—except Hind. "Why sheddest thou no tears?" they asked of her; "why weepst thou not for thy father ʿUtbah, for thy brother, and thine uncle?" "Nay," she replied, "I will not weep until ye again wage war with Mohammed and his followers. If weeping would wash away grief from my heart, I would weep even as ye; but it is not thus with Hind."⁵⁹

Dwelling on her triple bereavement, Hind now saw fit to challenge the claim of al-Khansā of being the most bereaved of all the Arabs. Al-Khansā, who had lost her father and two full-brothers before Islam, had continued to mourn them in stirring and touching verse. It was her custom to visit the annual fair at ʿUkāz, riding in a pavilion marked by a banner, and there to give expression in verse to her grief. Hind, we are told, set out for the fair in a similar pavilion, sought out the poetess, and challenged her claim, whereupon both women composed extempore elegies on their distinguished dead.⁶⁰ No decision in favor of either is recorded, but the future was to accord the claim to al-Khansā, who lost four noble sons in the Battle of Qādisīyah.⁶¹

In the meantime the Quraish were preparing to avenge Badr and remove the stigma cast upon them by that defeat. Abū Sufyān, now the most prominent and acknowledged leader in Makkah, was foremost in these preparations, of which Hind no doubt approved. Furthermore, by virtue of her position as his wife, if not indeed by virtue of her own aggressiveness, she seems to have been accorded the role of leader among the women. The day of expected revenge drew near, and Hind and her women were ready.

⁵⁹ Muir, *Life*, pp. 236 f.; Wāqidī, *Kitāb al-Maghāzī*, ed. Kremer (Calcutta, 1856), pp. 14-18; this will be the edition cited unless Wüstenfeld's German translation is specifically indicated.

⁶⁰ *Aghānī*, IV, 34 f.; Cheikho, *Commentaries* . . . , pp. 58 f.

⁶¹ *Iqābah*, IV, 551; Cheikho, *Commentaries* . . . , pp. 21-23.

The Quraish, led by Abū Sufyān, marched to the Battle of Uḥud (3/625) accompanied by their women and their gods in true heathen fashion. Most of the traditions enumerate the leaders as accompanied by their wives or mothers—Abū Sufyān, according to some, taking with him not only Hind but also a second wife, Umaimah bint Sa'd.⁶² Led by Hind, some fourteen or fifteen of these women, the cream of the Makkān social aristocracy, played the time-honored role of Arab women in major battles. They sang their stirring war song, they played their tambourines, they danced and rushed onto the battlefield heedless of danger. Their men, seeing them, had to fight or die. In short, Hind and her women were the "Lady of Victory" group. And Hind played her part with amazing energy and spirit.⁶³ Nor were the rest of the women lacking in spirit or courage. When at one time the Quraish standard was down, it was one of them, 'Amrah bint al-Ḥārith, wife of Ghurāb ibn Sufyān, who held it up.⁶⁴ When the battle was over and Quraish had won the day, Hind's long-awaited hour of revenge had come. It was not enough to know that Ḥamzah, who had killed her father at Badr, was dead by the hand of the Abyssinian, Abū Dasamah, whom she had specifically and repeatedly urged both before and during the battle to dispatch him, but she must herself see the fallen enemy and vent her wrath on his corpse. It is at this point that the traditions paint for us a particularly diabolic Hind. She is credited with tearing out Ḥamzah's liver and biting it, with cutting off his nose and ears, with accumulating on the field enough noses and ears to make necklaces, bracelets, and anklets which she wore with fiendish glee.⁶⁵ Then, standing on a high rock, she exultantly flaunted in the face of the fallen enemy the general victory and her personal revenge in extempore satirical verse which drew answer from the women in Mohammed's party and later from Ḥassān ibn Thābit.⁶⁶

It is hard to believe that Hind, the aristocratic matron of Makkah, went to the barbaric revenge with which she is credited. Muir⁶⁷ long ago suggested that this picture of her as a Fury

⁶² Ibn Hishām, p. 557; Wāqidī, p. 201; Ṭabarī, I, 1385 f.; *Aghānī*, XIV, 12 f.

⁶³ Ibn Hishām, p. 562; Wāqidī, pp. 207, 221; Ṭabarī, I, 1400 f.; *Aghānī*, XIV, 17.

⁶⁴ Wāqidī, p. 201.

⁶⁵ Ibn Hishām, pp. 580-82; Ibn Sa'd, III, 5 f.; Wāqidī, p. 279; Ṭabarī, I, 1415 f.; *Ya'qūbī*, II, 48.

⁶⁶ Ibn Hishām, pp. 580-82; Ṭabarī, I, 1415-17; *Aghānī*, XIV, 20 f.; Ḥassān ibn Thābit, *Diwān*, ed. Hirschfeld (London, 1910), Nos. 214, 224 f.

⁶⁷ *Life*, III (London, 1861), 129.

was overdrawn by the traditionists, and Lammens⁶⁸ goes further and suggests that the account of her orgy was likely an 'Abbāsīd invention. Both of these suggestions I find are reinforced by a direct though hitherto overlooked statement of Ibn Sa'd, who, after relating the incident, adds as his own opinion, "These are violent attacks on poor Hind."⁶⁹ It is interesting to notice in this connection that her son Mu'āwiyah, founder of the Umayyad dynasty and hated alike by 'Alids and 'Abbāsīds, is sometimes referred to in the traditions as the "son of the (human) liver-eater."⁷⁰

Another story of Hind at Uḥud seems likewise to bear the earmarks of an invention. It is that Abū Dujānah,⁷¹ armed by a sword which Mohammed had given him, penetrated the enemy ranks and headed for the women. He reached Hind and held his sword over her head ready to strike but hesitated, unwilling to have it said that he had stained that sword with a woman's blood.

We hear little of Hind's doings until the next and final trial of strength between Mohammed and the Quraish. This was in connection with the conquest of Makkah in the Year Eight of the Hijrah. Here she appears in opposition to Abū Sufyān's policy of appeasement and surrender. Had she had the final word, she would have ordered the Quraish to give battle. When she realized that Abū Sufyān had practically handed the city over to Mohammed, her rage knew no bounds. She publicly denounced Abū Sufyān. Taking him by his beard and striking him with her hands, she cried out, "Kill this old fool, for he has changed his religion." Abū Sufyān himself the while was crying out, "O people, become Moslems and be saved!"⁷² Again when 'Abbās was proclaiming Mohammed's terms of safety, Hind strove to stir up the crowd against him.⁷³ But her efforts were of no avail. Realizing that the day, and with it the cause, was lost, she vented her wrath this time on her powerless gods. Shattering her idols to pieces, she cried, "We have certainly been deceived in you!"⁷⁴

Tradition next has it that Mohammed had condemned five or six men and four women to death. Three of these were of the lower class, singing girls who had spoken evil of him. Two are believed to have

⁶⁸ *L'Arabie occidentale*, p. 124.

⁶⁹ Ibn Sa'd, III, 6.

⁷⁰ *Iqd*, II, 138; Ibn al-Ṭīqīqā, *Fakhrī*, ed. Derenbourg (Paris, 1895), p. 144.

⁷¹ Cf. Caussin de Perceval, III, 101.

⁷² Wāqidī, pp. 308, 413.

⁷³ Ibn Hishām, p. 815.

⁷⁴ Wüstenfeld, *op. cit.*, I, 78; *Iṣṣabah*, IV, 821.

escaped the sentence; the third, according to some, is said to have paid the penalty.⁷⁵ The fourth on whom this sentence was passed was none other than Hind, and she escaped it by becoming Moslem and hastening, all muffled up, to take the oath of allegiance to Mohammed, who did not realize at first that he was indeed speaking to his erstwhile enemy, Hind.

If any incident in Hind's career is to be discredited, this certainly should be. It is inconceivable that Abū Sufyān, despite his differences with his wife, would consent to any agreement with Mohammed with a sentence of death hanging over Hind. Nor can we imagine Mohammed, with his farsighted policy of peaceful conquest and reconciliation, seriously considering such a sentence. Finally, Hind, in the taking of the oath of allegiance, speaks not as one fearing or sidestepping a death sentence but delivers herself with pride and spirit if not with veiled resentment.

Mohammed, having first received the oath of allegiance from the men of the now conquered city, turned his attention next to the women whose oath he deemed necessary for the full completion of the conquest and for the firm establishment of the new faith. Again, as at Uḥud, Hind was the leader and the spokeswoman for the group. The scene, much dramatized by the historians, displays the Makkan women and their men, together with Abū Sufyān, at one side of the stage and Mohammed's party, including ʿUmar ibn al-Khaṭṭāb and ʿAbbās ibn ʿAbd al-Muṭṭalib, at the other, with Hind and Mohammed in the center. Mohammed began the administration of the oath:

Thou shalt have but one God.

"We grant you that."

Thou shalt not steal.

"I only stole provisions from Abū Sufyān, who is too stingy to give me enough." "That," said Mohammed, "is not theft."

Thou shalt not commit adultery.

"Does a lady commit adultery?"

Thou shalt not kill thy children.

"We brought up our young children but you killed them full grown at Badr."

⁷⁵ Ibn Saʿd II, 98; Yaʿqūbī, II, 60 f.; Ibn Hishām, pp. 819 f.

Thou shalt not slander.

"Slander is indeed abominable and exceeds all bounds."

Do not disobey me in anything that is right.

"Had we intended to disobey you, we would not be here now."⁷⁶

Hind was as good as her word. Thereafter she was on friendly terms with Mohammed, to whom on one occasion she is said to have brought a gift of two lambs⁷⁷ and to whom on other occasions she confided her economic difficulties with the close-fisted Abū Sufyān.⁷⁸ Later she was to fight as strenuously and wholeheartedly in the cause of Islam as she had previously fought against it.

It is not until early in the reign of ʿUmar ibn al-Khaṭṭāb that we hear again of Hind. She and Abū Sufyān were up in Syria visiting their son Muʿāwiyah, who was governor of that province. Both husband and wife took part against the Byzantines in the hard-fought Battle of Yarmūk (15/636), in which the Moslem women as a group fought with great energy. Hind's daughter, Juwairiyah, was among them and was wounded. Hind herself is cast in a characteristic role of leadership, her battle cry on this occasion being, "Strike the uncircumcised with your swords!"⁷⁹

Some time after this Abū Sufyān divorced Hind,⁸⁰ but the reason is nowhere told. Both were by now pretty well advanced in age, Abū Sufyān being past seventy, since he died some sixteen to twenty years later aged eighty-eight years.⁸¹ Hind, though past middle age, must have still retained her charm if the story that Muʿāwiyah refused her hand to an unnamed suitor is to be believed.⁸² The divorce does not seem to have affected her sons' affection for her, for we find Muʿāwiyah and his brother ʿUtbah taking pride in being the sons of Hind.⁸³

Hind took to trading after her divorce. She had no capital of her own and one would hardly expect Abū Sufyān, of whose stinginess she had complained repeatedly, to advance her any. We find her bor-

⁷⁶ Wāqidī, pp. 416 f.; Ibn Saʿd, VIII, 4; Tabarī, I, 1643 f.; Ibn al-Athīr, *Kāmil fi al-Tarikh (Chronicon)*, ed. Tornberg (Upsaliae, 1851-76), II, 192 f.; *Fakhri*, pp. 144 f.; *Iḡābah*, IV, 821. Cf. also Wensinck, *A Handbook of Early Muḥammadan Traditions* (Leiden, 1927), p. 8, under heading, "Abū Sufyān: His Covetousness." These treaty clauses are found also in *Sūrah* 60:12.

⁷⁷ Ibn Saʿd, VIII, 171 f.; Ibn al-Athīr, II, 191.

⁷⁸ Ibn Saʿd, VIII, 172; reported frequently in the *Ṣaḥīḥain*.

⁷⁹ Balādhurī, *Futūḥ*, p. 135; Muir, *Annals* (London, 1883), p. 109.

⁸⁰ Tabarī, I, 2767.

⁸¹ *Iḡābah*, IV, 821 f.

⁸² Nawawi, *Biog. Dict.*, p. 726; cf. also *EI*, I, 108.

⁸³ Tabarī, II, 69, 210; *Iqd*, II, 129.

rowing capital from the caliph ʿUmar and trading in the north in the lands of the Banū Kalb. Hearing that Abū Sufyān and his son ʿAmr were on a visit to Muʿāwiyah, she too made her way there to see Muʿāwiyah and to caution him against making lavish gifts to his father and half-brother lest the people be dissatisfied and complain of such gifts to the stern ʿUmar, who, being what he was, would not forgive him. Muʿāwiyah followed her advice and gave his father and brother a very modest gift, the size of which Abū Sufyān immediately attributed to Hind. The three traveled together to Madīnah, where Hind disposed of her merchandise and where she had some argument with ʿUmar about the trade duties which he refused to remit since, he said, they belonged not to him personally but to the public treasury.⁸⁴ She seems to have sensed how futile it was to oppose the stern ʿUmar, for we find her on yet another occasion advising Muʿāwiyah to act in accordance with his wishes.⁸⁵

Hind did not live to see Muʿāwiyah as caliph. According to some accounts, she died in the reign of ʿUmar in the year 14/635.⁸⁶ This must surely be an error, since she took part in the Battle of Yarmūk, at which time she was still the wife of Abū Sufyān. The story of her trading is placed by Ṭabarī under the year 23; this would fit in with the accounts that place her death in the reign of ʿUthmān.⁸⁷

We have gone into the details of Hind's career for several reasons. Her own personal story, in what we can gather of it, is arresting. She might in a way be considered the last "queen" of pre-Islamic western Arabia. The traditional accounts of her story bear repeatedly the mark of later political coloring. In her private role as daughter, wife, and mother she claimed and exercised her rights as a free and spirited Arab woman. In her public role as the leading woman in the Makkan republic she was both a fearless though unheeded counselor and a ready fighter. Resenting the major role she, a woman, played against Mohammed, the Moslem traditionist of a later century, encouraged further by dynastic rivalries, caricatured the temperamental and aggressive Hind in the cause of Islam and to the detriment of the fallen Umayyads.

But Hind was not the only Arab woman to take a leading and ag-

⁸⁴ Ṭabarī, I, 2766 f.

⁸⁵ *Iqd*, II, 300.

⁸⁶ Masʿūdī, *Tanbih* ("BGA"), VIII (1894), 287; Ibn al-Athīr, II, 380.

⁸⁷ *Iḡābah*, IV, 821 f.

gressive part against Mohammed and Islam. The Banū Ghatafān resented Mohammed's encroachment on their territory to the north-east of Madīnah. Their chief and leader was ʿUyainah ibn Ḥiṣn of the Fazārah who on several occasions clashed with Mohammed's men and with whom Mohammed finally made a treaty in order to keep the peace.⁸⁸ Again, during the siege of Madīnah in the year 5/627, ʿUyainah's threatening opposition to Mohammed caused the latter to favor a treaty between them even to conceding ʿUyainah one-third of the produce of the date trees of Madīnah. Fortunately for Mohammed there were those in his party who would not hear of this and who wished to give ʿUyainah "nothing but the sword."⁸⁹

The next year we find Zaid ibn Ḥārithah, Mohammed's adopted son, on an expedition to Wādī al-Qurā against the Fazārah seeking to avenge a previous skirmish in which he had received a wound. He found the Fazārah on this as on the previous occasion led by the widow of Mālik ibn Ḥudhaifah, Umm Qirfah Fāṭimah bint Rabīʿah, a well-known and powerful woman though much advanced in age. She in person led her party including her numerous sons and grandsons against the enemy. But the day went against her; she and her beautiful daughter, Umm Ziml Salmā bint Mālik, were taken captive. Zaid avenged himself by putting the aged woman to a barbarous execution, tying each foot to a beast which when driven tore her in two.⁹⁰

The defeat and drastic punishment may have had its influence on ʿUyainah since the dead Umm Qirfah was his aunt and the captive Salmā his cousin. At any rate we find him the next year in Mohammed's camp, though he was a follower with mental reservations. However, his influence was still so considerable that Mohammed counted him among those whose hearts must be won by generous gifts. He was, therefore, among those who received the largest share of the booty of Ḥunain, and one of those who refused to give any of it back to the unfortunate losers when these became Moslems. Distrusting this turbulent man, yet knowing his influence, Mohammed found it necessary to handle him with kid gloves.⁹¹

⁸⁸ Ibn Hishām, pp. 670, 719; Ṭabarī, I, 1463; Caetani, II, 55 f. and 119; cf. also for brief biographies, *Iḡābah*, III, 107-10; Nawawī, pp. 499 f.

⁸⁹ Ibn Hishām, p. 676; Muir, *Life*, pp. 307 and 312.

⁹⁰ Ibn Hishām, pp. 979 f.; Ṭabarī, I, 1557 f.; Wāqidī (Wüst.), pp. 236, 238 f.; Yaʿqūbī, II, 74 f.; Caetani, I, 700, 702.

⁹¹ Ibn Hishām, pp. 874, 877 f., 881, 983, 988; Wāqidī, pp. 422 f.

But ʿUyainah had never been a true Moslem at heart. We find him, therefore, among those who apostatized after Mohammed's death. On his side, he claimed to have known little peace and to have experienced constant uncertainty regarding his boundaries ever since Mohammed came on the scene. The Ghaṭafān had in the past been allied with the Banū Asad, and ʿUyainah now renewed this alliance. This meant that he made his own the cause of the pretender, Ṭulaiḥah ibn Khuwailid of the Asad. This he was not unwilling to do, preferring to have a prophet from among his allies than one from the Quraish; for Mohammed was, after all, dead and Ṭulaiḥah very much alive. The news of this alliance caused several tribes to fall away from Islam and several others to watch and keep close to the new allies. Abū Bakr dispatched his ablest general, Khālid ibn al-Walid, against them. At the ensuing Battle of Buzākhah, ʿUyainah and his seven hundred fighters soon discovered that God was not on the side of Ṭulaiḥah. They retreated. Ṭulaiḥah and his wife escaped to Syria and later both accepted Islam. ʿUyainah was taken captive and brought to Abū Bakr. Accused of apostasy he boldly exclaimed he was never a Moslem until then and thus won a pardon.⁹²

But not all of Ṭulaiḥah's party were willing to give up the fight. With their previous leaders deserting them, the small group of die-hards found a desperate and daring leader in none other than Salmā, cousin of ʿUyainah and daughter of the barbarously executed Umm Qirfah. Salmā, whom Mohammed managed to acquire from her captor in the affair of Wādī-al-Qurā, had been given to Aishah, whom she served for a time. Later she was married to a relative of Mohammed. She had gone over with ʿUyainah to Ṭulaiḥah and, remembering her mother's cruel fate, determined now to avenge it or die. To her banner flocked men of Fazārah, Asad, Hawāzin, Sulaim, and Ṭayy. Like her mother, she led her men in person, riding on her mother's camel. Around her the fighting was most severe, for Khālid had promised a hundred camels to him who should disable hers. The odds were heavily against her; she and her camel fell but not before a hundred others (*sic!*) had fallen around her.⁹³

⁹² Balādhurī, *Futūḥ*, p. 96; Ṭabarī, I, 1893, 1896; Yāqūt, *Geog.*, I, 601

⁹³ Ṭabarī, I, 1901-3; Yāqūt, *Geog.*, II, 353; *Sirat al-Hallabiyah*, III, 203-5; *Iṣṭabāh*, IV, 638 f., calls her Umm Qirfah the younger and makes her erroneously the granddaughter of Umm Qirfah the elder. Caetani (II, 623 f. note, and II, 811) questions this episode

It was this same period of the apostasy of the Year Eleven that brought the last of the Arab "queens" to the foreground. This was the prophetess Umm Ṣādir Sajāḥ bint Aws ibn Ḥiqq of the tribe of Tamīm. Her story is nowhere fully and consecutively told. We know nothing of her before her sudden appearance and little that is certain after that, while of the major episode itself in which she figures several versions are told.⁹⁴ Of these, two are more or less clearly defined. The one originating from the school of ʿIrāq and transmitted largely through Saif ibn ʿUmar, a fellow-Tamīmite living in the time of the ʿAbbāsīd Hārūn al-Rashīd, and supplemented by sundry notices, gives us a very ungratifying picture of Sajāḥ as a woman and as a politico-religious leader. However, this same Saif, thanks to Western scholarship, has been shown to be a notorious romancer whose great objective was the glorification of his tribe and the removal from it, if possible, of the guilt of apostasy.⁹⁵ As for the sundry notices, they bear every mark of wilful malice. According to this version, therefore, Sajāḥ was a Taghlibite and not a Tamīmite; she came from ʿIrāq and not from the Tamīm territory; she was a weakling whom the aged Musailamah, the false prophet, violated; she was a pretender impressed with Musailamah's second-rate and insipid if not vulgar utterances. In short, she was a weak woman and a false prophetess who eventually saw the true light and died a good Moslem.⁹⁶

The other and less biased version comes from the school of Madīnah.⁹⁷ Supplementing it with other notices and bearing in mind the general situation, it is possible to piece together the most probable story of Sajāḥ as follows. She was herself a Tamīmite, though her mother was of the Banū Taghlib, which tribe had long settled in southern ʿIrāq and was largely Christianized. In all probabilities Sajāḥ's religious ideas were influenced by those of her mother's

because it is received on the sole authority of Saif. However, it is difficult to see what motives Saif could have had for its fabrication. The elements in the story itself and Umm Ziml's backgrounds make the episode probable, though perhaps somewhat embellished by Saif.

⁹⁴ Caetani, II, 626-35, 644-48; II, 809-12, gives the best and most detailed account of Sajāḥ, which is largely followed in the brief notice of her in *EI*, IV, 44 f.

⁹⁵ Cf. *EI*, IV, 44, and Wellhausen, *Skizzen und Vorarbeiten*, VI (Berlin, 1899), 3 ff.

⁹⁶ Ṭabarī, I, 1908, 1911-19; *Aghānī*, XVIII, 166 f.; cf. Caetani, II, 628 ff.; *Fakhri*, pp. 104 f.

⁹⁷ Balādhurī, *Futūḥ*, pp. 99 f.; cf. Caetani, II, 626 f.

tribe, though there is nothing to make us conclude that she herself was a Christian. Some of the accounts tell us she was a *kāhinah*.⁹⁸ We have already seen the significant role a *kāhin* or *kāhinah* could and did play in heathen Arabia. *Kāhinah* or not, Sajāḥ must have already achieved some sort of leader's position with her people *before* the death of Mohammed, otherwise she could not have found such ready followers when, on Mohammed's death, she proclaimed herself a prophetess.

The politico-religious situation into which she plunged and on which she risked her political all was indeed a complicated and serious one. Mohammed was dead. Al-Aswad, Ṭulaiḥah, and Musailamah had come forth as prophets. This then was an age of prophets. A stout-hearted, ambitious woman, especially if she were already a *kāhinah*, would find the situation inviting, and into the arena of prophets would come a prophetess. Sajāḥ must have thought herself equal to the challenging situation.

Several of the divisions of the Banū Tamīm threw off the Moslem faith and cast in their lot with hers; among them were the Ḥanzalah subtribes with their leaders Wakī' ibn Mālik and Mālik ibn Nuwairah. Other divisions hesitated between Islam and apostasy. The Banū Tamīm were thus divided. This led to civil war among them, and Sajāḥ's faction lost two minor battles, after which some of her subtribes deserted. She was thus forced to make peace on condition she leave the Tamīm territory. This setback was not serious enough to cause her to forsake the call to prophesy. She would march against the prophet Musailamah and, having vanquished him, try to settle matters with Abū Bakr and the Quraish. For were not the Tamīm, like the Quraish, of the Maḍr Arabs? If prophecy can belong to the latter, it can also be the right of the former. The Quraish and Mohammed can stay in the Ḥijāz and northwestern Arabia; the Tamīm and Sijāḥ claimed the Najd and northeastern Arabia.⁹⁹

Sajāḥ and her army headed for Yamāmah, Musailamah's capital. Here, local rivalry and opposition to Musailamah had crystallized around Thumāmah ibn Uthāl, and Sajāḥ's advance came, therefore, at a most inopportune moment. Musailamah, taking council with his men, decided to meet her halfway and work out a peaceful arrange-

⁹⁸ Balādhuri, *op. cit.*, p. 99.

⁹⁹ Cf. *Aghānī*, XIV, 66; XVIII, 165 f.

ment. An interview was arranged, the result of which is variously told. It was at this point that the aged—he was said to have been a hundred and fifty years old—and distracted Musailamah is supposed to have assaulted the warring Sajāḥ, forced her into a dishonorable marriage, which he promptly repudiated, and then sent her humiliated, disgraced, and empty-handed back to her people in southern 'Irāq.¹⁰⁰ Utter nonsense! Another version states that at this interview Musailamah succeeded by his timely revelations in convincing Sajāḥ of his superior claim to prophecy and then proposed honorable marriage which she accepted. With this version would fit the tradition that Sajāḥ made common cause with Musailamah, with whom she stayed until the fall of Yamāmah.¹⁰¹ Some of the accounts, in addition to one or the other of the above versions of this interview, mention a treaty between Sajāḥ and Musailamah, according to which she was to withdraw her army in return for a year's revenue from Yamāmah. Half of this revenue was to be paid then, and the other half was to be turned over later to three of her generals whom she left behind for that purpose.¹⁰²

Were it not for Musailamah's extreme age and the mention of a treaty, the second version of the marriage might find some acceptance. As it is, the probabilities are in favor of the treaty alone. For, in the meantime, the die was being cast in favor of Quraish. Khālid had already made swift work of Ṭulaiḥah and perhaps also of Umm Ziml Salmā. Sajāḥ's own army was none too good or reliable. Musailamah's terms were generous. Sajāḥ might well have decided that under the circumstances discretion was the better part of valor. She accepted the terms and went home, not to the Tamīm territory from which she had been recently ousted but to her maternal uncles, the Taghlibs of 'Irāq. If she had any further claims of religious leadership and other ambitious plans of military conquest, the events of the succeeding months must have put an end to them. Khālid, the Sword of Islam, had swept like a scourge over the land. Mālik ibn Nuwairah, who had been her chief Tamīmite ally, was no more; and Musailamah, the last and most pretentious of the "false" prophets, had fallen in the

¹⁰⁰ *Aghānī*, XVIII, 166 f.; *Fakhri*, pp. 104 f.

¹⁰¹ Ṭabari, I, 1917-19; Ya'qūbī, II, 144; Abū al-Fidā, I, 208-12.

¹⁰² Ṭabari, I, 1919 f.; Ibn al-Athīr, II, 271.

disastrous Battle of 'Aqrabah. The balance of the revenue he had promised her was already in the hands of the conqueror who was, moreover, pushing further and further toward 'Irāq. Is it any wonder, then, that we hear no more of Sajāh, the warrior-prophetess? Did she stay with the Taghlib and become Christian? Or did she return to the Tamīm, now united once again in their faith, and, like them, become a Moslem? In the latter case she would be but following in the footsteps of Ṭulaiḥah. Most of the traditions tell briefly that she, in time, returned to the Tamīm, became a Moslem, and settled and died in Basrah, which had become the chief Tamimite city under Mu'āwiyah, who settled that tribe there in 41/661-62.¹⁰³

The Moslem traditions have preserved nothing for us of Sajāh's teaching. Only a few of her rhymed utterances have survived, and these are mostly orders to march to battle. Her deity is referred to as *Rabb-al-Sihāb*, "The Lord of the Clouds." She had her *mu'adhdhin's* or men that called the people to prayer; she was attended by a *ḥājib* or chamberlain and is said to have delivered her message from a *minbār* or pulpit.¹⁰⁴

Emerging from an almost complete obscurity, Sajāh, the warrior-prophetess, played a brief but major role on the political stage of Central Arabia and then stepped off that stage into an even more dense obscurity than that out of which she had first emerged. Yet, in her, the contemporary feminine counterpart of Mohammed the prophet-king, we see the last of that ancient and long though repeatedly broken line of independent Arab queens. The Arab lands that once produced the Queen of Shebah, the empress Julia Domna and the illustrious Queen Zenobia, will know no more independent and warring Arab queens. Henceforth a favorite "consort," an aggressive "queen-mother," or even an ambitious sister or aunt, will pull some political strings. A few of these will be so adroit at this performance that the political stage of their day will present us with nothing but a clever woman's puppet show. However, independent Moslem queens will emerge later, but they will not be the daughters of Arabia.

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¹⁰³ Tabarī, I, 1920; cf. Caetani, II, 648; Ibn al-Athīr, II, 271.

¹⁰⁴ *EI*, IV, 44.

PERSIAN AND EGYPTIAN CHRONOLOGY

RICHARD A. PARKER

Under this heading are here presented three studies in the chronology of Egypt during the first Persian domination. When Persian chronology itself is touched upon, it is, in the main, from the standpoint of the Egyptian evidence. Interest in these problems followed upon the invitation of Professor A. T. Olmstead to join in the work of his seminar in near eastern history for the year 1940-41.

I. THE ACCESSION DATE OF DARIUS I

Recent articles in this *Journal* by Professors Olmstead and Poebel have once again directed attention to the question of the accession date of Darius I.¹ The problem arises from the fact that two interpretations of the length of the reigns of Bardiya and Nebuchadnezzar III have been drawn from the dates by which Babylonian tablets are dated. Thus, if Bardiya ruled not seven months but a year and seven months, Darius' accession must be placed not late in 522 B.C., the usually accepted date, but in 521; and if Nebuchadnezzar III ruled not three months but nearly a year, it must be further dropped to 520, his first year, then, being 519/18.²

Consideration of the Egyptian data which bear on the problem has led me to the conclusion that the traditional date of 522 for Darius' accession is correct and that, no matter how one may be inclined to interpret the tablet material, it must be accommodated to that date. Decisive evidence for my conclusion is found in the double-dated Aramaic papyri from Egypt; but, before dealing with them, it may be appropriate to discuss briefly the other Egyptian material which has

¹ A. T. Olmstead, "Darius and His Behistun Inscription," *AJSL*, LV (1938), 392 ff.; Arno Poebel, "The Duration of the Reign of Smerdis, the Magian, and the Reigns of Nebuchadnezzar III and Nebuchadnezzar IV," *AJSL*, LVI (1939), 121 ff.; cf. also his earlier articles, "The Names and the Order of the Old Persian and Elamite Months during the Achaemenian Period," *ibid.*, LV (1938), 130 ff.; "Chronology of Darius' First Year of Reign," *ibid.*, pp. 142 ff. and 285 ff.

² It would not be possible to retain 522 for the date of Darius' accession and push back the reign of Cambyses by two years, for his seventh year is fixed astronomically by an eclipse of the moon to 523/22 B.C. (cf. n. 26 below).

been thought to be useful evidence. This last consists of the Apis stela of year 4 of Darius I, and one of the several Wadi Hammamat inscriptions of a Persian official, Atiyawahy.³

A. THE APIS STELA OF YEAR 4 OF DARIUS I⁴

This stela gives the dates of the birth, death, and burial of Apis and the length of his life. As he was born in the fifth year of Cambyses, Tybi 29, and died in the fourth year of Darius, Pachons 4, the length of his life ought to be significant. The number indicating this has been read as 7 years, 3 months, and 5 days by some scholars,⁵ but as 8 years, 3 months, and 5 days by others.⁶

The signs in question, with the *mp*-sign to the right, appear clearly as $\overset{\wedge}{|} | | |$. Borchardt says: "Die 7 besteht aus vier Strichen oben und drei, die auf Lücke stehen, darunter. Der schwache Strich in etwas grösseren Abstände links von diesen drei Strichen ist nicht beabsichtigt."⁷ This left lower stroke, however, does not appear to be at all "weak," though it is not so evenly spaced as the others. Posener states, without qualification: "La position de \wedge confirme la lecture 8."⁸ Why he should be so certain is not clear. The \wedge does not extend above the stroke in question, nor is it over the center of the eight strokes. He may perhaps mean that it is nearer a central position over the eight than the seven strokes, and a central position would be the objective in good Egyptian writing.

Furthermore, while not invariable, the normal writing of 7 with upper and lower strokes is $| | | |$; rarely it is $| | |$ or $| | | |$. Eight, however, is almost invariably $| | | |$.⁹ The eight strokes as arranged on the stela might thus be considered as an error for 7 (this must, of

³ Latest and best publication in G. Posener, *La première domination Perse en Egypte* (Le Caire, 1936), a valuable sourcebook for this period.

⁴ Full bibliography, translation, commentary, and photograph in *ibid.*, pp. 36 ff. and Pl. III.

⁵ Wiedemann, *Geschichte Ägyptens* (1880), p. 219; Borchardt, *Die Mittel zur zeitlichen Festlegung von Punkten der ägyptischen Geschichte und ihre Anwendung* (Kairo, 1935), p. 64. Wiedemann's reading is accepted by Poebel, "The Duration of the Reign of Smerdis . . .," *op. cit.*, pp. 128-29.

⁶ Revillout, *Notice des papyrus démotiques archaïques* (Paris, 1896), pp. 387-88; Chassinat, *Recueil de travaux*, XXIII (1901), 77-78; Gauthier, *Le Livre des rois d'Égypte* (Le Caire, 1915), IV, 138, n. 1; Posener, *op. cit.*, p. 39.

⁷ *Op. cit.*, p. 64, n. 3.

⁸ *Op. cit.*, p. 38 (m).

⁹ In *Medinet Habu III: The Calendar, etc.*, a text in which carelessly written figures abound, in addition to numerous examples written as above a somewhat hasty search revealed but one exception, $| | | |$ on Pl. 156, l. 867. Another was later pointed out to me by Dr. Keith Seele, $| | | |$ on Pl. 165, l. 1359.

course, be our eventual conclusion) or, possibly, as an unconventional rendering of 8, explained as a graver's correction of an original 7. As it stands, however, the stela must be considered quite indecisive as evidence.¹⁰

B. THE WADI HAMMAMAT INSCRIPTIONS OF ATIYAWAHY¹¹

Atiyawahy (*Atywhy*) was a $\overset{\wedge}{|} | | |$ *srs* of Persia and governor of Coptos who made numerous visits to the Wadi Hammamat quarries. Records of seven such visits have been found. One of them, No. 28, has been considered by Wiedemann¹² to give the lifetime of Atiyawahy as six years under Cambyses, thirty-six under Darius, and twelve under Xerxes—an assumption which, if correct, would demonstrate that Darius' first year followed the last of Cambyses without any interval. Posener, on the other hand, believes that this inscription refers to two different preceding visits and not to the lifetime of Atiyawahy at his last visit.¹³ In this he is certainly correct, as a consideration of the other inscriptions demonstrates. Let us list their essentials.

No.

24. Year 36 of . . . Darius . . .

25. Year 2, 1st month of the 1st season, day 19 of . . . Xerxes . . .

26. Year 6 of . . . Xerxes . . .

27. Year 10 of . . . Xerxes . . .

28. Year 6 of . . . Cambyses; year 36 of . . . Darius; year 12 of . . . Xerxes . . .

29. Year 12 of . . . Xerxes . . .

30. Year 36 of . . . Darius . . . ; year 13 of . . . Xerxes . . .

All the above are followed by the line: "Made by (*ir.n*) the *srs* of Persia, Atiyawahy." Wiedemann would understand all these inscriptions to record, not "Year x," but rather "X years," lived (made) by Atiyawahy.

¹⁰ Posener's objection (*op. cit.*, p. 40) to the use of this stela for chronological purposes, from the standpoint that Egyptian dating differed from Persian, will be met in Part III of this article. It may be said here, however, that it does not apply.

¹¹ Full bibliography in Posener, *op. cit.*, pp. 117 ff., his numbers 24 to 30. References are there given to photographs in Couyat-Montet, *Inscriptions du Ouadi Hammamat* (1912). For convenience I shall refer to the individual inscriptions by Posener's numbers.

¹² *Op. cit.*, pp. 220-22; quoted by Poebel, "The Duration of the Reign of Smerdis . . .," *op. cit.*, p. 129.

¹³ *Op. cit.*, p. 123.

Reading the initial signs, which are { $\overline{\circ}$, as *rnpt*, "year," instead of *hst-sp*, "regnal year," is a priori most improbable. We know from any number of other examples that inscriptions in the Wadi Hammat record a trip made to the quarries at a certain time by a certain official or group of officials. Such, for example, must be No. 25 above. To interpret that as "1 year, 19 days under Xerxes lived by Atiyawahy" would be an impossible strain on the Egyptian. This inscription merely records a date, a point in time and not a period of time, and so too must all the others, even though only to the year. The fact that Darius reigned thirty-six years and Atiyawahy visited the quarries in *year 36* need be nothing more than a coincidence. Again, if No. 28 were intended to record the life-span of Atiyawahy, why should Nos. 24 and 30, which also mention *year 36* of Darius, fail to include *year 6* (or six years) of Cambyses?

It must be admitted that the recurrence of *year 36* in later inscriptions is puzzling. Atiyawahy must have attached to it some special significance. One explanation of its prominence might be that it was his first official trip and that, as such, he commemorated it later. It is to be noted that during the earlier years of Darius we have inscriptions at the quarries made by an Egyptian director of work, Khnemibre, whose last date is in *year 30*.¹⁴ Atiyawahy would have taken over between then and *year 36*.

Year 6 of Cambyses, in No. 28, may commemorate another date in Atiyawahy's life which he thought important—perhaps his birth, perhaps a first trip as a boy. Posener has pointed out that, if he were twenty years old at the time, in *year 12* of Xerxes he would have been around seventy.¹⁵ This is a not impossible age, but it seems more likely that he was younger.

The Egyptian evidence we have thus far discussed has been seen to have slight value, but we may now turn to another body of material which will be found to have much greater significance.

C. THE ARAMAIC PAPYRI FROM EGYPT

Among the Aramaic papyri of the Persian period from Egypt are a number bearing double dates.¹⁶ It is now generally accepted that these

¹⁴ Posener, *op. cit.*, Nos. 11–23.

¹⁵ *Ibid.*, p. 123.

¹⁶ A. H. Sayce and A. Cowley, *Aramaic Papyri Discovered at Assuan* (London, 1906); A. Cowley, *Aramaic Papyri of the Fifth Century, B.C.* (Oxford, 1923).

dates are according to the Babylonian and the Egyptian calendars;¹⁷ this being so, they become most valuable data for checking Persian chronology.¹⁸ It has long been possible to convert Egyptian dates of this period into the Julian calendar with no error by the use of Eduard Mahler's *Chronologische Vergleichungs-Tabellen* (Wien, 1889). More recently, the same operation with Babylonian dates has become practicable, with a rare uncertainty of one day.¹⁹ For our purpose this uncertainty is insignificant.

As a hypothetical example, let us suppose that we have a papyrus dated "Abu 1 = Pachons 9, year 11 of King X," and we know that "year 11 of King X" might be any year from 456 to 452 B.C. In order to determine the exact year, we calculate the Julian dates for Abu 1 and Pachons 9 for all five years:

Abu 1 = August 11	Pachons 9 = August 20, 456 B.C.
" " = " 1	" " = " " 455
" " = " 20	" " = " " 454
" " = " 8	" " = " " 19, 453
" " = July 28	" " = " " 452

It is evident at once that 454 B.C. must be the correct year, and it is obvious that one day one way or the other in any of the Abu 1 dates would not invalidate this conclusion.²⁰

¹⁷ M. H. Pognon, "Chronologie des papyrus araméens d'Eléphantine," *Journal asiatique*, XVIII (10th ser., 1911), 337 ff.; Eduard Mahler, "Die Doppeldaten der aramäischen Papyri von Assuan," *ZA*, XXVI (1912), 61 ff.

¹⁸ Not until the writer had completed his work did he discover that he had been anticipated, both in his use of the Aramaic papyri and in his conclusions, by Professor Martin Sprengling in an early article, "Chronological Notes from Aramaic Papyri," *AJSL*, XXVII (1911), 233 ff. The fact that a second year following the traditional date must be considered, and our present much greater knowledge of the Babylonian calendar, justify him, he feels, in a new treatment of the subject.

¹⁹ The writer, in collaboration with Dr. Waldo H. Dubberstein, is now engaged in compiling tables of Julian dates for the beginning of every Babylonian month from 588 B.C. to the Christian Era. They are to be incorporated in a forthcoming study of the Neo-Babylonian calendar. The Babylonian dates in this article are based on this unpublished material, but they may be roughly checked, if desired, by the early tables of Eduard Mahler, *Zur Chronologie der Babylonier* ("Denks. d. kais. Akad. d. Wiss., math. naturw. Cl.," Vol. LXII [Wien, 1895]), pp. 641 ff., controlled by the dates for Nisanu 1 in D. Sidersky, "Contribution à l'étude de la chronologie néo-babylonienne," *RA*, XXX (1933), 57 ff.

²⁰ Other points to be considered are: (a) Babylonian days ran from sunset to sunset and Egyptian days from daybreak to daybreak, so that, if a papyrus were written in the evening, the Babylonian date would be one day later than the Egyptian. (b) The new moon was visible at Assuan forty-seven minutes earlier than at Babylon. As the new month began with the first appearance of the crescent, upon occasion a month could begin at Elephantine one day before it began at Babylon. Apart from all these considerations, mere scribal errors in dealing with two calendars are undoubtedly present.

Seven papyri have their dates sufficiently well preserved to be usable for our present purpose.²¹ They are tabulated according to the following scheme. The first regnal year worked out for each papyrus is the traditional date, calculated from the Ptolemaic canon, according to which the first years of the reigns of the six kings from Cambyses to Artaxerxes II are as follows:

	Years of Reign	Date of First Year (Not Accession Year)
Cambyses	8	529/28 B.C.
Darius I	36	521/20
Xerxes	21	485/84
Artaxerxes I	41	464/63
Darius II	17	423/22
Artaxerxes II	46	404/3

Following this traditional year are the Julian dates for the next two years, into which the given regnal year might fall according to the chronology resulting from a longer reign for Bardiya or Nebuchadnezzar or both.

1. Cowley 5 = Sayce and Cowley A

Ululu 18	=	Pachons 28	Year 15 of Xerxes
September 13	=	September 12/13	471 B.C.
October 2	=	" "	470
September 21	=	" 11	469

2. Cowley 6 = Sayce and Cowley B

Thoth = Dec. 19

Kislimu 18	=	Thoth 7 ^a or 14 ^a or 17 ^{1a}	Year 21, the beginning of the reign when King Artaxerxes sat on his throne ^b
January 3	=	December 23 30	January 2 465/64 B.C.
December 23	=	" " "	" " 464/63
" 12	=	" " "	" " 463/62

(a) Cowley states that there is hardly room for 17. This papyrus has been included in spite of the uncertainty of the Egyptian date because of the importance of the year dating, which shows that year 21 of Xerxes was also the accession year of Artaxerxes.

(b) As will be explained in Part III, the scribe normally used double-year dates after Thoth 1 until the Egyptian and Persian years again coin-

²¹ One other (Cowley 8 = Sayce and Cowley D) has its dates preserved, but they are demonstrably incorrect (cf. Mahler, *ZA*, XXVI [1912], 64).

ceded, after Nisanu 1. Thus we should expect here, "Kislimu 18, year 21 = Thoth x, year 22, the beginning of the reign, etc." However, for a long period up to one or two weeks before, the scribe had been accustomed to write only "year 21," and he may easily have forgotten the present necessity for a change, just as we ourselves frequently fail to write the correct year in January dates. Moreover, the scribe was undoubtedly confused by the fact that the accession year of the next king was involved.

3. Cowley 10

Kislimu 7	=	Thoth 4	Year 9 of Artaxerxes I ^a
December 14	=	December 18	456 B.C.
" 3	=	" "	455
" 22	=	" "	454

(a) Here again we should expect "Kislimu 7, year 9 = Thoth 4, year 10"; but see note (b) to No. 2 above.

4. Cowley 13 = Sayce and Cowley E

Kislimu 3	=	Mesore 10	Year 19 of Artaxerxes I
November 19 ^a	=	November 17	446 B.C.
December 7	=	" 16	445
November 27	=	" "	444

(a) Kislimu 3 in Babylonia was probably December 19, as the nineteenth year of Artaxerxes I should have a second Ululu (cf. the forthcoming study of the Neo-Babylonian calendar by the writer and Dr. Waldo H. Dubberstein). By this time the nineteen-year cycle with six Addaru's and one Ululu as intercalated months is fairly well attested, and the failure of the scribes in Elephantine to know or to recall that this particular year required an Ululu and not an Addaru can easily be explained by their isolated position geographically.

5. Cowley 14 = Sayce and Cowley F

Abu 14	=	Pachons 19	Year 25 of Artaxerxes I
August 27	=	August 26	440 B.C.
" 16	=	" "	439
" 6	=	" "	438

6. Cowley 25 = Sayce and Cowley J

Kislimu 3	=	Thoth 12	Year 8/9 of Darius II ^a
December 17	=	December 16	416 B.C.
" 6	=	" "	415
November 25	=	" "	414

(a) That is to say: year 8 of the Babylonian calendar is year 9 of the Egyptian.

7. Cowley 28 = Sayce and Cowley K
Shabatu 24 = Athyr 9 Year 13/14 of Darius II
 February 10 = February 10 410 B.C.
 " 29 = " " 409
 " 18 = " 9 408

The following table analyzes the results of our tabulation.

NUMBER OF DAYS BETWEEN BABYLONIAN AND EGYPTIAN DATES

	PAPYRUS NO.							TO-TAL	AVERAGE
	1	2*	3	4	5	6	7		
Traditional year	1	4	4	2	1	1	0	13	1 6/7
First following year . .	20	7	15	21	10	10	19	102	14 4/7
Second following year	10	18	4	11	20	21	9	93	13 2/7

* Taking December 30 as the Julian date.

Unless this analysis can be invalidated—and at present that possibility must be doubted—a most serious objection is here brought out to altering the chronology of Darius I. It is true that we have no double dates from his own time, but our papyri demonstrate that Xerxes' first year fell in 485/84 B.C., and, since we know from the evidence of the Babylonian tablets that Darius ruled thirty-six years, his first year must have been 521/20 B.C.

Now, it is true that the Babylonian dates as given above rest on tables which assume 522 B.C. as the accession year of Darius I, and one might argue that, if one year for Bardiya and one for Nebuchadnezzar III is to be inserted after Cambyses' eighth year, the table would have to be reconstructed from 522 onward. This argument, however, would lead us into considerable difficulties. In order to show this, let us construct a table of the nineteen years of the twelfth cycle after Nabunaser.²² This is a normal cycle with seven intercalated

²² The table is based upon Sidersky, *op. cit.*, p. 63, and *Etude sur la chronologie assyro-babylonienne* (Paris, 1916), pp. 29 and 38. The conventional system of nineteen-year cycles, beginning with Nabunaser, is here adhered to, although Kugler (*Sternkunde und Sternendienst in Babel*, II, Part II, Fasc. 2 [1924], 422 ff.) argues for no cycle before 528 B.C., an eight-year cycle from 528 to 505, a twenty-seven-year cycle from 504 to 383, and the nineteen-year cycle from 382 on. This is not the place for an elaborate examination of his argument, which is not completely convincing. The evidence now available supports the theory that the Babylonians, though perhaps without definite formulization, were following a

months, all attested by tablets.²³ There is no period of years longer than three from one intercalated month to another, and there are eight Nisanu 1 dates in March and eleven in April.

Now a most important point is that the Addaru II of the XVIIth year of the cycle is dated not to the eighth year of Cambyses but to

CYCLE XII

Year of Cycle	Regnal Year	Intercalated Month	Date of Nisanu 1
I	Cyrus 1	March 24, 538 B.C.
II	" 2	Ululu	" 12, 537*
III	" 3	Addaru	" 30, 536
IV	" 4	April 18, 535
V	" 5	" 9, 534
VI	" 6	Addaru	March 28, 533
VII	" 7	April 16, 532
VIII	" 8	April 5, 531
IX	" 9	Ululu	March 26, 530
X	Cambyses 1	April 12, 529
XI	" 2	" 1, 528
XII	" 3	Ululu	March 21, 527
XIII	" 4	April 9, 526
XIV	" 5	Addaru	March 29, 525
XV	" 6	April 18, 524
XVI	" 7	" 7, 523
XVII	" 8	Addaru	March 27, 522
XVIII	Darius 1	April 14, 521
XIX	" 2	" 3, 520

* This is an unusually low date, perhaps to be explained by the disorganization which must have accompanied Cyrus' conquest.

the accession year of Darius.²⁴ Hence, the insertion of a year for Bardiya and one for Nebuchadnezzar III, before Darius, would re-

nineteen-year cycle prior to 382 simply on the basis of intercalating a month whenever the calendar required it. One has only to refer to Kugler's table (*op. cit.*, pp. 424-25) to see that from 546/45 to 511/10 there are two nineteen-year cycles which coincide exactly, overlapping all but a small part of his three postulated eight-year cycles. However, even on the basis of eight-year cycles in the period from 528 to 505, it may be categorically stated that an analysis similar to that presented above on the basis of a nineteen-year cycle would result in the same conclusions.

²³ I am indebted to Dr. W. H. Dubberstein for checking this material. The references are given in Sidersky, *Etude*, p. 29, and Kugler, *op. cit.*, Vol. II, 412. The three Ululu's are not unusual in this early cycle.

²⁴ Three tablets, one of which requires an emendation from Ululu to Addaru, are listed by Poebel, "The Duration of the Reign of Smerdis," *op. cit.*, p. 134.

quire that this Addaru II be lowered to the XIXth year. Thus we should have to correct the above table as follows:

XVII.....	Cambyzes 8	March 27, 522
XVIII.....	Bardiya 1	" 16, 521
XIX.....	Nebuchad- nezzar 1	Addaru	" 5, 520

At once we can see two important objections: (a) there is a period of five years from one Addaru to another, which, to my knowledge, is not provable in any other cycle; (b) Nisanu 1 is seen to begin March 16 and March 5. Sidersky has tried to demonstrate that Nisanu 1 usually fell on or after the equinox, and only rarely, and by few days, before it.²⁵ His own later tables do not bear him out completely; but, apart from the exceptional dates of March 11 in the forty-first year of Nebuchadnezzar and March 12 in the second year of Cyrus, the usual dates, when Nisanu 1 falls in March, are in the last third of the month.

Furthermore, besides lowering Nisanu 1 in the XIXth year of the twelfth cycle to March 5, we should also have to lower every date in the following thirteenth cycle (for which we have seven attested intercalated months) by twenty-two days.²⁶ This would result in the following series:

I	March 23	VIII	March 7	XIV	March 29
II	" 12	IX	" 25	XV	" 18
III	" 1	X	" 14	XVI	" 8
IV	" 20	XI	" 3	XVII	" 26
V	" 8	XII	" 22	XVIII	" 15
VI	" 27	XIII	" 10	XIX	" 5
VII	" 17				

Not one date falls in April, which seems an impossibility for the Babylonian calendar.

But, it may be argued, we need only to intercalate another month somewhere in the five-year period between Addaru's to make everything satisfactory. To this procedure there are three objections: (a)

²⁵ *Etude*, pp. 70 ff.

²⁶ It should be stated that we can be quite certain of the Nisanu 1 dates* or this and the preceding cycle because of an eclipse of the moon which occurred in the seventh year of Cambyzes. A record of this has been preserved in Ptolemy's *Almagest* (iv. 14) and in a Babylonian tablet (Strass. *Camb.* No. 400). The latter dates the eclipse to the night of the fourteenth of Duzu, which agrees with the astronomical calculation of July 16, 523 B.C., 39 minutes before midnight (cf. Kugler, *op. cit.*, I, 61 ff.). Accordingly, we must date Nisanu 1 of Cambyzes' seventh year to April 7, 523. From this fixed point we can calculate forward and backward in the twelfth and thirteenth cycles, for which we possess certain knowledge of the intercalated months.

in a period when tablets are plentiful we have no evidence for such a month; (b) we should then have a total of eight intercalated months in the twelfth cycle, which a priori is at least most improbable if not impossible;²⁷ and (c) we should have to raise all the dates of the thirteenth cycle by eight days, giving this series:

I	April 22	VIII	April 6	XIV	April 28
II	" 11	IX	" 24	XV	" 17
III	March 31	X	" 13	XVI	" 7
IV	April 19	XI	" 2	XVII	" 25
V	" 7	XII	" 21	XVIII	" 14
VI	" 26	XIII	" 9	XIX	" 4
VII	" 16				

These dates, with only one in March, seem as improbable as the previous series.

It is unnecessary to go through the same process to check the result of dropping the Addaru II of the XVIIth year by only one place, to the XVIIIth year of the twelfth cycle. The reader can easily see that lowering all the Nisanu 1 dates of the thirteenth cycle by eleven days, or raising them by nineteen, would be equally unsatisfactory.

Our conclusion should be, therefore, that we can neither intercalate another month in the twelfth cycle nor move the Addaru II of year XVII to a later year. Thus we can feel certain of the Julian equivalents of the Babylonian dates in the Aramaic papyri; and, in proving that, we have demonstrated that the Addaru II of Darius' accession year must be placed in the XVIIth year of the twelfth cycle, or 522/21 B.C., and his first year was therefore 521/20 B.C.

II. THE DATE AND RECONSTRUCTION OF A PAGE OF THE "DAY-BOOK" OF THE MEMPHIS ARSENAL

In 1931 N. Aimé-Giron published a number of Aramaic texts, among which were some papyrus fragments found by Firth at Saqqarah in 1926.²⁸ Bearing the numbers 5 to 24 in his publication, they are said by him to constitute parts of a "day-book" kept at the Memphis arsenal or dockyard. Although the larger fragments frequently bear parts of dates, both Egyptian and Persian, no king is

²⁷ It is true that Pohl, *NRu*, I, No. 20, gives an Addaru II to the second year of Nabu-naid, making eight for the eleventh cycle, but this must be either a copyist's or scribal error, as we should then have three Addaru II's in successive years, and four in one five-year period, bringing a Nisanu 1 back to May 7—all quite unlikely. Furthermore, it would be impossible to attribute the extra month of Cycle XII to the following cycle, for we have seven attested months for that cycle.

²⁸ *Textes araméens d'Égypte* (Le Caire, 1931), pp. 12 ff.

ever mentioned, and the editor demonstrates no date for their composition.

Study of the fragments has led me to the conclusion that they can be dated with certainty to the reign of Xerxes, in a period from October 25, 472 B.C., to May 17, 471. Moreover, at least five of the fragments—Nos. 10, 11, 13, 14, and 15—once formed the larger part of a single page²⁹ of the "day-book." The method, based upon the fragmentary dates, by which this page was reconstructed need not be detailed here, but the correctness of the reconstruction requires demonstration.

As can be seen in Plate IV, the recto of the page bore two columns, of which approximately half of column A is missing on the right. The verso, Plate V, apparently bore but one column. Four dates are preserved, in whole or in part, and we may tabulate them as follows:

Recto A 9 = . . . [Choiak]k year
 B 3 11 (+x) year 14 = 3 Choiak, year
 B 11 23 Addaru, ye[ar XII]II = 8 Choiak, year
 Verso C 3 24 Addaru, year [XII]II = 9 Choiak, year 15

There is no question of year 14 (Persian) being correct in C 3, for, whenever two years are mentioned, the Persian is one less than the Egyptian.³⁰ The sequence of tabulated dates thus conclusively proves our arrangement of the fragments. No doubt, the still large holes might be filled in to some extent with the smaller fragments, but without the physical evidence of the papyrus as a guide that would be a hazardous venture.

We can now restore with certainty Addaru 18, in B 3.³¹ Also in A 9 the date must have been Addaru 16 or 17 = Choiak 1 or 2. In the reign of what king did Addaru 16, year 14, equal Choiak 1, year 15? Four kings are possible; and we tabulate the results for each, arrived at as explained in Section I, C, of this article.

<i>Addaru 16, year 14 = Choiak 1, year 15</i>	
Darius I	March 26 = March 28, 507 B.C.
Xerxes	" 18 = " 19, 471
Artaxerxes I	" 26 = " 14, 450
Darius II	" 21 = " 3, 409

²⁹ This page could, of course, be merely the end of a long papyrus roll. Elsewhere in this issue (pp. 302 ff.) Dr. R. A. Bowman discusses the restored text.

³⁰ Cf. Sec. III of this article. ³¹ The editor read 17(?), *op. cit.*, p. 32.

Artaxerxes I and Darius II may be immediately eliminated, but at first glance it would seem difficult to decide between Darius I and Xerxes. The following considerations make a decision easier. We are concerned with a governmental archive which has frequent entries—a record which should offer little opportunity for casual errors in double-dating. It is easily possible to account for a lack of agreement by one day, but a two-day interval would require a somewhat Procrustean explanation.

In Xerxes' fourteenth year Tebetu began on January 4, 471, Shabatu on February 2, Addaru on March 3; Nisanu of his fifteenth year began on April 2.³² This means that both Tebetu and Shabatu were months of twenty-nine days. Now it is extremely doubtful that any other aid than actual observation of the crescent was in use in Egypt for the determination of the month's beginning; and, if the evening of the twenty-ninth day of Shabatu were cloudy at Memphis, it is quite possible that the crescent would not be seen and that the month would be given another day, especially as the preceding month had but twenty-nine days. This would result in Addaru's beginning on March 4, and Addaru 16 would then be March 19, in complete agreement with the Egyptian date of Choiak 1.

Addaru in Darius' fourteenth year began, by astronomical calculation, on March 11. Even if the preceding month had but twenty-nine days, under no circumstances could the first day of Addaru be delayed more than one day. The king of these fragments, then, can be only Xerxes.

We can now date other fragments and list all of them in their chronological order. The earliest is No. 8, and it should be noted that the verso was written before the recto.³³

8 v. 19 Tash[ritu]	= [11 Epiphi, year 14] = Oct. 25, 472 B.C.
20 Tashritu	= [12 Epiphi, year 14] = Oct. 26, 472
8 r. [27 Tash]ritu	= 19 Epiphi, [year 14] = Nov. 2, 472
5 r. [6 Addaru, year 14]	= 21 Athyr, [year 15] = March 9, 471

³² These dates have all been calculated astronomically by the use of Schoch's new moon tables in Langdon and Fotheringham, *The Venus Tablets of Ammizaduga* (London, 1928).

³³ The assumption is that the editor named as recto the side with the horizontal fibers uppermost. While it was generally true that the Egyptian scribe wrote first on this side, there are numerous examples where the opposite is true. The year must also be 14, not 15, as the editor proposes, because in year 15 of Xerxes, Epiphi 19 could not have fallen in Tashritu.

Now come the dates on the reconstructed page, given in the table above:

A 9 =	March 19 or 20, 471	
B 3 =	" 21, 471	
B 11 =	" 25, "	
C 3 =	" 26 "	
18 r. 30 Nisanu ²⁴	= [14 Tybi, year 15]	= May 1, 471
12 r. [x Nisanu or Airu]	= [x Ty]bi, year 15	= April 18–May 17, 471 (Airu begins May 2).

III. PERSIAN AND EGYPTIAN METHODS OF DATING

Through our discussion of the Aramaic papyri from Egypt we have become aware that there was not complete accord between Egyptian and Persian dating. The names and usually the days of the months differ, and some of the papyri record two regnal years—always, it should be noticed, consecutive. We have now to inquire into the reasons underlying this double dating.

Under the Twenty-sixth Dynasty the regnal year coincided with the civil year,³⁵ which began with the first day of the first month of the first season, in Persian times and thereafter called Thoth 1. That portion of the civil year which remained after the death of a king was counted as *year 1* of his successor.³⁶ According to the Persian method of dating adopted from the Babylonians, the regnal and civil years also coincided, beginning with Nisanu 1, but the unexpired part of the civil year after a king's death was called the *accession year* of his successor. It should perhaps be further explained that the Egyptian year consisted of twelve months of thirty days each with five epagomenal days added at the year's end, while the Babylonian year was made up of twelve or thirteen lunar months of twenty-nine or thirty days each.

As we shall demonstrate, the Egyptians in part accommodated their dating to that of their Persian overlords as follows: (1) they kept

²⁴ Astronomically, Nisanu had but twenty-nine days, a clear indication that *observation* of the crescent was relied upon.

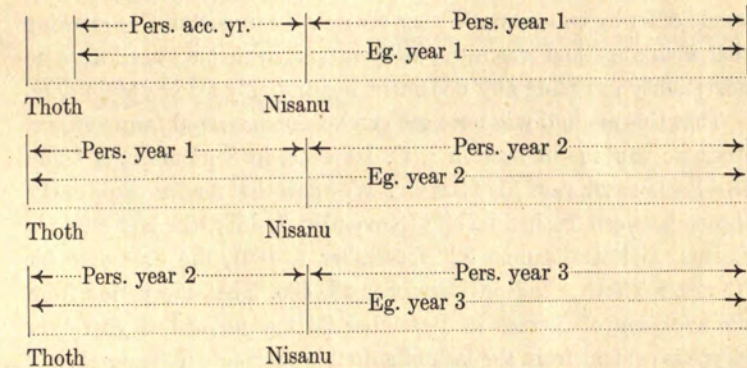
³⁵ This was also true of the Middle Kingdom. In the Eighteenth Dynasty, however, and probably in the following dynasties, regnal years were counted from the day of accession, independently of the civil year (cf. Eduard Meyer, *Aegyptische Chronologie* [Berlin, 1904], pp. 187 ff.).

³⁶ Thus, e.g., from the reign of Psammetichus III (only six months according to Manetho and Herodotus) we have a demotic papyrus dated to the month of Tybi, year 2. His first "year" could, therefore, have been at most no longer than one and a fraction months, as Tybi is the fifth month of the year (cf. Spiegelberg, *Dem. Pap. Strassburg*, No. 2, p. 15).

Thoth 1 as their date for beginning regnal years, but (2) they adopted the principle of the *accession year* whenever the new king came to the throne after Nisanu 1 and before Thoth 1.

The first is easily proved from the double year-dates in the Aramaic papyri.³⁷ In 525 B.C. Thoth 1 fell on January 2 (Julian). A hundred years later, since the Egyptian year was 365 days and thus lost a day every four years, it came on December 8, 426. Babylonian Nisanu 1, however, always fell near to or shortly after the spring equinox. Thus any given regnal year of a Persian king began in Egypt three to four months before it began in Babylonia. Any Aramaic papyri written in this period would bear two year-dates, with the Egyptian always greater by one than the Persian. After Nisanu 1 the years would again coincide and be written only once.

The demonstration of the second proposition rests on the fact that such double year-dates are not found except in the three to four months' stretch of the Egyptian year prior to Nisanu 1, and that these double dates are always of consecutive numbers. To clarify this point, let us illustrate the usual Twenty-sixth Dynasty dating for a Persian king who came to the throne after Thoth 1 but before Nisanu 1. Over a three-year period we would have the following:



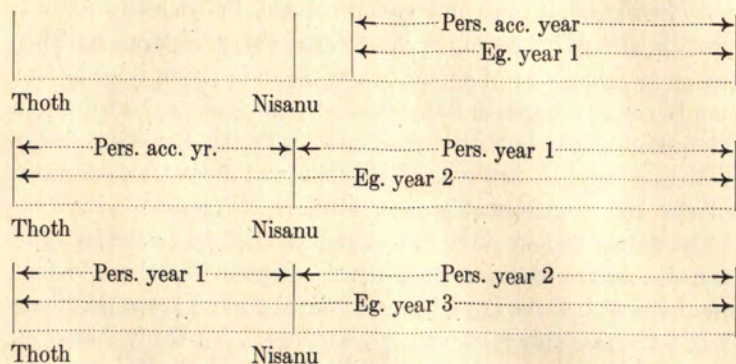
The accession of Darius II would fit such a dating situation, for he came to the throne in early February, 423 B.C.³⁸ Thoth 1 came on

³⁷ No double dates for this period have yet been noted in any document written in Egyptian.

³⁸ The last recorded date for Artaxerxes I is Shabatu 17, February 26, 423 (BE, IX, No. 109), while the first for Darius II is Shabatu 2, February 15, 423 (BE, X, No. 1). I owe the information for this and the following footnote to Dr. W. H. Dubberstein.

December 7, 424, and Nisanu 1 on April 11, 423. Correctly, therefore, papyri Nos. 6 and 7 in Section I, C, above give double year-dates for Thoth 12 and Athyr 9.

Now let us assume that the usual dating was also followed for a king who came to the throne after Nisanu 1 and before the next Thoth 1. Our diagram would then be this:



It is at once clear that the two regnal years would never coincide and that there would be a three- to four-month period yearly in which they would differ by two years. We can see now the importance of knowing that such a method was never in operation, so that we need have no uncertainty in dating any document bearing only an Egyptian date.

That this method was not used can be demonstrated from our first Aramaic papyrus in Section I, C. Its date, in September, is Ululu 18 = Pachons 28, year 15 of Xerxes. We know that Xerxes came to the throne between Tashritu 27(?) (November 7, 486), the last date for Darius, and Arahsmnu 22 (December 1, 486), the first date for Xerxes.³⁹ Thoth 1 fell on December 23, 486. That the period from the accession of Xerxes to December 23 was not called year 1 in Egypt is evident from the lack of a double year-date in September of his fifteenth year. Confirmation is obtained from II above, where a date of year 14/15 in Addaru/Choiak, after Thoth and before Nisanu, can fit only Xerxes.

It cannot be argued that the news of the accession of Xerxes might not reach Egypt until after Thoth 1 and that the circumstances,

³⁹ V.A.S., IV, No. 180; V, No. 117.

therefore, would be the same as those of Darius II. Even though the news might be late, the date of accession would certainly be known, and Egyptian dating would be based on it. It seems quite clear, therefore, that the Egyptians consciously adjusted their dating to avoid any such situation as outlined in the second diagram above.

While we are thus sure of the accuracy of dates from Darius I⁴⁰ on, there remains some uncertainty for Cambyses, arising from the fact that two dating methods came into use after the conquest. According to the first, the six-month reign of Psammetichus III was disregarded and Cambyses' year 1 was considered the balance of the civil year after the death of Amasis (526 B.C.). In his second year, therefore, Cambyses actually conquered Egypt, and in his fifth year he died.⁴¹

In the second method employed the Persian regnal years were taken over and applied retroactively to Egypt, wiping out not only the reign of Psammetichus III but also the last four years of Amasis.⁴² It can be seen, then, that any date falling in years 1-5 of Cambyses might be off three years, and other data must be used to determine it accurately.

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⁴⁰ We have no double-dated papyri from his reign, but the Apis stela discussed in Sec. I, A, gives us assurance. Had the balance of Cambyses' eighth year been counted as the first of Darius, the life of Apis could have been only six years, and there is no justification at all for reading the disputed numeral as 6.

⁴¹ This dating is found in *Ryl. Dem. Pap.*, No. IX, written in the ninth year of Darius (cf. Griffith, *Ryl.*, III, 106).

⁴² Cf. *Cairo Dem. Pap.*, No. 50059 (Spiegelberg, *Catalogue général ... du Caire. Die Demotischen Denkmäler*, III, 42 ff.), which refers to year 2 and year 8 of Cambyses.

AN ARAMAIC JOURNAL PAGE

RAYMOND A. BOWMAN

At the request of Richard A. Parker, who has reconstructed and dated a page of papyrus from several fragments of a series which Aimé-Giron had already suggested must somehow belong together as a journal of the Memphis arsenal,¹ I have prepared this reading of the reconstructed text (Pls. IV-V).

These badly damaged fragments of text are somewhat unique in having their most legible readings in the date formulas which epigraphers from experience usually expect to find most difficult. Aside from the date lines, the text is in very poor condition. Portions are so mutilated that only isolated letters or even traces of letters are discernible, and gaping holes make it almost impossible to gain an intelligible context.

Under such circumstances it is difficult to improve on the usual careful work of Aimé-Giron and almost impossible to glean more than he has from them. However, the demonstration that the pieces can be read when the fragments are put together as Parker has done it, by matching lines and by joining the legible date formulas, can be regarded as a contribution in advance of what Aimé-Giron has done. Then, too, my independent readings of the photographs of these fragments sometimes differ from those already proposed. Often, it is true, these differences depend upon an alternative interpretation of very mutilated letters, but these readings might throw light on some of the more legible sections of text and, if sound, might affect Aramaic lexicography.

TEXT²

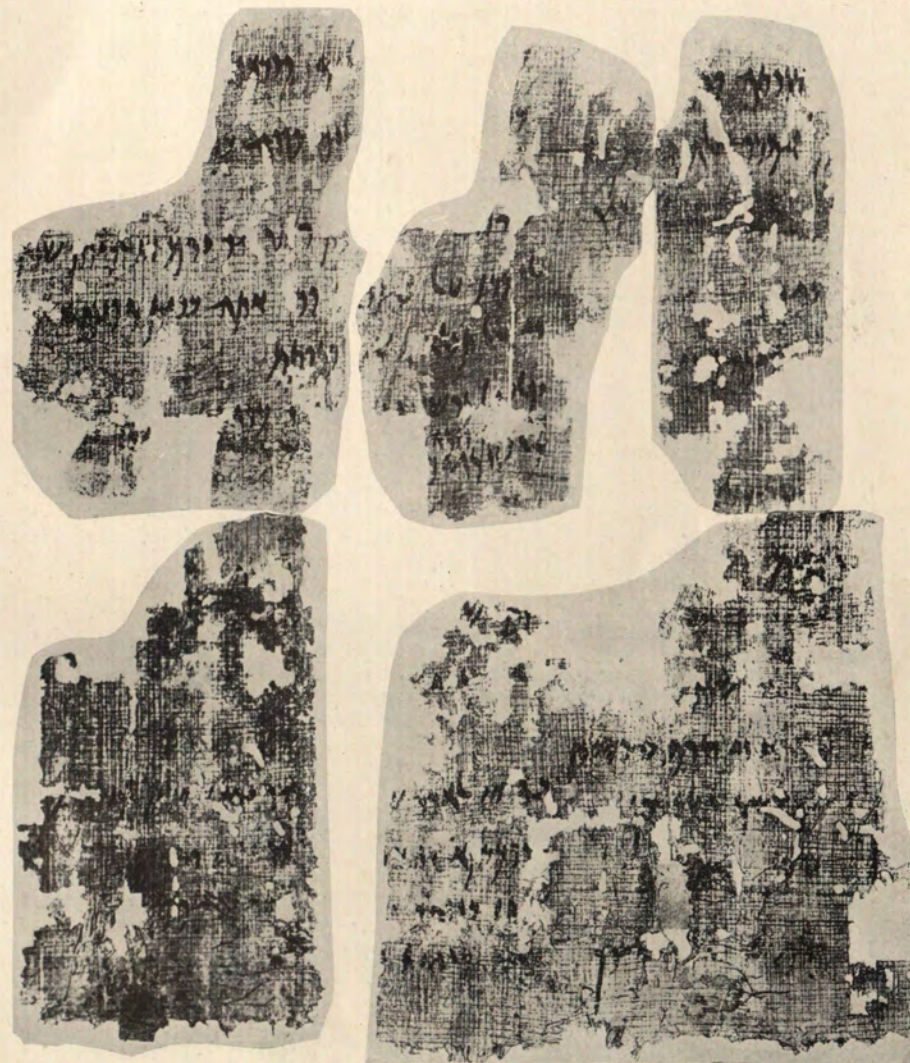
COLUMN A, RECTO

- ךן כנחה פרמננריא (1)
- ךן אמרו עלה סויןמך III (2)

¹ Cf. p. 295 above. N. Aimé-Giron, *Textes araméens d'Égypte* (Cairo, 1931), pp. 12 ff. (hereafter abbreviated "A-G").

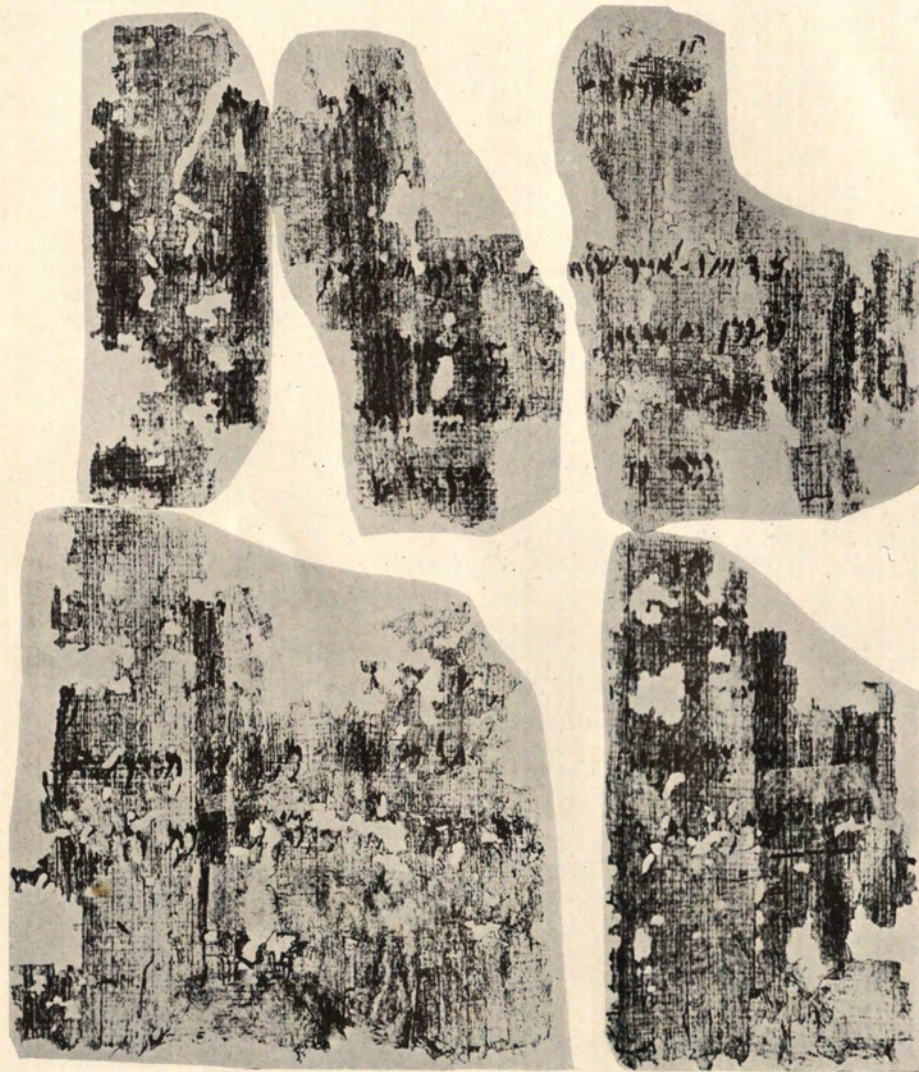
² I attempt to indicate as closely as possible the approximate state of the text. Letters about which there is some doubt are dotted, those even more doubtful are dotted in

PLATE IV



AN ARAMAIC JOURNAL PAGE

PLATE V



AN ARAMAIC JOURNAL PAGE

- (3) | בנר
- (4) | בנפ[ר]ן יד
- (5) | ספ[י]נה I זי ת[נ]נה
- (6) | יא כל II
- (7) | כדי שים טעם]
- (8) |]
- (9) | ב [אדר שנה I III X] הו יום ... לכיח[ק] שנת II III X
- (10) | ת . מלא זי קדם בנר III שבן
- (11) | מ[צ]רין יעבדון
- (12) | בל ... יא . . ה[ל]ל
- (13) | [הו] יום III [III] II [לכ]יח[ק]

COLUMN B, RECTO

- (1) | ליד חרמ[ח] י בר א
- (2) | ליד פחה בר ב
- (3) | ב [אדר שנה II III II] IX [II III X] שנת I III X] הו יום III לכיח[ק] שנת
- (4) | לזכר[ו] על בנפ[ר]ן י ב[ר] פר . אתה בבית ספ[י]נה לא
- (5) | זי על תבלא פנפתם
- (6) | צל[צ]ל נוש ר ברה
- (6) | ב[ר]ן חנה י I
- (7) | כל III נגיד[ו]ן]
- (8) | [ל]ם ז י
- (9) | [כ]פם ש[] של[ם] [ז]י נג
- (10) | א [נ]תן יר[]]

brackets; traces of letters are indicated by the mark o. Conjectural restorations are undotted but are in brackets.

Dots are used to indicate the approximate number of spaces or letters of the text where none remain, where such spaces can be computed at the beginning of or within lines.

A thin vertical stroke interrupts the text in each line where fragments are divided.

X נת 11 ב XX III לאדר ש[נת X III] I הו יום II III II לכוך ש[נת X
 II III
 12 [א]ופכרתא די ע[ב]דר [ל]אנ[ו] [א]וס[ב]ית[א]
 13 כ[ן] [ת]ן בה[ו] [א] מ[ב]י
 14 [ו] דיגית [ו]

COLUMN C, VERSO

1 | הו
 2 עבדצדק די [על] יא ס מ[י]א כא
 [—]
 3 ב XX III לאדר שנת II III II הו יום III III III לכוך שנת II X
 II I]
 4 בגפר די על נוזין בל שת[י]בוך ל כ ב
 5 צלצל די [ב] [ח] מרא [ל] על
 6 ומי בר[ו] ש[ר]ן [ן] נ[ו]פ[ו]ת
 7 [ת] הו |
 8 מך על ב[ו]ת[ו] [פ] ש פן [ש]ב [פ]
 9 [א] [ב] [ו] [ב] [א]ן [ו] [ב]
 10 בר כמשפלט[ו] [א] ל[ד]גל ו[י]וד[ו]ת די ממשפ[ר] עם מודכשראצ[ו]
 11 בב[י]סא די ס[ו]ר[ו] קדם קדם קדם

TRANSLATION

COLUMN A

1. his companions the supply officers
2. thus they said, "Simak(?) went up (the river?) 3 (days ago?)
3. within
4. Bagaphernes hand of(?)
5. 1 boat which is h[ere]
6. the total 1 (plus?)
7. [when] a decree [was made]
8. (only traces)
9. [On the of Addaru, year 14, that is the day of Choiak] year [15]
10. the which preceded amidst 3
11. Egyptians will make
12. (only scattered letters)
13. [that is] day 8 (or 9?) [of Choi]ak

COLUMN B

1. through the agency of(?) Hormaḥi
2. through the agency of Paḥah son of
3. On the 18th day of Addaru, year 14, that is, the 3d day of Choiak, year [15]
4. for a memorandum concerning Bagaphernes son of He went into the boathouse to [inspect a boat?]
5. which was on the dry land Pa-niptēm
6. a ship(?)
- 6(bis)
7. Total 3 keels(?)
8. [Meshu]llam who
9. [according to] the instruction of Sh Shelem[ia]h who
10. (scattered letters)
11. On the 23d of Addaru, ye[ar 14], that is the 8th day of Choiak ye[ar 15]
12. the substructure(?) which they m[ade] the .sbyt
13. (scattered letters only)
14.

COLUMN C

1. one(?)
2. Abdzedeq who is [in charge of] the the water(?)
3. On the 24th of Addaru, year 14, that is the 9th day of Choiak, year 15
4. Bagaphernes who is in charge of cordage (or sails?) Shethrabuzana(?)
5. the boat(?) which they [our?] lord who(?) is in charge of(?)
6. son of
7. he(?)
8. from(?) over the hou[se] of the boats?
9. Belonging to the company of
10. son of Chemosh-peleṭ the, belonging to the company of Vayazdata(?) of Memphis, along with Marduk-šar-ušur
11. in the wooden bowl(?) which before the [whi]ch is before

NOTES³

COLUMN A

Line 1 (A-G, No. 14, recto, l. 1)
 The length of the line is determined by line 9 of this column which is assumed to be a new date entry. The number of spaces available is approximated by counting line 3 of column C (the verso), the most complete date line.
³ The system of transliteration used is that proposed by A. A. Brux, "A Simplified System of Hebrew-English and Aramaic-English Transliteration," *AJSL*, LVIII (Jan-

The conjunction *w* supplied by A-G before *knwth* is not found in the photograph, and there is no room for it. The first traces of a letter show, by its extended staff, that it is *t* or *k* and not *w*. The last word, restored by A-G on the basis of occurrences in the shipbuilding papyrus of 412 B.C.,⁴ is convincing, but I have translated as "supply officers" rather than "commanders" because their function in the shipbuilding papyrus was that of ordnance officials who inspected equipment and ordered repairs.

Line 2 (No. 14, r., l. 2, and No. 15, r., l. a1)

Instead of the *lh* of A-G we must read *lh* (for there are definite traces of *c* and the space between words would be too great without it), which could be only the verb "he went up," although it is used nowhere else in the papyri. The rest of the line is very obscure. One expects a proper name after the verb. The first darkened traces suggest the head of *s* with a short staff.⁵ The next letter is a small one without a staff, probably *y*. This is followed by a definite *m* and the bottom of a long thin staff which almost intrudes into the line below. A-G read it as *n*, which cannot be entirely excluded, but the traces favor a *k*. A name fitting the traces here, *symk*, appears in a papyrus of 418 B.C.⁶ Smudges that follow are difficult to read. The last blur has been identified as *ḡ* but the thick parallel strokes suggest the figure 2 or possibly 3. The meaning of such a cryptic figure is not clear. It may be a reference to a journey up the river on the "third" day of the month.

Line 3 (No. 14, r., l. 3, and No. 15, r., l. a2) and line 4 (No. 14, r., l. 4)

Obscure traces in line 3, but only the last word is clear. Only one word is clear in line 4, but uncertain traces suggest that the line was once completed.

Line 5 (No. 14, r., l. 5)

A-G reads only *spynh* . . . , which seems certain although the first letter is almost completely lost. A single thick stroke follows this word, then a trace of *z*, a complete *y*, and finally what seems to be *t*. The single stroke is apparently a figure. The practice of writing the numeral for the indefinite article is illustrated by Cowley.⁷

The space following this line is blank for a single line. Since the text of this column preserves only the ends of lines, one cannot determine whether this blank is merely the end of a short line or a deliberate dividing space.

Lines 6 (No. 14, r., l. 7), 7 (No. 10, r., l. 1), and 8 (No. 10, r., l. 2)

uary, 1941), 57 ff. In order to reproduce the unpointed text, however, each character, consonant or vowel, is written in the consonantal value.

⁴ A. E. Cowley, *Aramaic Papyri of the Fifth Century B.C.* (Oxford, 1923), No. 26, ll. 4 and 8.

⁵ Cowley, *AP*, No. 5 (471 B.C.), l. 16. Cf. the *s* in the name *mḥsh* in the plate for this papyrus in A. H. Sayce and A. E. Cowley, *Aramaic Papyri Discovered at Assuan* (London, 1906), Papyrus A, Pl. 2, l. 16.

⁶ Cowley, *AP*, No. 22, l. 27.

⁷ *Ibid.*, No. 5, l. 4 (471 B.C.) and elsewhere.

Line 9 (No. 10, r., l. 3)

Apparently a date line introducing a new entry, as in lines 3 and 14 of column B and line 3 of column C. If this be so, one may then approximate the width of this column by reference to those more complete lines.

Line 10 (No. 10, r., l. 4)

A-G reads] *mḡ zy qdm bywm-št*, which he understands to say *mḡ* which "arrived" in the "sixth day." As far as *zy*, the line is very badly damaged. On the edge are traces of what might be *t*. The *l* and *ḡ* are rather certain. The second letter of *qdm* looks like *w* but may be a poor *d*. As indicated above, this is regarded as a verb by A-G, who says: "Qdm doit être considéré ici comme un verbe, probablement au *pael* avec le sens de 'se présenter, arriver.'" *Qdm* does occur in the papyri as a verb (in Cowley, *op. cit.*, No. 82, l. 6) in the sense "to be presented" and in the Targums in the sense "to precede," "to be early."

Immediately after *qdm* the letters get smaller and crowded, as though attempting to avoid the next column. It would seem that the smaller letters are a later addition to the text, like the interlinear letters in B6 bis.

It is impossible to read *bywm* here, for the second letter is definitely *g*, and what would have to be read *m* would have no counterpart in the papyri.

The *št* read by A-G seems possible but would be unusual for a numeral here where strokes are ordinarily used. The best reading is *šbn*, although *šnn* with the second *n* short and curved cannot be excluded.

Line 11 (No. 10, r., l. 5)

A-G reads] *o o bmsryn yḡdun*. The *m* is quite probable, but the *b*, read as clear by A-G, cannot be identified. The reading *yḡdun* is certainly preferable to his alternative reading *yḡbqun*.

Line 12 (No. 10, r., l. 6)

I have been tempted to read *bbbl*, "in Babylon," but the second *b* is improbable. What has been read as *h* might also be *b*, which seems to be followed by traces of two more letters of which only the tip of *l* suggests anything. A-G ignores these last traces.

Line 13 (No. 10, r., l. 7)

A-G reads] *p om oo [k]yḡk*. Since it is probably correct to read the month name at the end of this line, one may look for other signs of a date formula. This I see in *ywm*, "day," as in column B, line 3. If this be correct, the space before the month name is enough for eight or nine strokes.

COLUMN B

Line 1 (No. 15, r., col. B, l. 1, and No. 11, r., l. 1)

A-G reads the *ḡ* as clear, but there is scarcely enough of a trace to identify the letter. For *lyd* we may read "through the agency of —" parallel in meaning to the common expression *ana qāti* of the Neo-Babylonian business docu-

ments. The proper name has been identified as the Egyptian *Hr-m-ḥ-t*, with the final element written *mhy*, as in the Aramaean spelling, *Whpr-mhy*.

Line 2 (No. 15, r., col. B, l. 2, and No. 11, r., l. 2)

The name *Pḥh* is identified as the Egyptian *P-ḥ-t* which is found in demotic as *P^a-ḥ^e* and in Greek as *paḥs*.⁸

A horizontal check line just below the first letter of this line and extending into the space between the columns marks the end of an entry and is followed by a space a little wider than normal between lines in this papyrus.

Line 3 (No. 15, r., col. B, l. 3, and No. 11, r., l. 3) and line 4 (No. 15, r., col. B, l. 4, and No. 11, r., l. 4)

A-G does not associate these two fragments. Number 15, recto, column B, line 4, he reads as above, but the second, he believes, starts with *m*, the last letter of a personal name followed by *br*, "son of," with the *ḥ* as the second name. However, what has been read as the *b* in *br* looks much more like *p* or even *w*; since it has no definite head, the traces of the letter before this word in the photograph look more like *r* than like *m*. It is imperative from the join on the verso that there be a bit of space between the two fragments, if Parker's restoration of the date is certain. This would permit the reading of the *n* of Bagaphernes and the *b* of "son of" in the space and regarding the *pr* or *wr*., whatever it may be, as a second name. The *ḥ* is in this context more likely to be a verb than a personal name.

The *lm* at the end would seem to be not an adverbial particle indicating the beginning of the message but the first part of an infinitive form.

Line 5 (No. 15, r., col. B, l. 5, and No. 11, r., l. 5)

Again appears the troublesome *ḥl tbl* first encountered in the shipbuilding papyrus (*AP*, No. 26, ll. 4 and 8). A-G cautiously makes a suggestion that *tbl* may be the Hebrew and Aramaic *tēbhāl*, "world," in the sense of "dry land." Assyrian *tābālu* often occurs in contexts where "dry land" is contrasted with "water." A-G thinks of a "dry dock" in this place but feels that this meaning does not fit in Cowley's context.

It does not suit the context as Cowley's restorations have constructed it, but some of the attempts at restoration are definitely wrong in that difficult papyrus. Cowley's own rendering of *ḥl tbl* as "according to measure, i.e., accurately" compared with Hebrew *ḥēbhāl*, "measuring line," is as poor as his *ytngd*, "drawn up," in the sense of "specifications." Against the idea that the "boat" be "drawn up," Cowley raised two objections: first, that the word for boat is always the feminine *spynh* and would not therefore be used with the masculine verb *ytngd* in *AP*, No. 26, line 4, and, second, that whatever was "drawn up" had to be sent to the treasury.⁹ Consequently, in his restoration of line 3, he supplies *ḥsrn*, understood as "specifications," as the

⁸ Cf. W. Spiegelberg, "Ägyptisches Sprachgut in den aus Ägypten stammenden aramäischen Urkunden der Perserzeit," *Orientalische Studien Theodor Nöldeke* (Gieszen, 1906), II, 1103, No. 24.

⁹ *AP*, p. 92, sub. l. 4.

object to be "drawn up accurately." But the subject need be neither "boat" nor "specifications." It might be a part of a boat that is regarded as of masculine gender. One could suggest "keel" as something that might be "drawn up" on land. It is possible that the *ngydh* of *AP*, No. 26, line 8, is to be read "its keel" or "its prow." The root *ngd*, with the meanings "stretch, draw, pull, prolong, guide," would be an excellent one from which to derive a noun "keel" or "prow," since several of the meanings would fit the requirements for a long projecting part of a ship that was of some use in dragging it up on the beach. I suggest that *ngydh*, "its keel," be substituted for Cowley's *ḥsrn* in the reconstruction of line 3 and read also in line 8. This would overcome the first of his objections.

As for the second objection, if the text is properly read, nothing is said about sending a boat or specifications to the treasury. As elsewhere in the shipbuilding papyrus (cf. l. 6) the verb *šlh* in line 4 means "send word"; the treasury officials were to be notified when the boat was ready for inspection.

If further need exists to support A-G's interpretation, it is found in line 7 of the shipbuilding papyrus (*AP*, No. 26), where it is definitely stated that the boat was "[upon] the sand which was before the fortress," just where the [*ngydh*] *ytngd* [*ḥl tbl*] of lines 3 and 4 instructed the boat should be. In my opinion, with its proper context, the meaning of *ḥl tbl* as "dry dock" fits the context of the shipbuilding papyrus.

After broken context the last clear word in the line seems to read *pnptm*, which A-G regards as possibly an Egyptian name formed by prefixing the element *p* to the name *Nfr-Ḥtm*. But he rightly notes that one would then expect the spelling *np^ttm* written with an *ḥ*. More probable is the suggestion made to me by Parker that the god-element is *Nfrtm*, the son of Ptah, the principal god at Memphis. Such a formation as *P-Nfrtm* would be appropriate for a local Egyptian. It has already been recognized that the element *nfr* can be spelled without the *r* in personal names, as in *Pḥ-Nphtp*. But it is interesting to find that the name *Nfr-tm* is written in Babylonian as *-ni-ip-te-e-mu*.¹⁰ Since our name is spelled in Babylonian fashion and the spelling of the names *Marduk-šar-ušur* and *Memphis* (col. C, l. 10) likewise show Babylonian spelling, it seems quite likely that our scribe had Babylonian rather than Egyptian training.

The remainder of this line is blank.

Line 6 (No. 15, r., col. B, l. 6, and No. 11, r., l. 6)

The third letter of the first word is very difficult, but the traces suggest nothing better than A-G's *š*, peculiarly made but proved by the example in the name *ḥdšdq* in column C, line 2. Apparently the same word appears as *šlšḥ* in line 5 of column C, with which A-G cautiously compares the *šlšal knāphāyim* of Isa. 18:1, which is rendered by the Septuagint as *plōiōn pteruges*. Thinking of the verb *nsḥ* with the traces that follow, he renders this as "bateau de charge."

¹⁰ H. Ranke, *Keilschriftliches Material zur altägyptischen Vokalisation* (Berlin, 1910) p. 40.

Where I read . . . *w bwh*, A-G has "*kwgoo*." He suggests that this and the following line contain proper names, but there is too little evidence to venture even a conjecture here.

Line 6 bis (No. 15, r., col. B, l. 6 bis, and No. 11, r., l. 7)

An interlinear line in smaller letters, apparently a later addition. Whether the last line on fragment 11, recto, belongs to this line or to the line below is not easily determined. The next line, as a total, was probably short. This leads one to believe that the interlinear line was begun with small letters, but, when the line below concluded, the letters became larger as the interlinear line was finished.

In contrast to my reading, A-G has in fragment 15, *byool hzh* and in fragment 11, line 7, recto, *yd I*. A third possibility for beginning the line is *bo . . . k I znh* . . . , but this is less likely, for there are traces of another letter immediately after what would then be *znh*.

Line 7 (No. 15, r., col. B, l. 7)

As A-G has noted, the things or persons here enumerated can be read either *ngyd* or *ngyr*. The occurrence of *ngyd* in this context would be another witness to the possibility of a substantive in the *ngydh* of line 8 of the shipbuilding papyrus.

One would scarcely read *nāghīdh*, "leader" or "ruler," here. If the preceding entries are concerned with boats, as they seem to be, and with repairs, as *l tbl* would signify (if read as "dry dock"), then one might well read "keels" here as the type of repairs made.

Immediately after the total there is a horizontal line, as after line 2, and a space marking the end of an entry.

Line 8 (No. 13, r., l. 1)

A-G reads on this darkened fragment *oom . . . zy* The beginning of the line, which should be on fragment 10, recto, as column B, line 1, is entirely gone, and, as the photograph shows, little can be done with the next two lines also.

Line 9 (No. 10, r., col. B, l. 1, and No. 13, r., l. 2)

This line is much destroyed. It has been read as *oot š* (in frag. 10) and *šlm oo[z](y ng* . . . (in frag. 13), but I have indicated here only what I can see in the photograph. The *[k]pm* is probable; it is the term used in the papyri to indicate one dictating to a scribe¹¹ but might also be used to indicate one giving orders.

Line 10 (No. 10, r., col. B, l. 2, and No. 13, r., l. 3)

A-G reads in No. 10 *oo² . . tn y*, and in No. 13 he questions whether there is any writing at all. The end of the line is probably blank, indicating a short line ending an entry. There is no horizontal line, since that part of the papyrus is destroyed, but a blank line is left after the entry.

Line 11 (No. 10, r., col. B, l. 3, and No. 13, r., l. 5)

The use of brackets in the transcription by A-G does not accord with what is visible in the text.

¹¹ Cf. Cowley, *AP*, No. 5, l. 15; No. 6, l. 17; No. 8, l. 28, etc.

Line 12 (No. 10, r., col. B, l. 3, and No. 13, r., l. 6)

The first word is a probable restoration based on an occurrence of the word in the shipbuilding papyrus where it is also associated, as here, with the verb "to make."¹² From the traces that remain this is a better restoration than the second one that A-G offers, *[n]wpkrt*, "construction de bateau," derived from Persian *navi* and *krtā*. Although this word must still be classed as "unknown," it is probably derived from the Persian *upa*, "under,"¹³ and *kar*, "make," and, if not part of a ship, is probably a reference to a substructure upon which a boat will be set while being built or repaired. In the shipbuilding papyrus the ordnance officials who inspect the boat in need of repair are to make the *wpkrt*. Since there (ll. 8-9) the superintendent of the carpenters is presumably included among the ordnance officials, it was doubtless he who had charge of making the *wpkrt*.

For fragment 13 A-G reads *l:lon² wpoyt*, but the papyrus is too destroyed by wear and stain to be certain of any reading. The *šbyt* I read suggests at once the *spytkn* of the shipbuilding papyrus (l. 9) which Cowley understood as a place name (cf. also *spt*, "nome"), the home of the chief of the carpenters. But the second letter, although somewhat doubtful, looks more like *b* than *p*.

Line 13 (No. 10, r., col. B, l. 5, and No. 13, r., l. 6)

A-G has here *kotn bhoo*, with all the letters marked as doubtful.

Line 14 (No. 10, r., col. B, l. 6, and No. 13, r., bottom)

This has been read "*ooo kygt*," which A-G suggests may be the Persian name Kaigazat(?) formed with the element *azata*, "noble." The first letter might be *d* or *r* but hardly *k*.

Nothing is legible in this line on fragment 13. There once may have been a few letters, now blurred, at the beginning. Apparently it was a short line, ending just beyond the edge of the break.

COLUMN C

Line 1 (No. 11, verso, l. 1) and *line 2* (No. 11, v., l. 2, No. 15, v., l. 1, and No. 14, v., l. 1)

A space two lines deep follows this entry.

Line 3 (No. 11, v., l. 3, No. 15, v., l. 2, and No. 14, v., l. 2) and *line 4* (No. 11, v., l. 4, No. 15, v., l. 3, and No. 14, v., l. 3)

Perhaps this Bagaphernes is the same as in column B, line 4, and possibly is to be read in column A, line 4, also. In the papyri this name is found for a judge (*dyn*) who "came up (the river)" to Syene in the thirty-first year of

¹² Cowley, *AP*, No. 26, l. 5.

¹³ This element is found in Sanskrit in the sense of "under" and also in Greek as *υπο* but is not found in Old Persian. However, in the Aramaic from Persepolis alongside the word *gnzbr*, "treasurer" or "stores keeper," is found *wpnzbr*, which W. Eilers has suggested must be "subtreasurer." This would indicate that the element *wp* here may mean *sub* and that other words, such as *wpšr*, beginning with this element, are to be explained in the same fashion.

Artaxerxes I (435 B.C.); and as the son of *Wšhy*, it is mentioned as that of a contributor to the fund of the Jewish temple at Elephantine in 419 B.C.¹⁴

A-G has translated *nwzyn* as "câbles" or "voiles," deriving the terms from the root *nwz*, meaning "to twist, twine; to weave."

For fragment 15 the reading by A-G is *oob yšt̄mrwn*, of which he translates only "qu'ils se gardent," as though from the root *šmr*. But there is no trace of the initial *y*, and the *m* would be very peculiar. I prefer to read it as the Persian name *Šēthrabāzana* (Old Persian *Xšathrabūjyāna*) found elsewhere in the papyri as *Štrzn*¹⁵ and in the Aramaic rescripts from the time of Darius as *Štharbōznay*.¹⁶ This reading, more in accordance with the Persian pronunciation than the others, satisfies all the traces of letters, although the *r* is crowded. For the uncertain traces on fragment 14 only the *b* has been suggested as legible heretofore, and even that has been marked as doubtful.

Line 5 (No. 11, v., l. 5, No. 15, v., l. 4, and No. 14, v., l. 4)

For the *ššl* compare the note on column B, line 6 above. In the first fragment A-G's readings end with the *zy*. In fragment 15 he reads *ool. thōn mr* For the last he reads only *y š*

Line 6 (No. 11, v., l. 6, No. 15, v., l. 5, and No. 14, v., l. 5)

A-G has read for fragment 15 *šrnō zooo* and for number 14 merely traces with nothing legible.

Line 7 (No. 13, v., l. 1)

This has been read [. *b]gw* by A-G, but the *h* seems quite clear with no trace of a preceding *b*. A slanting trace of a tail of a letter seems to be *p*, because of its length below the line, but it might be either *r* or *d*.

Line 8 (No. 13, v., l. 2, and No. 10, v., l. 1)

For fragment 13 *mñ . . . l ooo* has been read heretofore and for 10 *wñ . . . š d . . . p*

Line 9 (No. 13, v., l. 3, and No. 10, v., l. 2)

Since the *dgl* referred to is presumably at Memphis, it is difficult to determine the name of its commander. At Elephantine names of commandants beginning with *ḡ* are the Persian *ḡrtbnw* and *ḡtrwprn* and the Babylonian *ḡdnnbw*.

For fragment 10 A-G reads "*ošo . . . o . . . l ošn oooof*"—too broken to permit even a conjecture for restoration.

After this line he notes a vacant space, but, according to the photograph, there is nothing unusual in the spacing here.

¹⁴ Cowley, *AP*, No. 16, ll. [1] and 6; No. 22, l. 133.

¹⁵ Cowley, *AP*, No. 5 (471 B.C.), l. 16, and possibly to be restored in No. 13 (447 B.C.) 1.18.

¹⁶ Ezra 5:3, 6; 6:6, 13.

Line 10 (No. 13, v., l. 4, and No. 10, v., l. 3)

The Moabite name *kmšpl̄t* is unique. A-G cites the divine name *kmš* used in the Moabite stone and the use of the element *-pl̄t* in Hebrew names¹⁷ but does not note personal names using the Chemosh element. It is interesting to note that, aside from the name *Chemosh-nadbi* found in the annals of Sennacherib,¹⁸ other examples in cuneiform are found, like *Chemosh-pelet*, in the Persian period. In the sixth year of Cambyses a business document from Babylon involves the "Babylonians" *Itti-nābu-balātu* and *Sin-kitri*, both sons of one bearing the hybrid name *Chemosh-šar-ušur*.¹⁹ Among the witnesses in a business document involving prominent and official individuals, dated to the tenth day of the second Addaru of the sixteenth year of Darius (computed by Parker to be March 27, 505 B.C.), we find mentioned Ḥanṭušu the son of Chemosh-ilu.²⁰ It is apparent that from the beginning of the Persian period Moabites participated in the activities of the Persian empire, often in prominent positions, as far away from Moab as Babylonia and Egypt.

In the translation I have tried to indicate that some official title must have followed the personal name.

The name of the commandant of the company is read by A-G as *Wyool*, but there is scarcely room for two letters in the gap. Since *z* takes up but small space, my conjecture may be correct.

A-G's reading *mnp̄y* for Memphis, while in agreement with the spelling of the name elsewhere in the papyri,²¹ does not agree with the somewhat obscured traces here. The second letter suggests *m* rather than *n*, and there is scarcely room for *y* after the *p*. Its spelling thus suggests the cuneiform name of the city, *me-im-pi* or *m̄-im-pi*, rather than the Egyptian *Men-nufe*.

For the name Marduk-šar-ušur, A-G rightly notes that *šar* is usually spelled in Assyrian fashion *sar* instead of in the Babylonian manner as here, but he is misleading when he suggests that the *s* is almost gone. Only one stroke is missing, and the traces that remain could only be read as *š*.

Line 11 (No. 13, v., l. 5, and No. 10, v., l. 4)

A-G reads as "very probable" *bbym* as the first word, but aside from regarding the first *b* as a preposition he has nothing to offer with respect to meaning. A better reading is *bbys*. This may be the talmudic *bīsā*, "kneading bowl," or "kneading trough," which may also identify the wooden object known in cuneiform texts as *bisu*.

ORIENTAL INSTITUTE
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¹⁷ Cf. *Palṭt*; *Pillay*; *Palṭṭ̄l*, *Platyāhu*, and *Platyāh*.

¹⁸ D. D. Luckenbill, *The Annals of Sennacherib*, Col. II, l. 56; *Ancient Records of Assyria and Babylonia*, Vol. II, §239, p. 119.

¹⁹ M. de Clercq, *Collection de Clercq* (Paris, 1903), II, 126 ff., Tablet C, ll. 1-2, 5 and 12, Pl. XXVI, 3A-F.

²⁰ G. Contenau, *Contrats néo-babyloniens*, Vol. II (1929), No. 193, l. 33.

²¹ Cowley, *AP*, No. 37, l. 11.

DARIUS AND XERXES IN BABYLONIA

GEORGE G. CAMERON

Problems connected with the chronology of the accession years of the false Bardiya, the two Nebuchadnezzars, and Darius have repeatedly been published in this *Journal*. Those connected with the accession of Xerxes have received ample publication elsewhere. New discoveries and a re-examination of the sources now make possible a clearer interpretation of these events.

I. BARDIYA AND "TARZIYA"

The available evidence from Babylonia, supplemented by that from Egypt (see R. A. Parker's article in this issue), definitely confirm the traditional date of 522/21 for the accession year of Darius. Once this is recognized, it is obvious that all the material from Babylonia must fit in one way or another into the general picture. That it does so is clear from the following summary.

Herodotus, in Book iii, chapters 66-67, informs us that Cambyses reigned for seven years and five months. As others have shown, this figure is absolutely correct. The earliest document recording the sole rule of Cambyses¹ is dated on the twelfth day of the sixth month, 530 B.C.; from the Behistun inscription, § 11, we know that Bardiya revolted on the fourteenth day of the twelfth month (523/22); Cambyses reigned, therefore, six full months of 530/29 plus six full years plus eleven months of 523/22—a total of seven years and five months. A few scribes in Babylonia, as is to be expected, were unaware of Bardiya's revolt; others, of Cambyses' death. The last tablet dated to Cambyses, signed on the twenty-third day of the first month of the eighth year,² was written by such a scribe. But the scribe who wrote the first extant published tablet of Bardiya knew that the last month of the Babylonian year 523/22 was in itself Bardiya's "accession year"; therefore, he correctly dated a document written in the

¹ See W. H. Dubberstein, "The Chronology of Cyrus and Cambyses," *AJSL*, LV (1938), 417-19.

² See Olmstead, *AJSL*, LV (1938), 397.

next Babylonian calendar year to "year one, first month, day 19."³ Another scribe, unaware that Bardiya's revolt began in the last month of 523/22, dated his tablet to "accession year, second month."⁴ Other scribes were similarly confused; some continued to regard 522/21 as Bardiya's "accession year," while others regarded it (correctly) as his "first year."⁵

In § 13 of his Behistun inscription Darius says that Bardiya was killed on the tenth day of the seventh month. The last tablet dated to him is "year one, seventh month, first day"—nine days before his death. Herodotus iii. 66-68 relates that Bardiya was unmasked and killed in the eighth month of his reign; this, too, is accurate. Bardiya's reign included the last month of 523/22 plus six full months of 522/21 and part of the seventh month—a total of eight months.

One tablet, never published in full, would seem to indicate that a *Tarziya* (often identified with *Barziya*/*Bardiya*) still reigned in the eighth month.⁶ Pinches, who gave us our first information about this text, stated the case as follows:⁷

The tablet 82-9-18, 360a,⁸ which is a receipt for a certain amount of tithe for the month Marcheswan (October), paid to Takiš-Gula [*sic!*] (apparently a receiver of tithes at Sipar or Sepharvaim), is dated "the 11th day of Marcheswan, in the first year of Tarzia, king of Babylon and countries. . . ." This *Tarzia* is apparently a variant for the more common *Barzia*.

The type-set signs given by Pinches read:

(*araḥ*) *Araḥsamnu* U₄-11-KÁM
 MU 1- KÁM 7 TAR-zi-ia
 šār TIN-TIR(*ki*) u KUR-KUR

That any scribe in Sippar, one month and one day after the death of Bardiya in the *Nisaeen Plain not far from modern Kirmanshah*—and therefore only a few days' ride from Sippar—could still be dating tablets to him, even in a moment of absent-mindedness, is impossible.⁹

³ Poebel, *AJSL*, LVI (1939), 123.

⁴ *Ibid.*

⁵ In addition to the list of known tablets dated to Bardiya (see Poebel, *loc. cit.*), Professor F. W. Geers noted the unpublished text BM 77436 which reads: (*araḥ*) *Abu MU-1-KAM mbar-zi-ia šār babili šār mádti* (day and provenience unknown).

⁶ See now Poebel, *op. cit.*, pp. 123, n. 7, and 138 f.

⁷ *The Babylonian and Oriental Record*, I (1886/87), 54 f.

⁸ Now BM 74635, according to Kugler, *Sternkunde und Sterndienst in Babel*, II, 395.

⁹ See also Poebel, *op. cit.*, pp. 138 f.

Some other explanation is demanded. The one here proposed is so simple that, although this writer is unable to collate the original, he has no doubt that it is the correct one. Clearly the scribe intended to write

MU 𐎠 KÁM 𐎠 kám-bu-zi-ia
"Year one, Cambyses"

but, by the commonest sort of haplography, wrote instead

MU 𐎠 kám-bu-zi-ia.

Pinches knew that MU-1-KÁM was the normal way of writing "year one," and expected the name of a king to follow. Perhaps a badly written *bu*¹⁰ appeared to him as 𐎠 TAR; thus arose a "king" Tarziya, whose existence we must strongly doubt.

Striking corroboration of this explanation is found in the appearance of Taqish-Gula, named by Pinches as "a receiver of tithes" at Sippar. The name is quite common. There is hardly any reason to doubt, however, that the Taqish-Gula at Sippar named in the Pinches tablet is any other than the person of the same name who received offerings in this city for the *sattukku*¹¹ from the second year of Cyrus onward.¹² From the reign of Cambyses specifically, Taqish-Gula figures as the recipient of objects for this offering¹³ on one tablet from the accession year, six from the first year,¹⁴ four from the second year, fourteen from the third year, two from the fourth year, and two from the fifth year. Thereafter Taqish-Gula, in this capacity, is heard of no more.

II. NEBUCHADNEZZAR III

The words of Darius, in § 16 of his Behistun inscription, indicate that Bardiya's death was followed immediately by the "revolt" of Nidintu-Bel, who called himself Nebuchadnezzar. It is now quite

¹⁰ It is still more probable that the scribe actually wrote MU 𐎠 KÁM 𐎠, then had his attention diverted for a moment; when he turned back to his tablet he saw the KÁM and, without bothering to erase the vertical wedge, wrote immediately after it *bu-zi-ia*. The vertical wedge, followed by a somewhat crowded Neo-Babylonian *bu* (see the form of this sign as in Strassmaier, *Cambyses*, No. 60, rev., l. 1) could have been mistaken for 𐎠 TAR.

¹¹ Pinches' translation of the signs for *sattukku* at this period was "tax, tithe"; see his *Inscribed Babylonian Tablets* (London, 1888), pp. 16, 34.

¹² See Tallqvist, *Neubabylonisches Namenbuch*, s. v.

¹³ *Ibid.*, supplemented by occurrences found in the files of the Oriental Institute's Assyrian Dictionary.

¹⁴ Strassmaier (*Cambyses*, No. 69) mentions Taqish-Gula and is actually dated year 1, month 8, day 4, or one week earlier than the Pinches "TARZIYA" tablet.

clear that all tablets assignable to this period which belong to Nebuchadnezzar III are dated to the "accession year of Nebuchadnezzar."¹⁵ The first tablet so dated has been thought to be one written on the seventeenth day of the seventh month—a week after Bardiya's death. One other tablet, however, appears to be dated to him at Sippar on the fourteenth day of the fourth month in the same year.¹⁶ This cannot be correct, however, for it would signify either that Nebuchadnezzar III had already "revolted" at Sippar in the fourth month (unlikely, since Sippar acknowledged Bardiya in that month) or that this one tablet, dated "accession year," belongs to Nebuchadnezzar IV (also improbable, since all the documents dated to the latter are dated "year one").¹⁷ A solution, though given here quite reluctantly, is that Strassmaier's copy is to blame. The Neo-Babylonian signs for the months Du'zu (fourth month) and Tashritu (seventh month) are similar; one vertical wedge, often written under two horizontals, is the chief difference.¹⁸ If we assume that the tablet in question comes from the fourteenth day of the *seventh* month, instead of the fourth month, then all our available evidence checks. Bardiya was killed near Kirmanshah on the tenth day of the seventh month; the news could easily reach Sippar four days later, as the writer knows from experience,¹⁹ at which

¹⁵ Poebel, *op. cit.*, pp. 134 and 136 ff.

¹⁶ Strassmaier, *Nabuchodonosor*, No. 1. Cf. Poebel, *op. cit.*, pp. 135, n. 51, and 139 (where it is cited as unpublished) with reference to Ungnad, *OLZ*, Vol. X (1907), cols. 464 f., who there states that tablets of Nebuchadnezzar III are dated from the fourteenth day of the fourth month of the accession year to the twenty-seventh day of the seventh month of his first year (Poebel, *loc. cit.*, n. 66, cites the last as being [unpublished] in Berlin). Ungnad refers, however, to his review in *ZA*, XIX (1905/6), 416, n. 1, where he assigns Strassmaier, *Nabuchodonosor*, Nos. 1 and (perhaps) 18 to this period. No. 1 is the tablet dated the fourteenth day of the fourth month of the accession year, and No. 18 is dated the twenty-seventh day of the seventh month of the first year! Poebel's table on p. 135, with notes 51 and 66 are, therefore, to be corrected; n. 64 is likewise to be struck out, for the tablet referred to is Strassmaier, *Nabuchodonosor*, No. 17, cited in Poebel's n. 63.

That No. 1 in Strassmaier, *Nabuchodonosor*, belongs to this period (with Ungnad) seems clear from the occurrences in *Sippar* of Bel-eṭir, *epiššānu*, from the tenth, twelfth, thirteenth, and fifteenth years of Nabunaid; the second year of Cyrus; the first, second, and fifth years of Cambyses; and the fifth year of Darius.

¹⁷ See Poebel, *op. cit.*, pp. 135 and 139 f.

¹⁸ The respective signs for these two months have confused others also (cf. Thompson, *Catalogue of the Late Babylonian Tablets in the Bodleian Library, Oxford* [London, 1927], pp. 8 [No. A 111] and 80 [Corrigenda], who writes of another date "Tammuz [or Tishri?]", and Dougherty, *GGCI*, Vol. II, No. 84, where the copy clearly shows Du'zu [fourth month] but in the "Catalogue of Tablets" on p. 60 this is read "seventh month").

¹⁹ See also W. Hinz, "Das erste Jahr des Grosskönigs Dareios," *ZDMG*, XCII (1938), 146.

time Nebuchadnezzar III "revolted," and a scribe dated his tablet accordingly to "Nebuchadnezzar, accession year, seventh month, fourteenth day."²⁰

This Nebuchadnezzar ruled, at least in Babylon and Borsippa, uninterruptedly through the twenty-first day of the ninth month (his last extant published tablet). Darius engaged him in battle five days later, and again on the second day of the tenth month. Four days after the latter event, presumably by the sixth day of the tenth month, or December 22, 522, Darius was acknowledged king in Babylonia.²¹

III. NEBUCHADNEZZAR IV

In § 49 of the Behistun inscription Darius tells us that there was a second Babylonian "revolt" under a pretender "Nebuchadnezzar" (IV) while he was in Persia and Media—thus, presumably, sometime in the fourth month (of 521/20).²² The first tablet of this period dated to "Nebuchadnezzar, Year 1"—and therefore assignable to Nebuchadnezzar IV—was written on an unknown day of the fourth month at Babylon.²³ Throughout the fifth month and the first day of the sixth, however, Sippar was held by Darius, and scribes dated tablets to him.²⁴ By the latter part of the sixth month, specifically on the twenty-fourth day, Sippar was in the hands of troops of Nebuchadnezzar,²⁵ who also occupied Babylon and Borsippa in the north and Uruk in the south throughout parts of the sixth and seventh months. Their control at Sippar was only temporary; Darius was acknowledged as ruler there on an unknown day of the seventh month, but Nebuchadnezzar once more on the twenty-seventh of that month.²⁶

²⁰ Nebuchadnezzar III may even have revolted between the first day (last tablet of Bardiya) and the fourteenth day of the seventh month.

²¹ Note, however, Poebel's remarks in *op. cit.*, p. 134, n. 33. ²² *Ibid.*, pp. 140 f.

²³ Strassmaier, *Nabuchodonosor*, No. 12. Contra Ungnad, *Z A*, XIX (1905/6), 416, n. 1, the Marduk-našir-apli descendant of Egibi in this tablet can scarcely be any other than Marduk-našir-apli, son of Itti-Marduk-balaštu, descendant of Egibi of Strassmaier, *op. cit.*, Nos. 13 and 17, admittedly of this period; cf. also Aplā, son of Suqaia, descendant of Lušu-ana-nur-Marduk of No. 12, with Bel-iddin, son of Suqaia, descendant of Lušu-ana-nur-Marduk, in Strassmaier, *Darius*, No. 236 (eighth year); also . . . mir, son of Bel-aḥḥe-bullit of No. 12, with Itti-Bel-lummir, son of Bel-aḥḥe-bullit, descendant of Sagdidi, in several Cambyses tablets (see Tallqvist, *op. cit.*, s.v.). See Poebel's table (*op. cit.*, p. 135) and correct accordingly by comparison with n. 16 above.

²⁴ Strassmaier, *Darius*, Nos. 16-19; correct Poebel's table (at n. 60).

²⁵ Strassmaier, *Nabuchodonosor*, No. 15.

²⁶ Strassmaier, *Darius*, No. 20; *Nabuchodonosor*, No. 18.

This curious picture of the fluctuating fortunes of the city as it recognized first one sovereign, then another, as troops of one or the other aspirants were quartered in its midst or camped just outside, is exactly what we should expect from these turbulent days. It has an exact parallel in ancient times in the struggle for the city Seleucia between two Parthian rulers, Phraates IV and Tiridates II, each of whom commemorated his all too brief dominance by actually coining money within the city's walls.²⁷ News of the capture of Nebuchadnezzar IV on the twenty-second day of the eighth month was doubtless received in Sippar with a sigh of relief. Babylonia, though now under the control of a foreign conqueror, was at peace, and the documents were thereafter dated to the reign of Darius.

IV. XERXES AND THE BABYLONIAN REVOLT

For thirty-five years Darius capably governed Babylonia. From his thirty-sixth year come tablets which bear witness to his continued control through the fifth and sixth months and into the seventh.²⁸ His death, therefore, can with considerable probability be placed in the seventh month of 486, which began on October 12.

It has generally been assumed that Babylonia revolted once more either just before his death or when news of it arrived in the lowlands. Tablets dated to the accession years of Bel-shimanni, Shamash-eriba, "Akshimakshu," and "Shikushti" have been accepted as bearing witness to this revolt.²⁹

Now it is quite true that Xerxes' appointment as legitimate heir to the throne, before the death of his father, passed over several other sons, as he himself declares,³⁰ and that this situation might well have provoked

²⁷ N. C. Debevoise, *A Political History of Parthia*, p. 137, n. 45.

²⁸ The tablets from the last three months of the thirty-sixth year are:

Month	Day	Provenience	Reference
V	5	Babylon(?)	BM 33,966, cited in <i>Guide to the Nimroud Central Saloon . . . British Museum</i> (1886), p. 117, No. 96, and <i>Guide to the Babylonian and Assyrian Antiquities</i> (3d ed., 1922), p. 158, No. 345
V	9	Borsippa	VS, VI, No. 166 (cf. No. 177)
V	28	Borsippa	VS, III, No. 164
VI	13	Dilbat	VS, III, No. 165
VI	19	Dilbat	VS, V, No. 110
VII	27(?)	Borsippa	VS, IV, No. 180

²⁹ Präšek, *Geschichte der Meder und Perser*, II, 112 and 148 ff.; R. W. Rogers, *History of Ancient Persia* (New York, 1929), pp. 146 f.; Ungnad, *OLZ*, Vol. X (1907), cols. 464-67.

³⁰ Herzfeld, "SAOC," No. 5; last published, *Altpersische Inschriften* (Berlin, 1938), No. 15, pp. 35-38.

revolts both within Persia and elsewhere. It is also true that he himself refers to the revolt of an unnamed country early in his reign.³¹ His statement in itself is not, however, unequivocal.³² Disaffected elements in Egypt or in Asia Minor, instead of in Babylonia, could be referred to by the words "Among those lands which are enumerated above there was (one which) was restless." Furthermore, there is no evidence to show that Xerxes carried out in *Babylonia* his assertion that, "where formerly the daiva's were worshipped, there I worshipped Ahura Mazda. . . ." Thus, although the present writer believes that Babylonia was intended, it must be admitted that these inscriptions themselves give no proof for this belief and cannot help us in dating the revolts.

On the other hand, the available evidence shows that Darius' chosen heir, Xerxes, was accepted immediately in Babylonia. The last tablet of Darius in the seventh month is followed without a break by an accession-year tablet of Xerxes in the eighth month (Arahsamnu 22 = December 1, 486). Other extant tablets demonstrate that Xerxes' reign was uninterrupted throughout the balance of his accession year and all of his first year.³³

³¹ Herzfeld, *Altpersische Inschriften*, No. 14, pp. 27-35; see also Kent, *Language*, XIII (1937), 292-305, and *JAOS*, LVIII (1938), 324 f., for the Old Persian version, and Weissbach, *Symbolae Paulo Koschaker dedicatae* (Leiden, 1939), pp. 189-98, for the Elamite version.

³² Contra Hartmann, *OLZ*, Vol. XL (1937), cols. 145-60; esp. cols. 158-60.

³³ The tablets of Xerxes from Babylonia are:

Year	Month	Day	Provenience	Reference	
Acc.	VIII	22	Borsippa	<i>VS</i> , V, No. 117	
	IX (or XII?)	11	Borsippa	<i>VS</i> , VI, No. 177 ("Akshimakshu")	
	X	7	Strassmaier, in <i>Actes du huitième Congrès international des Orientalistes</i> , II (1893), Section Sémitique (B), after p. 283, No. 16	
	X	22	<i>Ibid.</i> , No. 17	
	XI	9	Borsippa	<i>VS</i> , VI, No. 178 ("Akshimakshu")	
	XI	27	Strassmaier, <i>Actes</i> . . . , No. 18.	
	XII	21	Borsippa	"Shikushti" tablet; see below	
	x	x	Bit-Zahiran(?)	Evetts, <i>Inscriptions of the Reigns of Evil-Merodach</i> Appen., No. 2	
	1	1(1)	I	Babylon	<i>BE</i> , VIII, No. 119
			I	Thompson, <i>op. cit.</i> , p. 13, No. A 124
II		Sippar	<i>Guide to the Nimroud Central Saloon</i> <i>British Museum</i> , p. 120, No. 104		
III		3	Borsippa	<i>VS</i> , IV, No. 191	
III		x	Babylon	<i>VS</i> , VI, No. 179	
IV (or VII)		15	Thompson, <i>op. cit.</i> , p. 8, No. A 111	
V		20	Strassmaier, <i>Actes</i> . . . , No. 19	
VI		17	<i>VS</i> , IV, No. 192	

[Footnote 33 continued on page 321]

Furthermore, the assumed king "Akshimakshu" is none other than Xerxes himself;³⁴ the scribe or scribes who so wrote the royal name had obviously misheard it and would no doubt have been quite surprised to discover that historians of a later day would ascribe it to another than Darius' legitimate successor.

Likewise there was no Babylonian rebel named "Shikushti." The evidence for such rests entirely on a brief paper read by Pinches before the Thirteenth Congress of Orientalists meeting in Hamburg in 1902, a résumé of which was published in the proceedings of that body.³⁵ Therein he presented the formulas of two tablets, one dated to Belshimanni, the other to "Šikušti, king of Babyon, king of the lands." Belshimanni is known from other sources and must be accredited (see below). But Pinches himself was dubious about "Shikushti," for he says:

The . . . Persian royal name is . . . mutilated, the first character, *ši*, and the last one, *ti*, being all that is certain. Between these two characters are two others, which look as if they might be *ki-nim*, but the inclination at

[Footnote 33 continued from page 320]

Year	Month	Day	Provenience	Reference	
2	VIII	30	<i>VS</i> , IV, No. 193	
	X	29	Rimmé (Borsippa)	O. Krückmann, <i>Neubabylonische Rechts- und Verwaltungs-Texte</i> , No. 98	
	XII	3	Susa	<i>VS</i> , IV, No. 194	
	XII	13	<i>VS</i> , VI, No. 180	
	I	x	Sippar	Evetts, <i>op. cit.</i> , Appen., No. 3	
	III	14(?)	<i>VS</i> , VI, No. 181	
	III	15	Sippar (?)	Revellout, <i>PSBA</i> , IX (1887), 238	
	IV	6	Borsippa	Clay, <i>BRM</i> , I, No. 85	
	2(1)	VIb	25	Dür-	<i>VS</i> , V, No. 118; see n. 40
	3	II	2	Babylon	Strassmaier, <i>Actes</i> . . . , No. 20
4	IV	1	Babylon(?)	Evetts, <i>op. cit.</i> , Appen., No. 4	
5(?)	I	22	<i>VS</i> , III, No. 181	
5	IV	2	Babylon	Evetts, <i>op. cit.</i> , Appen., No. 5	
9	II	9	Babylon	Moore, <i>Neo-Babylonian Documents in the University of Michigan Collection</i> , No. 56	
12	X	6(?)	Nippur	<i>BE</i> , VIII, No. 120	
16(?)	V	26	Arahtu	<i>VS</i> , III, No. 185	
16	VI	2	Borsippa	Krückmann, <i>op. cit.</i> , Nos. 173-77; also <i>VS</i> , III, No. 182	
	VI	5	Kâr-Tashmetum (Babylon)	<i>VS</i> , III, No. 183	
16(?)	VI	10	Borsippa	<i>VS</i> , III, No. 184	
	x	x	Arahtu	<i>VS</i> , III, No. 186 (cf. No. 185)	

Over 110 Elamite tablets from Persepolis, now undergoing examination in Chicago or copied by the writer in Teheran, document all the years of Xerxes except 8, 9, 11, 13, 14, 17, and 21. This group of texts is from the Treasury.

³⁴ See also Ungnad, *OLZ*, Vol. XI (1908), Beihefte, p. 25; F. W. König, *Reallexikon der Assyriologie*, s.v. "Akšimakšu." The dates of the tablets bearing this name fit admirably into the accession year of Xerxes as can be observed in the preceding note.

³⁵ T. G. Pinches, "Notes upon a Small Collection of Tablets . . . Belonging to Lord Amherst of Hackney," *Verhandlungen des XIII. Internationalen Orientalisten-Kongresses* (Leiden, 1904), pp. 267-70.

which some of the wedges of the inscription are written suggests that some, which look like "corner-wedges," are in reality uprights, or even horizontals, and in this case a modification of the traces would be necessary, changing . . . *ki* to . . . *ku*, and . . . *nim* to . . . *uš*.

No copy is given, and the owner of the collection, Lord Amherst, died before the tablet could be included in a proposed second volume of the Amherst tablets; the collection was sold in separate lots, and the location of this text at present is unknown.³⁶ Pinches' transliteration runs as follows:

Bar-sip D.S. [draḥ] Adari, ūmu ēšrāa-išten, [šattu] rēš lugal-nam-uš(?) -ḫu, Ši-ku(?) -uš(?) -ti šar Bābīli D.S. šar mātāti.

Apparent at once is the fact that *lugal-nam-uš(?) -ḫu* cannot stand. It is, of course, possible that *šarrāti* could be written LUGAL.NAM (if this is not another mistake of Pinches) instead of NAM.LUGAL(.LA). But, if so, then the royal name must begin either with the doubtful *uš* or at least with the *ḫu*; the latter sign must on the original be followed by a vertical wedge which Pinches could interpret as the male determinative. Anyone who takes the trouble to turn the transliteration back into the script of the period may see that the royal name can be read as one of the many forms of Xerxes' name. These forms vary from *Ak-ši-ia-ar-ši* and *Ak-ši-ia-mar-šu* (with their parallels) to *Ḫi-ši-i-ar-šu* (with its parallels) and *Aḫ-ši-ia-ar-šu*.³⁷ Pinches' *uš(?) -ḫu(?)* <^m>*ši-x-x-ti* can easily be a misreading of partially illegible signs which are merely a minor variation of the latter form. His *uš(?) -ḫu(?)* is clearly ^m*Aḫ*, which is followed by *ši*; *uš(?) -ti* at the end is obviously *mar-ši*, and the whole name is to be read ^m*Aḫ-ši-i(?) -mar-ši*. Thus we may safely eliminate "Shikinimti" or "Shikushti" from the list of pretenders or claimants to the throne of Babylon. Since the witnesses' names "belong to the end of the reign of Darius, and are also found during that of Xerxes" (so Pinches), we may unhesitatingly add this tablet to the few already known from *Borsippa* dated in Xerxes' accession year.³⁸

Through the accession and first years of Xerxes, therefore, there

³⁶ Pinches' prediction that his original paper would "probably appear" in *PSBA* seems never to have been fulfilled. It is to be hoped that he made a complete copy of this text and that his copy is now in the hands of E. F. Weidner, who inherited Pinches' copies of other tablets in the same collection; see *AJO*, XIII, Heft 1/2 (1940), 46.

³⁷ So written in Everts, *op. cit.*, Appen., No. 3; written *Aḫ-šū-mar-ši-i* and *Aḫ-ši-ia-mar-šū* in Strassmaier, *Actes* . . . , Nos. 16 and 19, respectively.

³⁸ See n. 33, above.

was no opportunity for any Babylonian to raise the standard of revolt. A revolt in the second year is likewise improbable. We know from Elamite tablets from Persepolis that the second year was an intercalary year, possessing a second Ululu.³⁹ As we shall see, tablets dated to the accession years of Bel-shimanni and Shamash-eriba, accredited rebels, come from months five and six (Abu and Ululu) and six and seven (Ululu and Tashritu), respectively. Now, if either of their revolts (particularly the latter) occurred in the second year, we would expect—in spite of our limited evidence—one of the tablets to be dated in that second Ululu. The fact that none is so dated is meager proof, but our suspicion that there was no disturbance in this year is corroborated by a Babylonian tablet of Xerxes' reign dated to a second Ululu of a year which must be the second year.⁴⁰ If, therefore, Xerxes held Babylonia in this month of the second year, it is at least unlikely that the land had revolted at all (or was in rebellion in the preceding and following months) in this year.

One or both of the revolts may, however, have occurred in any year of Xerxes after the second year (exclusive of the sixteenth). The third and fourth years are represented by only one tablet each; the fifth by two; the ninth and twelfth by one each. All the other years of Xerxes have no documentation whatsoever. This may, of course, be more or less accidental. What can hardly be an accident, however, is one of two changes in the royal titulary. In all the documents from the accession year through the first three months of the first year⁴¹ Xerxes' title is "King of Babylon, King of the Lands," with a minor variation.⁴² In the fifth month of the first year came word—no doubt by royal decree—that the title was to be changed. One scribe in that

³⁹ Nos. PT 4-158 and PT 4-745, copied by the writer in Teheran.

⁴⁰ *VS*, V, No. 118. The copied signs of the year-date are cross-hatched, indicating doubt on the part of the copyist Ungnad, and look like 4 above, plus 2 or 3, beneath. This would be an abnormal way of writing 6 or more in this period, however. Further, as will be made clear below, the title given Xerxes ("King of Persia, Media, king of Babylon and the Lands") is one used only from about the fifth month of the first year through the fourth month of the fourth year. The tablet can therefore hardly be dated after the fourth year, for it is not an unimportant document (cf. the witnesses), and its scribe would surely be aware of the correct royal titulary. Inasmuch as we know from other sources that Xerxes' second year actually had an intercalated Ululu (see n. 39), Ungnad's copy must represent a crowded 2-κΑΜ.

⁴¹ Excluding *VS*, VI, Nos. 177 (accession year) and 180 (first year), and *VS*, IV, No. 192 (first year), which bear no title, and the two documents in Thompson, *op. cit.*, for which no title is given.

⁴² "King of Babylon and Lands."

month wrote "King of Persia, Media," after Xerxes' name,⁴³ but thereafter, through the fourth month of the fourth year, the title is always "King of Persia, Media, King of Babylon and the Lands" (with minor variants). This alteration was merely an expansion of the first title and is scarcely worth emphasizing. The really marked change, and one which may well indicate royal exasperation with the refractory people of Babylonia, comes with the fifth year. Probably with the very first month of that year⁴⁴ "King of Babylon" is dropped from the royal titulary and is never again used throughout the balance of Xerxes' reign or in any of his successors'. The chief *Persian* title, "King of the Lands,"⁴⁵ though used earlier in Babylonia, now became standard. Its use for the first time in Xerxes' reign early in the fifth year is an argument, however weak, for dating at least one Babylonian revolt to the preceding or fourth year (482) just before Xerxes set out for Greece. Noteworthy is the fact that in Xerxes' army list Babylonia and Assyria are bracketed together,⁴⁶ indicating that each had lost its status as an independent unit. Further, when we bear in mind the fact that, according to some classical sources, Xerxes is connected with the theft of a statue of Marduk, with a revolt, and perhaps with a razing of Babylon *before his Grecian campaign* (481/80), then it appears quite likely that these events were bound up intimately with the revolts of Bel-shimanni and Shamash-eriba in the fourth year and an arbitrary change in the royal titulary by the fifth. When the revolts were put down, Marduk's statue was carried off so that no future rebel could "grasp the hands of Bel" and thus be recognized as the legitimate ruler of Babylonia.

The classical sources, unfortunately, do not entirely agree as to the time of the theft and the revolt. One passage of Herodotus (i. 183) merely states that, whereas Darius did not dare to carry off a coveted statue of Bel-Marduk, Xerxes not only succeeded in doing so but even killed a priest of the god in the process. Another passage (iii. 150 ff.) contains a folk story (often copied by later Latin authors) relating how Babylon revolted from Darius but, after a siege lasting

⁴³ Strassmaier, *Actes* ... , No. 19.

⁴⁴ VS, III, No. 181; cf. Evetts, *op. cit.*, Appen., No. 5.

⁴⁵ Cf. Herzfeld, *AMI*, I, 15, for its possible Median origin.

⁴⁶ Herodotus vii. 63, which was called to my attention by Professor A. T. Olmstead.

over twenty-one months, was subdued through the machinations and self-mutilations of Zopyrus. The usually unreliable Ctesias⁴⁷ tells how Xerxes became king, visited Babylon to see the tomb of "Belitana," and then proceeded to Ecbatana, where he heard that Babylon had revolted and killed its commander, Zopyrus. Babylon, says Ctesias, was recovered by Megabyzos. Photius says of this version: "What [Herodotus] relates of Zopyrus is attributed by Ctesias (with the exception of a mule giving birth to a foal) to Megabyzos, son-in-law of Xerxes." Ctesias then specifically states that this revolt occurred and was put down before Xerxes departed for Greece.⁴⁸

Unfortunately, likewise, there is nothing to indicate the specific year or years of Xerxes into which fall the "accession-year" documents of Bel-shimanni and Shamash-eriba.⁴⁹ All in all, there is no conclusive evidence regarding the time of the revolt. This much, however, appears evident: Xerxes followed Darius—in Babylonia at least—without any untoward event. There were no revolts against him in his accession year or in the first and second years which followed. His adoption of the title "King of Lands" by his fifth year is, in all likelihood, to be connected with the subjection of one or more revolts in Babylonia in the third, or, more probably, the fourth year.

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⁴⁷ Frags. 20-23 (ed. Müller; Gilmore's edition is not available). These sections of Ctesias' narrative are paraphrased and explained in Aelian *Var. Histor.* xiii. 3.

⁴⁸ Arrian *Anab.* vii. 17 (cf. also iii. 16) dates the sack of the Marduk temple after the return from Greece. No date is given in Strabo *Geogr.* xvi. 1. 5 or in Plutarch, *Moralia*, p. 173C.

⁴⁹ The Bel-shimanni tablets are:

Month	Day	Provenience	Title	Reference
V	10?	Borsippa	King of Babylon and Lands	See n. 35
VI	1	Dilbat	King of Babylon	VS, VI, No. 331
x	x	Borsippa	King of Babylon and Lands	VS, III, No. 180

The Shamash-eriba tablets are:

Month	Day	Provenience	Title	Reference
VI	25	Borsippa	King of Babylon	VS, III, No. 178
VI	x	Borsippa	King of Babylon, king of Lands	VS, III, No. 179
VII	21	Borsippa	King of Babylon	VS, V, No. 116
VII	22	Babylon	King of Babylon and Lands	ZA, III (1888), 140 f. and 157 f.
VII	23	Borsippa	King of Babylon, king of Lands	VS, VI, No. 173

TWO INSCRIPTIONS OF ASHURNASIRPAL

It is not generally known that, in the possession of the Walker Art Gallery at Minneapolis, there are two inscriptions of Ashurnasirpal. These texts are inscribed on two limestone blocks or tablets; the first (No. 375) measures roughly 18 by 30 inches, and the second (Nos. 373+374) 18 by 36 inches. Both tablets are in an excellent state of preservation; the cuneiform characters are extremely clear, and it is only in the case of the second tablet that even a portion of the text has been obliterated (the right-hand edge of the stone is slightly worn).

The two texts are very similar to those published by Budge and King.¹ That on the first tablet is 23 lines in length and follows the version there given without significant variation except for the latter portion (rev., ll. 9 ff.) which is identical with the text published by Le Gac in *Les Inscriptions d'Assur-našir-aplu III*, page 168, center.

The inscription on the second tablet, 24 lines in length, is quite similar to the fragmentary versions published on pages 166-68 of Le Gac's volume. The missing parts of lines 8-9 of those inscriptions appear as follows (l. 24): *ù rubê (pl) šá da-ra-te êpuš(uš) ú-si-im-ši*, with which the Walker text ends.

UNIVERSITY OF MINNESOTA

TOM JONES

¹ *Annals of the Kings of Assyria* (London, 1902), I, 173-76: "Inscription upon Limestone Tablets Recording the Building of Ashur-našir-pal's Palace in Calah."

FIFTIETH ANNIVERSARY CELEBRATION

THE UNIVERSITY OF CHICAGO

In connection with the celebration of its Fiftieth Anniversary, the University of Chicago extends a cordial invitation to scholars and scientists to attend a series of symposia to be held from September 22 to 26, inclusive. "Approaches to Linguistics," "Interpretation and Criticism of Art and Literature," "Problems in Historical Materials," and "Archeology as a Tool in Humanistic and Social Studies" are some of the topics which will be of considerable interest to our readers. Among those who will take part are Am. Alonso, C. R. Morey, M. I. Rostovtzeff, E. H. Sturtevant, R. L. Engberg, H. Frankfort, A. L. Kroeber, R. H. Lowie, R. P. McKeon, W. L. Westermann, and H. R. Willoughby, of whom the first four named will receive honorary degrees in the course of the celebration. Everyone who wishes to accept the invitation is urged to send his name and address as soon as possible to The Director of the Fiftieth Anniversary Celebration, The University of Chicago.

BOOK REVIEWS

KLEIN, WALTER C. *Al-Ibānah ʿan Uṣūl ad-Diyānah* ("American Oriental Series," Vol. XIX.) New Haven, 1940. Pp. xiii+143. \$2.00.

In his version of al-Ashʿarī's *al-Ibānah ʿan Uṣūl ad-Diyānah* (*The Elucidation of Islām's Foundation*) Walter C. Klein presents this Moslem classic's first translation into a Western language. The translation's value is measurably increased by the Introduction—thirty-eight pages of skilfully condensed exposition. *Ijtihād* (p. 8, n. 19) is better rendered, literally, by "self-exertion" than by "effort, struggle." No one will question the assertion (p. 11) of the Umayyads' "unmistakable secularity," but one wonders whether the next statement—"Only one of them, ʿUmar II, was a devout man"—is not too strong. Little mention is made of Zoroastrian influences in Islām and none of Manichaeism: *zindīq's* (p. 22) are defined only as "dualists, atheists." *bilā kayfa*, "the magic form of words" (p. 36), is rendered by "without further inquiry" (p. 24); "without *how-ness*" (i.e., "without further specification") or "without modality" would more nearly convey the sense to the "Christian theologian, non-expert to the resourceful intricacies of Moslem theology," whom the translation is intended to instruct. With the same Christian theologian in mind, one regrets that such a well-balanced book should be marred by the improper term "Muḥammadanism" (p. 1) for "al-Islām."

The author fears lest his style "occasionally seem unsuitably colloquial" (p. 39); this reader found no passage where such was the case. The translation stands in lucid, straightforward prose, and the footnotes are both complete and instructive. al-Ḥajjāj b. Yūsuf, "the notorious Umayyad general" (p. 54, n. 124), would more aptly be called "viceroys" or "governor-general." There are few misprints: page 129, note 539, for *Moth read Mother*. In the index of Arabic words (p. 137 f.) *arsh* and *istiwa* (so given in Professor Macdonald's transliteration, p. 83, n. 306) should appear as *ʿarš* and *istiwā* to conform with Dr. Klein's system. *ḥadīṭ* (p. 137) has lost its accent. Ibn ʿAsākir's "Kitāb tabyīn kidb al-muftarī fī ma nasaba ila-l-imām . . . al-Aṣḥarī" (cited on p. 25, n. 78, and again in the Bibliography, p. 39) should read "Kitāb . . . fī mā nusiba. . . ."

The sum of such small mistakes in no way detracts from the value of the volume. This handy translation, buttressed by its Introduction, notes, Bibliography, and excellent indexes (of Arabic words, Introduction, and Qurʾānic citations) will be a ready tool for the hands of many. The translator well terms the *Ibānah* "an arsenal of arguments"; for recasting these weapons into dependable English he deserves the thanks of all students of the Moslem world.

L. V. THOMAS

Oriental Institute
University of Chicago

BOOKS RECEIVED

GLUECK, NELSON. *The Other Side of the Jordan*. New Haven: American Schools of Oriental Research, 1940. Pp. xviii+208+127 figs. and Frontispiece.

This compact volume is based on the lectures given by the author in this country from October to December, 1939. Most of the material has been presented before in various journals, for the most part in the annuals and the bulletins of the American Schools of Oriental Research, for which Dr. Glueck carried on his work as director in Jerusalem. The results of the author's explorations, the archeological finds, and their bearing on the period of the Hebrew advance into Palestine and the monarchy are of the utmost importance to the student. His chapter on Nabataean civilization includes the results of the work at Khirbet et-Tannur and gives a fine picture of that as yet too little known kingdom.—A. D. TUSHINGHAM.

GRANT, ELIHU, and WRIGHT, G. ERNEST. *Ain Shems Excavations (Palestine)*, Part V: *Text*. ("Biblical and Kindred Studies" No. 8.) Haverford, Pa.: Haverford College, 1939. Pp. 172+11 figs. +2 pls. in color, including Frontispiece.

With this volume of text to accompany the plates in *Ain Shems IV* (1938) is begun the definitive publication of the material collected during five field expeditions (1928-33) conducted by Haverford College. The material is well organized, separate chapters in Section I being devoted to stratification in general, defenses, and to each of the separate settlements on the site. In Section II the pottery is discussed stratum by stratum, and then the rest of the artifacts. Under the latter is included a very fine section by Emily de Nyse Wright tying the flint finds of Ain Shems with what is known of the Neolithic culture and periods immediately following, i.e., the Tahunian, before 4000 B.C., the Ghassulian of the fourth millennium, and the Canaanite: the culture of the historical period which begins as early as the second half of the fourth millennium.—A. D. TUSHINGHAM.

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- THE JOURNAL OF GEOLOGY. *Founded in 1893. Edited by ROLLIN T. CHAMBERLIN. Semi-quarterly, \$6.00 a year*
- PHYSIOLOGICAL ZOOLOGY. *Founded in 1928. Edited by WARDER C. ALLEE. Quarterly, \$7.50 a year*
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Volume LVIII

OCTOBER 1941

Number 4

365.25

The Origin of Monumental Architecture in Egypt	<i>H. Frankfort</i>	329
The Old Aramaic Alphabet at Tell Halaf: The Date of the "Altar" Inscription - - - - -	<i>Raymond A. Bowman</i>	359
The Story of Jericho: Further Light on the Biblical Narrative	<i>John Garstang</i>	368
Darius and His Egyptian Campaign - - -	<i>Richard A. Parker</i>	373
Hurrian Consonantal Pattern - - -	<i>Pierre M. Pures</i>	378
The Oriental Institute Archeological Report on the Near East, 1941 - - - - -		405
Book Reviews - - - - -		417
J. M. Powis Smith, <i>The Prophets and Their Times</i> (Theophile J. Meek)		
Douglas E. Van Buren, <i>The Cylinder Seals of the Pontifical Biblical Institute</i> (H. Frankfort)		
Richard F. S. Starr, <i>Indus Valley Painted Pottery</i> (Donald E. McCown)		
Books Received - - - - -		427

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CONTENTS OF VOLUME LVIII

ABBOTT, NABIA, Pre-Islamic Arab Queens - - - - -	1
——, Women and the State on the Eve of Islam - - - - -	259
BOWMAN, RAYMOND A., An Aramaic Journal Page - - - - -	302
——, The Old Aramaic Alphabet at Tell Halaf: The Date of the “Altar” Inscription - - - - -	359
BRAIDWOOD, ROBERT J., The Date of the Byblos Temples - - - - -	254
BRUX, ADOLPH A., A Simplified System of Hebrew-English and Ara- maic-English Transliteration - - - - -	57
CAMERON, GEORGE G., Darius and Xerxes in Babylonia - - - - -	314
FRANKFORT, H., The Origin of Monumental Architecture in Egypt - - - - -	329
——, Book Review: <i>The Cylinder Seals of the Pontifical Biblical In- stitute</i> by Douglas E. Van Buren - - - - -	419
GARSTANG, JOHN, The Story of Jericho: Further Light on the Biblical Narrative - - - - -	368
HARDY, ROBERT S., The Old Hittite Kingdom—A Political History - - - - -	177
HYATT, J. PHILIP, A Note on YIWWĀDA ^c in Ps. 74:5 - - - - -	99
IRWIN, W. A., Samuel and the Rise of the Monarchy - - - - -	113
——, A Rejoinder - - - - -	100
JACOBSEN, THORKILD, Book Review: <i>Lamentation over the Destruction of Ur</i> by Samuel N. Kramer - - - - -	219
JONES, TOM, Two Inscriptions of Ashurnasirpal - - - - -	326
KRAELING, E. G., The Origin of the Name “Hebrews” - - - - -	237
MCCOWN, DONALD E., Book Review: <i>Indus Valley Painted Pottery</i> by Richard F. S. Starr - - - - -	421
MAY, HERBERT GORDON, “Al . . .” in the Superscriptions of the Psalms - - - - -	70
MEEK, THEOPHILE, J., Book Review: <i>The Prophets and Their Times</i> by J. M. P. Smith, Revised Edition by W. A. Irwin - - - - -	417
OBERMANN, JULIAN, Koran and Agada: The Events at Mount Sinai - - - - -	23
The Oriental Institute Archeological Report on the Near East - - - - -	104
The Oriental Institute Archeological Report on the Near East, 1941 - - - - -	405
PARKER, RICHARD A., Darius and His Egyptian Campaign - - - - -	373

PARKER, RICHARD A., Persian and Egyptian Chronology - - -	285
PURVES, PIERRE M., Hurrian Consonantal Pattern - - -	378
SPRENGLING, M., Pahlavi Notes - - - - -	169
—, The Berlin Druze Lexicon (<i>continued</i>) - - - - -	91
STAPLES, W. E., The Reading of Hebrew - - - - -	139
THOMAS, L. V., Book Review: <i>Al-Ibānah ‘an Uṣūl ad-Diyānah</i> by Walter C. Klein - - - - -	327
TORREY, CHARLES C., A Forged Phoenician Royal Inscription in the Louvre - - - - -	135
WATERMAN, LEROY, Some Repercussions from Late Levitical Genealogical Accretions in P and the Chronicler - - - - -	49
WILSON, JOHN A., The Egyptian Middle Kingdom at Megiddo - -	225
WINLOCK, H. E., Graffiti of the Priesthood of the Eleventh Dynasty Temples at Thebes - - - - -	146
WORRELL, W. H., Four Safaitic Graffiti - - - - -	217
—, Notice of a Second-Century Text in Coptic Letters - - -	84
ZIMMERMANN, FRANK, On Eccles. 3:18 - - - - -	100
BOOKS NOTED:	
Glueck, Nelson, <i>The Other Side of the Jordan</i> - - - - -	328
Grant, Elihu, and Wright, G. Ernest, <i>Ain Shems Excavations, Part V: Text</i> - - - - -	328
Horst, Friedrich, <i>Die zwölf kleinen Propheten</i> - - - - -	102
"Ishmerai," <i>Ezekiel Speaks Today: A Critical Analysis of the Prophecy of Ezekiel in the Light of Modern Events</i> - - - - -	427
Klein, Walter C., <i>Al-Ibānah ‘an Uṣūl ad-Diyānah</i> - - - - -	327
Kraeling, Emil G., <i>The Book of the Ways of God</i> - - - - -	102
Kramer, Samuel N., <i>Lamentation over the Destruction of Ur</i> - - -	219
Lacheman, E. R., <i>Miscellaneous Texts</i> - - - - -	102
Langton, N. and B., <i>The Cat in Ancient Egypt</i> - - - - -	224
Lautner, Julius Georg, <i>Altbabylonische Personenmiete und Erntearbeiter Verträge</i> - - - - -	428
Lieberman, Rabbi Saul, <i>Shkiin: A Few Words on Some Jewish Legends, Customs, and Literary Sources Found in Karaite and Christian Works</i> -	103
Lutz, Henry F., <i>A Neo-Babylonian Debenture and A Recorded Deposition concerning Presentment for Tax Payment</i> - - - - -	427
Marrero, Leví, <i>Perfil del imperio hitita</i> - - - - -	427
Smith, J. M. P., <i>The Prophets and Their Times</i> . Revised edition by W. A. Irwin - - - - -	417
Starr, Richard F. S., <i>Indus Valley Painted Pottery</i> - - - - -	421
Van Buren, Douglas E., <i>The Cylinder Seals of the Pontifical Institute</i> -	419
Van den Oudenrijn, Marc-Antoine, <i>La Vie de Saint Za Mikā‘el ‘Aragāwī</i>	224
Wright, G. Ernest, <i>See Grant, Elihu</i>	

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THE ORIGIN OF MONUMENTAL ARCHITECTURE
IN EGYPT

H. FRANKFORT

Interchange of ideas between individuals, it is generally admitted, stimulates mental activity. But that intercourse between communities has the same effect is not so readily granted. In fact, any suggestion of foreign influence upon a community is likely to be regarded as derogatory to the group. It is forgotten that the "cultural potential"¹ of the group is one of the most important elements in the process and that there is an immense difference between mechanical copying, on the one hand, and, on the other, creative borrowing in which a stimulus from outside unchains indigenous inventiveness.

The origin of monumental architecture in Egypt is a case in point. Suddenly, with the First Egyptian Dynasty, we find throughout the country buildings of sun-dried brick, ornamented with elaborate recesses. This type of architecture did not survive the Fourth Dynasty, but its derivations, translated into stone or paint, are found at all periods in the "false doors" of the tombs and in the traditional frame

¹ A useful term introduced by Captain G. H. L. F. Pitt-Rivers. See *Race and Culture* (London: Royal Anthropological Institute of Great Britain and Ireland, 1935), p. 4.

The illustrations used for this article are drawn from the following sources: Pl. I, A and B: courtesy the Oriental Institute and Dr. Heinrich Balcz; Pl. I, C, with Figs. 3, 7, and 11: from Petrie, Wainwright, and Gardiner, *Tarkhan*, Vol. I; Pl. I, D, and Fig. 12: from Capart, *L'Art égyptien*, Vol. I; Pl. I, E, and Fig. 4: courtesy E. A. Speiser; Figs. 1 and 2: from De Morgan, *Récherches ... de l'Égypte*; Figs. 5 and 6: from *UVB*, Vol. VIII; Fig. 8: from *Kemí*, VII (1938), 40; Fig. 9: after Balcz.

which surrounds the Horus name of Pharaoh. The meaning and function of these two features are fairly well known. They are therefore commonly used as the starting-point for a discussion of the earliest Egyptian buildings in brick. But, as we shall see, we can explain neither the character of these latter structures nor the sudden appearance of a highly sophisticated method of brick-building in Egypt in that way. Moreover, any explanation of the facts in strictly Egyptological terms ignores the remarkable circumstance that closely similar buildings were erected at about the same time in Babylonia. Now all these problems find a simple and straightforward solution when we assume that a technique and style which we can watch developing in Mesopotamia became known to the Egyptians about the time of Menes.

Recent Egyptological writings show a noticeable aversion to this sort of explanation. This is largely a reaction against the gross overworking of the argument of foreign influence by a preceding generation of authors. But the prevalent assumption that Egypt, especially in the formative phases of its development, was entirely self-contained is equally unwarranted. Hence this prefatory plea for an unbiased approach to the problem under discussion.

I

Stone architecture, so characteristic for Egypt, was not introduced before the conclusion of the experimental phase of Egyptian culture in the Third Dynasty. Blocks of stone, boulders adapted to a limited architectural purpose, had been used already in the First Dynasty for important parts in the subterranean sections of the royal tombs, such as doorsills, floors, and the portecullis. But the visible portions of the tombs, as well as the cenotaphs and valley temples which formed part of the funerary monuments of Dynasties I-II, were built of sun-dried bricks. They represent the earliest Egyptian buildings in that material and, at the same time, are the earliest monumental buildings known in the Nile Valley. They therefore form a new departure in two respects.

As regards the building-material, there is no difficulty in assuming that the Egyptians invented brickmaking of their own accord. The point cannot be proved, though, for we do not know of any structures

in brick antedating the First Dynasty.² At Maadi some square bricks were found³ (later Egyptian bricks are always oblong), but they did not belong to any recognizable structure. The houses there were built of wood and matting, distinguished from those at Badari and from Amratian houses by a somewhat more solid method of construction. At Merimde dugouts have been discovered, oval structures, half-underground, built up with lumps of clay. In building these, and in daubing wattle and reed shelters with clay, there was ample opportunity to invent brickmaking, which merely required standardization of the lumps used. But opportunity does not always lead to invention,⁴ and there is nothing to show that bricks were used in Egypt to any extent prior to the First Dynasty. Corroborative evidence that the introduction of brick architecture was one of the features distinguishing early dynastic from prehistoric culture comes from Nubia. There the predynastic culture continued to flourish after the reign of Menes, and brick architecture is not found in protodynastic times.

We have evidence that public buildings too were built of wood and matting in predynastic Egypt. The Hunters' Palette depicts one of these buildings, and pictures of others survive in hieroglyphs representing certain traditional shrines or palaces (Fig. 10).⁵ There is, therefore, nothing in what we know of predynastic architecture that would lead up to the brick structures of the First Dynasty. Yet the latter sometimes recall the older buildings in one respect: they occasionally carry a painted decoration which renders poles and multi-

² Petrie and Quibel (*Naqada and Ballas*, p. 54) mention "New Race" graves with brick lining, but there is no certainty that they antedate the First Dynasty. Petrie (*Prehistoric Egypt*, p. 44) gives the sequence dates of these graves as 50-70 and 74, respectively, and sequence dates do not work well for the last part of the predynastic period. Babylonian influence becomes strongly manifest at sequence date 63, where the Gebel el Araq knife handle is to be placed because of the type of its flint blade.

³ *Mitteilungen des Deutschen Instituts für Aegypt. Altertumskunde* (hereafter abbreviated *MDIAA*), V (1934), 112.

⁴ The invention of copper-working for practical purposes is a good example of this lack of connection between opportunity and achievement. A. Lucas has shown (*JEA*, XIII, 162 ff.) that Egypt was most favorably situated to make the discovery, yet comparison of the actual artifacts shows that Asia possessed a far superior copper industry throughout the fourth and third millenniums B.C. and that it was already regularly making objects of practical use, while Egypt produced such objects and some ornaments only occasionally. Copper came into practical use in Egypt by the end of the Predynastic period when intercourse with Asia had demonstrably reached considerable intensity (see Sec. III of this article).

⁵ The interpretation as shrines is usual. That on the lower right of our figure is called a palace by Sethe, *Urgeschichte und älteste Religion der Ägypter* (1930), p. 130, n. 2.

colored mats. There is no example of this painting known in the First Dynasty, when only traces of whitewash or red paint are found. But a few of the later instances show a painted decoration which is obviously intended to equate the structure with the venerable pavilions and kiosks of an earlier age.⁶ The more striking is the contrast between the plan of the new buildings and the requirements of the old material which the painting simulates. We observe the rendering of a wooden framework of uprights braced by horizontal poles; and variegated mats are lashed to the latter by means of ropes (Fig. 12). This procedure leads in actuality to wide unbroken stretches of surface. But the brick wall, upon which the image of this construction is painted in the Old Kingdom, presents a succession of projections and recesses of not more than ten to twenty centimeters in width. There can be no question of any connection between the plan of the brick structures and those which had hitherto been built of wood and mats. The painting is evidently superimposed upon a building of entirely independent origin, and it cannot, therefore, explain the latter at all.⁷

The contrast just discussed underlines the extraordinary fact that there are no antecedents in Egypt for structures of such complexity as the tombs and cenotaphs of the First Dynasty. Of predynastic times none are known to us; the tombs of Aha and Narmer, the rivals

⁶ This is the view most commonly held. Reisner (*Development of the Egyptian Tomb*, p. 292) assumes that the painting imitates a temporary structure erected to harbor offerings or perhaps the king's body while the tomb was being prepared. It would be most unusual to find that the permanent structure simulated the appearance of the temporary one instead of the reverse.

⁷ It may be an accident, but the earliest occurrence of this style of painting dates to the Third Dynasty (Hesire); if not accidental, this fact would suggest a wilful approximation to hallowed archaic structures rather than a mere imitation of a recently defunct style of building. The latter assumption would have been supported if the earliest brick buildings, those of the First Dynasty, had been covered with designs imitating mats on a wooden framework. The importance of this question is that imitation would lead to a faithful reproduction of as many original features as was practicable, while the magical equation did not require an exact reproduction of the original structure, and the existence of the recessed surface of the brickwork would be no objection to covering it with a suitably adapted design suggesting the older technique of building. A. van Gennep and G. Jequier in *Le Tissage aux cartons et son utilisation décorative dans l'ancienne Egypte* (Neuchâtel, 1916) show that the designs on the "false doors" could have been produced by the technique which they discuss. If the "false door" is, however, considered in its relation with the actual buildings of the First Dynasty which have survived and which are recessed all around, it is hard to see how a technique which produces narrow strips of woven material can account for the architectural forms with which we are confronted. (See Sec. II, 4, below.)

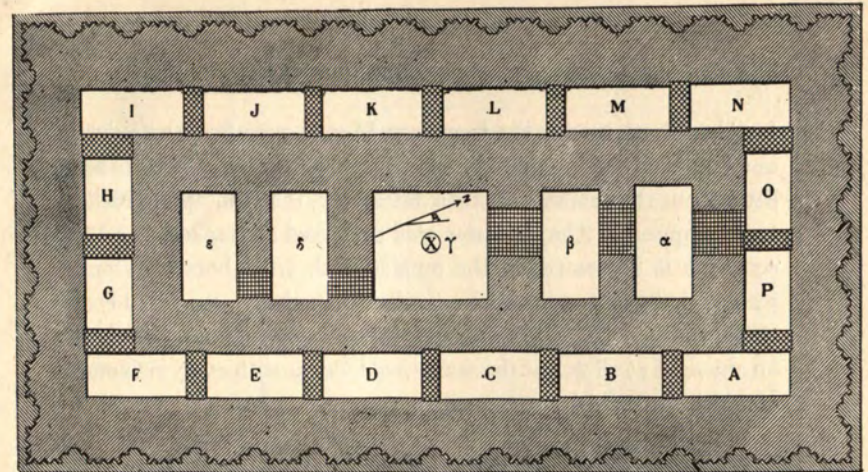


FIG. 1

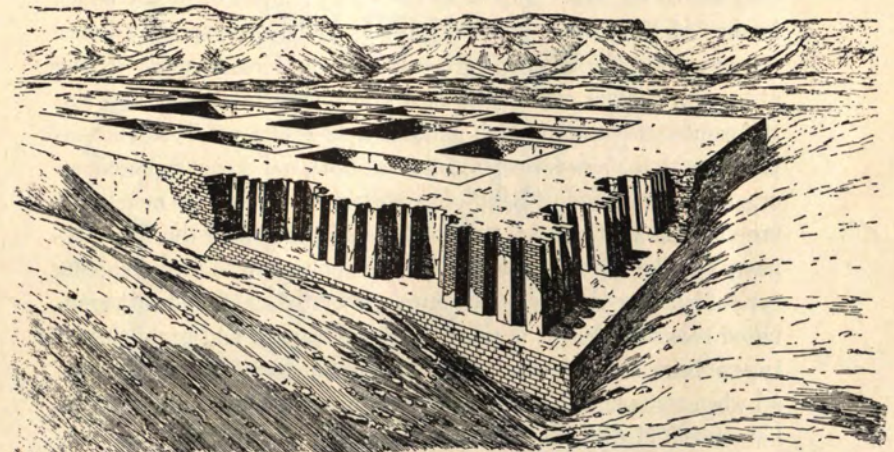


FIG. 2

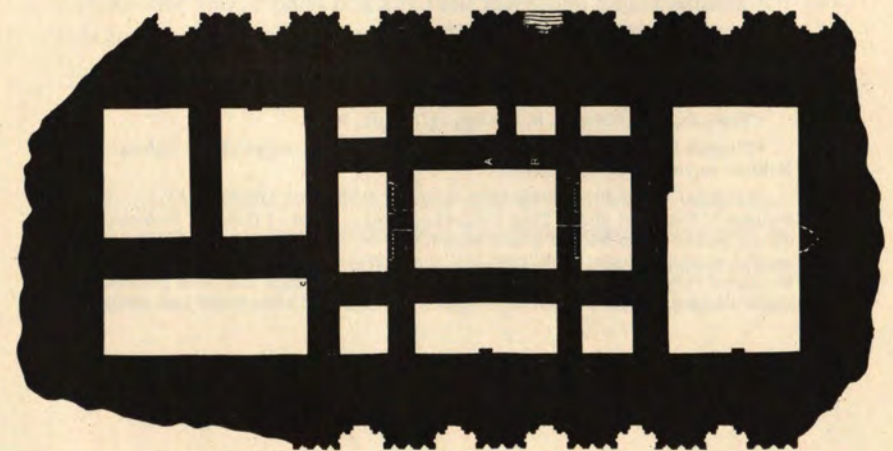


FIG. 3

in which stout posts are connected by plank walls.¹¹ We must insist at once that this explanation differs in all essentials from the similar-sounding one which has been frequently applied to the Egyptian recessed buildings and which we shall discuss in detail below (Sec. II, 1). At Tepe Gawra we are, first, in a region where even now wood is not scarce. Second, the plan, with the correspondence of projections and recesses inside and outside the building, suggests an actual wood construction, while in Egypt, as we shall see, the connection between brickwork and its supposed wooden prototype cannot be established by a simple functional interpretation but requires a fanciful and forced theory. Third, we notice at Tepe Gawra significant features which show that (if the theory of a wooden prototype is correct) the bricks are already being utilized in an independent manner and with special regard for their potentialities. For in the corners, and in the buttresses of the northern temple, stepped blocks of brickwork were used as reinforcements. Whatever value we attach to the theory of a derivation from woodwork (and the theory is not indispensable for the rest of our argument), these blocks of stepped brickwork, and the alternation of "posts" and flat walls at Tepe Gawra, show the first stages on the road which led to the elaborate recessing of the Uruk and Jemdet Nasr periods, which so closely resembles that introduced in Egypt under the First Dynasty. A further step on this road may be seen in the temples of Eanna at Warka, where the earlier buildings show simple recesses, and these inside (Fig. 5, Building C); then we notice doors which have been subsequently bricked up (this also happened in the temple on the Anu ziggurat),¹² and thus outside niches may have been suggested and introduced; they become more complicated in the later buildings in Eanna, dating to the Jemdet Nasr period. If, on the other hand, the earlier stages of the "White Temple" (Fig. 6) go back to as early a period as has recently been suggested,¹³ the development at Eanna would merely be collateral and the invention of recessing is

¹¹ UVB, X, 26. The sculptured vase base from Khafaje (*OIC*, No. 20, Fig. 27) cannot be quoted in this connection. The "planks" of the side are joined in such a way that rain water would be guided into the house through the cracks. Nor is Lenzen's view that the lower part would represent a stable tenable; the object is part of a numerous series of decorated temple furniture which does not reflect objects from daily life at all.

¹² UVB, VIII, 4.

¹³ Ann Louise Perkins, "Comparative Stratigraphy of Mesopotamia" (unpublished dissertation to appear shortly as an *SAOC*).

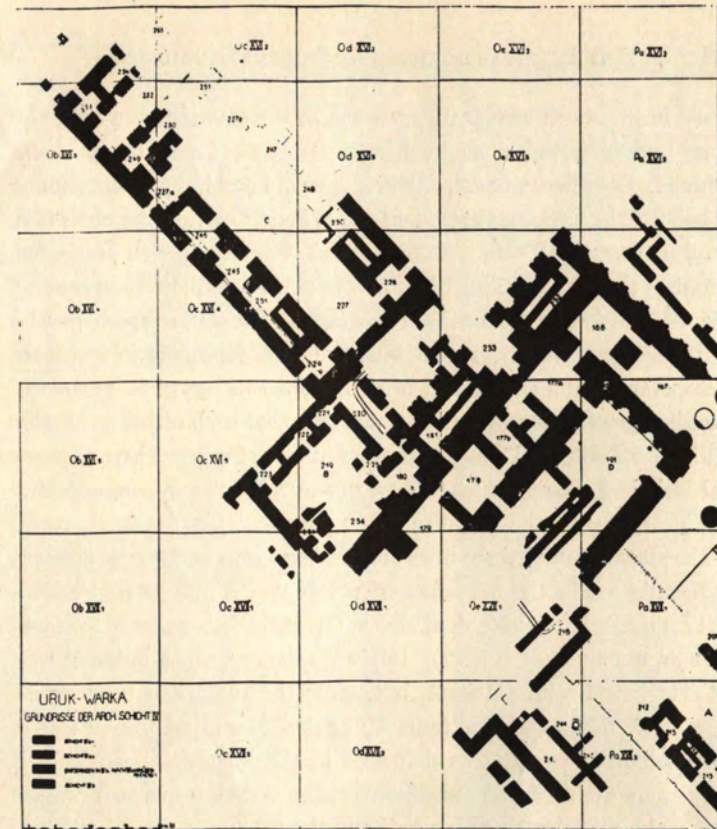


FIG. 5

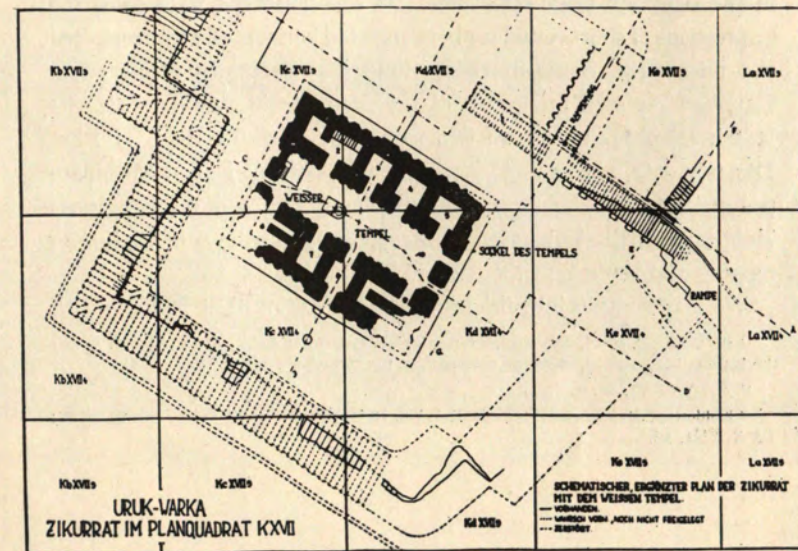


FIG. 6

as old in the south as it is in the north of the country. We need not worry unduly, however, about these early stages of recessing in Mesopotamia. It suffices to realize that it is an old and indigenous manner of building in brick in that country. Whether or not one accepts a wooden prototype for the buildings of Gawra XIII, whether or not one considers that the blocking of doorways helps to explain the presence of outside niches in the south, in any case the close correspondence of details between the developed brick building, especially in southern Mesopotamia, and the earliest brick buildings of Egypt is far too extraordinary to be accidental. It is not only that such niches as appear in Figure 5, Building D, recur exactly similar in Egypt. There are several technical details which the two countries have in common and which corroborate the evidence of the plans.

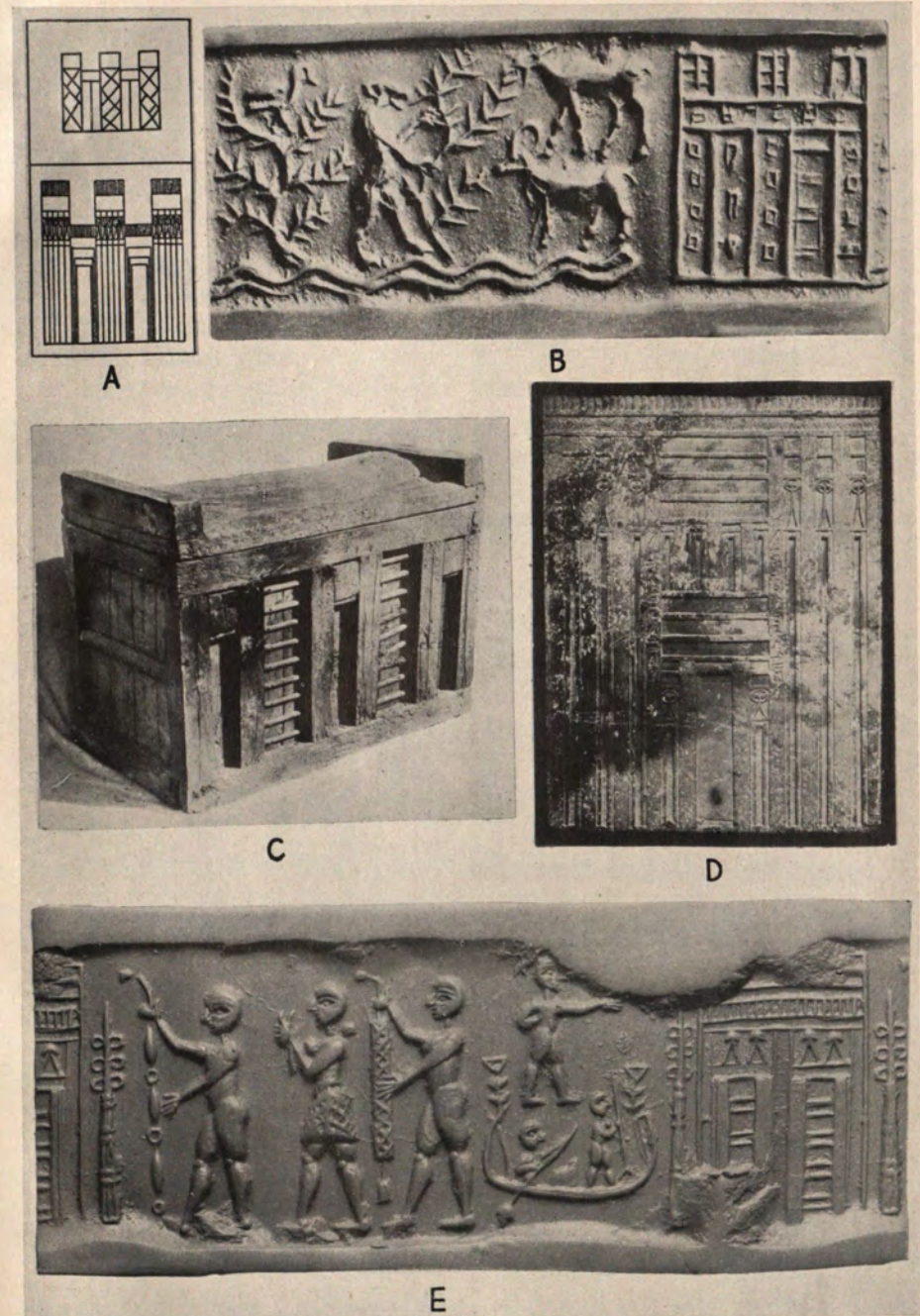
The alternation of three rows of stretchers with one row of headers at Naqada recalls the *Riemchenverband* of Warka and its parallels at Tell Asmar.¹⁴ The cenotaph of Neithotep at Naqada appears to stand upon an oblong base (Fig. 2). In reality, however, the building was not set upon a solid platform, but, after the walls had been erected, a small revetment was built up against it around the outside.¹⁵ This method is the same as that used in the much-discussed *kisu* of Babylonian architecture, first discovered at Babylon but now known from many places such as Ishchali and the "White Temple" at Warka.¹⁶ The latter building possesses yet another feature which recurs in the Egyptian brick structures. Its well-preserved walls show the impressions of short round timbers, inserted horizontally to strengthen the brickwork. Some of these timbers are actually preserved in Egyptian recessed brickwork of the mature Old Kingdom at Abu Roâsh (Fig. 8), but a wooden sarcophagus of Tarkhan, of First Dynasty date, allows us to judge the effect of this feature in the appearance of the older Egyptian buildings (Pl. I, c), and this usage is identical with that observed in Mesopotamia in the combination of recesses and timbers in the "White Temple."

We may recapitulate the argument by stating that building with

¹⁴ *OIC*, No. 20, p. 11, and Figs. 7-8; Heinrich, *Schilf und Lehm*, p. 40. At Tell Asmar the layers of headers are more numerous than the stretchers.

¹⁵ *ÄZ*, XXXVI, 88, n. 3.

¹⁶ Koldewey, *Das wiedererstehende Babylon*, Index under "*kisu*"; *OIC*, No. 20, p. 78; *UVB*, VIII, 38.



sun-dried bricks gradually developed in Mesopotamia to the exact state of complexity in which we meet it, without indigenous antecedents, in First Dynasty Egypt. There is consequently a strong *prima facie* case for assuming that its introduction into Egypt was based on knowledge of contemporary buildings in Mesopotamia. And the corroborative evidence to that effect is conclusive.

To appreciate this, we must realize that only one period in Mesopotamia provides parallels for *all* the features we have just mentioned. That is the Jemdet Nasr period. Only then do we find small oblong bricks used, if not exclusively, then at least predominantly. In preceding ages the bricks were, on the whole, of a much larger type; and in the succeeding Early Dynastic period the bricks were plano-convex. In this latter period the recessing is, moreover, of a different character; if used all around a building, it is of a highly simplified type, while the complicated recesses resembling those in Egypt are reserved for the two towers which flank and thereby accentuate the entrance into palace or temple complex. But in the Jemdet Nasr period we find all-around recessing used outside the buildings and also inside in important rooms or courts such as Room 252 in Figure 5 or Sin Temple V at Khafaje. The extant brick buildings of the First Dynasty do not contain such rooms or courts, but we find the same usage in some temporary buildings of the Fourth Dynasty, erected in brick where the permanent structures were to be of stone.¹⁷ We know that the stone architecture of this dynasty did not use recessing. Its appearance in the brickwork with which Shepseskaf completed the unfinished valley temple of Mycerinus shows, therefore, that recessing was an established traditional feature of brick-building in Egypt at the time and that there is some likelihood that it was used around courts or rooms, just as it was used around the outside of buildings, at the time of its first introduction. This presumed similarity would agree, in any case, with those more firmly established indices pointing to the Jemdet Nasr period (to the exclusion of the periods preceding and following it) as the stage in Mesopotamian culture in which Egypt learned the use of bricks for monumental buildings.

¹⁷ Reisner, *Mycerinus*, Plans IV, VIII, IX and Pls. 31c, 32b, e, 33, and 74. For Mesopotamia see Room 252 of Building C in Fig. 5 and the central room of Sin Temple V in *OIC*, No. 20, Fig. 26.

This conclusion is corroborated by two independent sets of evidence. In the first place, it has long been known that a number of features of pre- and protodynastic Egyptian culture possess Mesopotamian parallels; these can now be assigned to the Jemdet Nasr period. In the second place, there is independent stratigraphical proof from Tell-Judeideh that the Jemdet Nasr period was contemporary with the Late Predynastic and Protodynastic period in Egypt.

We shall consider these two sets of evidence in Section III of this paper. For the moment we merely point out how much material falls into place, how many phenomena find an explanation, when we interpret the sudden appearance of brick architecture in an accomplished form under the First Dynasty as due to Mesopotamian examples. We may well ask whether any alternative explanation can compare with it in coherence and economy of means.

II

All earlier explanations of the recessed buildings of Egypt are defective in that they do not account for the remarkable resemblance which the structures show with contemporary buildings in Mesopotamia. We repeat that this resemblance requires explanation no less than the connections with material falling traditionally within the scope of the Egyptologist. Since, however, these Mesopotamian parallels have been discovered only recently, we shall here consider—on their merits, irrespective of their common failing—the current theories on the origin of this type of structure. They may be classed under four headings. The first two are entirely without foundation; the last two are valuable in establishing the relation between the protodynastic and later usages without, however, explaining how recessing first came to be adopted.

1. In 1912 Flinders Petrie found at Tarkhan sarcophagi made of planks which had originally served another purpose, as shown by a series of holes which stood in no relation to the function of the planks as parts of coffins but which enabled them originally to be lashed to other boards (Fig. 11). Petrie, knowing the unheralded appearance of recessed brick-building in the First Dynasty and having previously entertained the idea that there might be wooden prototypes for these buildings, attempted to join the boards, reused in the Tarkhan coffins,

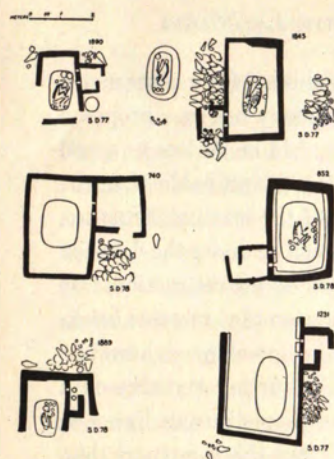


FIG. 7

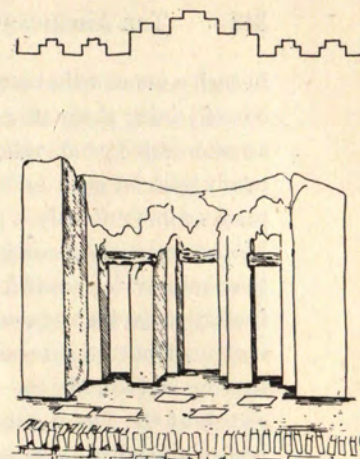


FIG. 8



FIG. 9

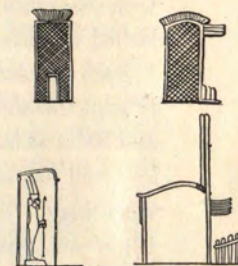


FIG. 10

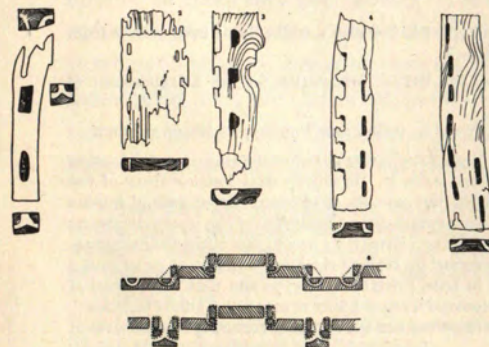


FIG. 11

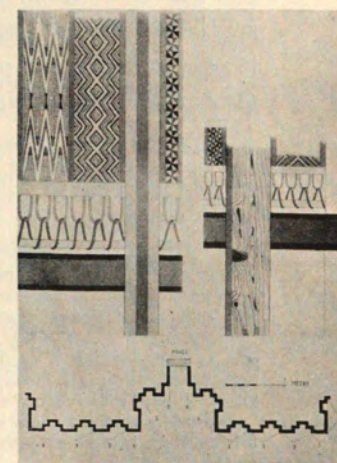


FIG. 12

in such a manner that a recessed surface resulted. Many other methods of joining them are possible, although nobody has ever proposed an alternative combination; and indeed it would be useless to spend much time on such an attempt since it is most probable that the planks represent only a minute remainder of the original structure. Moreover, no one reading Petrie's account¹⁸ can be under the delusion that he provides proof for the correctness of his reconstruction. He frankly states that he *started* with the assumption that recessed brick-building rendered a wooden prototype; he then proclaimed, however, that he had found "the actual timbers." He further embarks on a sketch of the life of "the more prosperous people" using "wooden houses which had to be moved twice a year"; but this is no more than an ingenious fantasy in which puzzling ancient remains are combined with observations of modern Egyptians, though these are innocent of lashed wooden houses.

Such an imaginative reconstruction is often enlightening. In the present instance, however, it has served us ill. Newberry in 1923¹⁹ and Balez as late as 1929²⁰ adopted Petrie's flight of fancy, and Woolley even introduced it, lock, stock, and barrel, into Mesopotamian archeology.²¹ It is not remembered that the painted decoration (Fig. 12) of this type of building in Egypt, even if it were not incompatible with the wall surface upon which it is superimposed, would not represent a wooden original at all but a framework of poles to which mats are lashed. The building thus rendered agrees perfectly with the pictures of early buildings preserved in hieroglyphs (Fig. 10) and on such monuments as the Hunters' Palette.²² The whole assumption that

¹⁸ Petrie, *Tarkhan I and Memphis V*, p. 24. See also a coffin lid of such planks from Abu Ro'ash in *Kemi*, Vol. VII, Pl. XIII, 2.

¹⁹ Presidential address to Sec. H of the British Association for the Advancement of Science, 1923, p. 17.

²⁰ *MDIAA* (see above, n. 3), I, 78 ff.

²¹ Hall and Woolley, *Al Ubaid*, pp. 68 ff.

²² Professor E. Baldwin Smith, *Egyptian Architecture as Cultural Expression*, approaches our problem as an architect but does not solve it. He admits that Petrie's story of the wooden houses is unsubstantiated (p. 33) but accepts as relevant his scheme of joining the coffin boards from Tarkhan (p. 47) and postulates a prototype for the *serekh* consisting of a palace constructed of small-wood joinery. Even if we accept the author's interpretation of one of the archaic buildings preserved on seals of the First Dynasty as proof that a "light timbered construction" existed in Late Predynastic Egypt and that it consisted of "a paneled framework made of small pieces of wood skilfully squared and joined together," this would not in any way explain the recessing nor the fact that recessing appears first of all in brick buildings.

wooden houses existed at all is based on the finding of some perforated boards in secondary use. The perforation must, of course, be explained. But we know of a most important class of products which were demonstrably built of lashed and pegged timbers. These are the ships of the ancient Egyptians.²³ It is interesting that some of the boards at Tarkhan are curved (Fig. 11), most unsuitably if they were lashed together to resist climatic changes and to protect the inhabitants of movable wooden houses from "gaps which let the wind blow directly in," but very understandably if they derive from boats. The most natural explanation of the Tarkhan boards is, evidently, that they represent valuable raw material salvaged from wrecked or disused Nile craft which was unsuitable for use in furniture for the living by reason of the holes but which served well enough for coffins.

2. According to certain Egyptologists, the recessed brick buildings present a Lower Egyptian style. This assumption does not explain their origin, of course. Balez and Newberry, as we have seen, borrow Petrie's hypothesis to account for them, but the first uses one other argument, namely, that in some renderings of these structures a pair of bound papyrus flowers appears high up in the niches (Pl. I, D). This design is considered a parallel of the later symbol in which the hieroglyph *sm^c*, "to unite," joins the heraldic plants of Lower and Upper Egypt. Much ingenuity is spent on the explanation of the fact that two identical plants combined may stand for the combination of two different regions. The entirely hypothetical course of events postulated by Sethe,²⁴ on the strength of later texts (treated with a method of most dubious validity²⁵), is made to supply the political units, the coalescence of which is reflected in the two bound flowers. It seems to be quite forgotten that we lose here every connection with ascertained fact, even irrespective of the circumstance that we know next to nothing about the Delta in prehistoric times but a great deal about Mesopotamia during that period.

²³ Short wood only being available, the planks of the hull were very thick and joined by large pegs in the shape of dowels which would well fit the holes which we observe in the boards. Lashing was also resorted to (see W. F. Edgerton, "Ancient Egyptian Ships and Shipping," *AJSL*, XXXIX, 109 ff., esp. 128 and 134).

²⁴ Sethe, *op. cit.*

²⁵ Kees, in *Nachrichten der Gesellschaft für Wissenschaften in Göttingen* (Phil.-hist. Klasse, 1930): *Kultlegende und Urgeschichte*.

We might, perhaps, drop the matter here, but the distinction of Upper and Lower Egyptian features assumes the characteristics of a fashion and should be warned against. Its origin may be found, perhaps, in Scharff's successful explanation of the puzzling overlapping of the First and Second Predynastic (Amratian and Gerzean) cultures.²⁶ He made it appear highly probable that the Gerzean culture existed in Lower Egypt while the Amratian still continued in the Upper Nile Valley. The distinction between Upper and Lower Egyptian styles of architecture has, however, no evidence in its favor. Yet we find Steckeweh, in an excellent publication,²⁷ contrasting the "open" architecture of Upper Egypt, which tends to elaborate its outward appearance, with the "closed-in-on-itself," introspective style of the North. Wolf²⁸ and Pflüger,²⁹ on the other hand, find the Upper Egyptian style, with its "grave greatness," a "truly African" phenomenon, while the "charming gay lightness of the Mediterranean world" is said to be clearly felt in the Lower Egyptian style. This swamp of fortuitous and contradictory interpretations has even brought some very distinguished authors under its spell. Junker refers to our tombs as a Lower Egyptian type;³⁰ Scharff³¹ admits that the cenotaph of Neithotep at Naqada, in Upper Egypt, is the earliest example of recessed brick-building but insists, nevertheless, on its Lower Egyptian character. The Lower Egyptian style would, besides recessing, be characterized by the inclusion of magazines or other rooms in its superstructure, while the Upper Egyptian type would use smooth outer wall for its solid superstructure but possess subterranean rooms approached by a shaft. If this were correct, it would be paradoxical that in the tomb with two "false doors" it is not the northern but the

²⁶ *JEA*, XIV, 261 ff.

²⁷ *Die Fürstengräber von Qaw* (1936), p. 43. The place which the tombs at Qaw occupy in the history of art is very well established by the author. I merely take exception to certain of his interpretative generalizations.

²⁸ *ÄZ*, LXVII (1931), 129 ff.

²⁹ *JEA*, XXIII, 7 ff.

³⁰ Junker, *Giza II*, p. 6. Daum (in *Giza I*, pp. 66 ff.) postulates a contrast between the Naqada and the Abydos types of tombs which can hardly be substantiated, since the superstructures of the Abydos tombs are unknown. Moreover, he ignores the existence of the valley temples at Abydos.

³¹ *Handbuch der Archäologie*, pp. 440 ff. When Scharff mentions only the tomb of Neithotep at Naqada as an Upper Egyptian example of the supposed Lower Egyptian type, he ignores the valley temples of kings Zer and Zet and Queen Merneith at Abydos.

southern one that shows the recesses in most cases.³² However, the distinction does not hold good. At Sakkara tombs have recently come to light which belonged to high officials, Hemaka and Nebetka, of kings of the First Dynasty, especially of Den-Wedimu.³³ And in these tombs we find recesses but also rooms in both substructure and superstructure.

It would seem that the attempt to distinguish between Upper and Lower Egyptian peculiarities is, in any case, unlikely to lead us very far. The physical configuration of the Nile Valley makes for a uniform culture. What evidence we have seems to show that fashions set by the Residence spread easily and quickly throughout the land except in times of disturbance.³⁴ We can distinguish metropolitan and provincial styles; but, if there were at any time marked local differences between the north and the south, they seem to have become unrecognizable at this distance of time.

Rejecting this theory, we must account for the group of the two joined papyrus flowers high up in the recesses, just as we have accounted for the perforations of the coffin boards at Tarkhan after rejecting Petrie's hypothesis. In the present instance we can be less certain of the correct interpretation, for the flowers preserved are only in ancient renderings, not in the original. It is interesting, in the first place, that a very similar design appears in a Mesopotamian rendering of a recessed building, namely, on a seal cylinder of the Uruk period from Tell Billa (Pl. I, *E*). Here there can be no question of papyrus, of course; but, if the Egyptians took over certain fairly complicated types of buildings, they may have retained the detail of the flower design but translated it into familiar forms. Its simplest explanation would be a wooden window grille. That would explain quite naturally its position high up in the recesses. In stone models of similar recessed buildings found at Warka,³⁵ the windows seem to be triangular; but that is certainly unusual, and to assume that the pair of flowers in the frame render a square space closed by a grille with an attractive de-

³² Reisner, *Tomb Development*, p. 249.

³³ W. B. Emery, *The Tomb of Hemaka*; *JEA*, XXIV, 243; *Annales du service des antiquités*, XXXVIII (1938), 455 ff.

³⁴ Brunton (*Qaw and Badari*, I, 75) shows how the spread of certain types of pottery and other objects was interrupted by the upheaval of the First Intermediate period.

³⁵ *UVB*, Vol. VII, Pl. 48.

sign does not tax our imagination unduly. The design is found in Egypt used for this very purpose; it is shown in the Sixth Dynasty in the carved wooden screens which close the curved top, front and back, of Ipi's litter,³⁶ and it recurs in the open-work (*mushrabiya*) upper parts of doorways in the New Kingdom.³⁷

3. It has been assumed that the recessed buildings render a royal palace.³⁸ Not that any palaces anterior to the Eighteenth Dynasty have been discovered, but the framework in which the king's Horus name is written is also known as a hieroglyph reading *serekh*, perhaps best translated "seat of royalty." The *serekh* design shows indeed a panel of recesses; but it is a design on the flat and consequently teaches us very little about the actual appearance of the building which it is supposed to depict. It may be well to recall that no satisfactory correlation has as yet been established between the ruins of the three buildings, probably palaces, actually excavated at Tell el Amarna, and the two detailed drawings of the royal palace which have been found in the rock tombs at the same site. In dealing with the *serekh* we have much less to go by. If we try to translate this design back into three dimensions, we have no guaranty whatsoever that our reconstruction hits the mark. We do not even know which part of the palace is indicated by the *serekh* design. Why should it be the façade and not, for instance, part of the throne-room? Moreover, if we accept the interpretation as façade, we gloss over the very real difficulty that the actual buildings which we know—namely, the cenotaphs and valley temples of the First Dynasty—are covered with recesses on *all four walls*. The idea of a façade picture is fortuitous.

The *serekh* sign actually supports our view of a Babylonian origin of recessed brick-building. If it is hazardous to argue from an ancient rendering to the appearance of the building in the round, we may at least compare Egyptian abbreviated renderings with similar renderings from Mesopotamia. These are preserved on seal cylinders of the Uruk and Jemdet Nasr periods (Fig. 9, *right*). It is quite clear that they resemble the Egyptian designs (Fig. 9, *left*) so closely that some

³⁶ Wreszinski, *Atlas*, I, 405.

³⁷ *ÄZ*, LXXIII, 68 ff. with Pl. VIIIa and Fig. 3.

³⁸ The fullest account of the reasons which support the identification with a royal palace are given in Reisner, *Development of the Egyptian Tomb*.

relation must exist. There are, however, sufficient differences to make it impossible to assume that these Mesopotamian designs themselves were copied in Egypt. Consequently, the actual buildings, of which the Egyptian and Mesopotamian engravings are independent abbreviations, must have resembled each other; and this is the conclusion reached in Section I of this paper.

I am tempted to indulge here in a little speculation. If the tomb, and possibly the palace, of pharaoh assumed architectural forms which in Mesopotamia (whence these forms were derived) were characteristic for temples, this choice of the Egyptians is perfectly understandable, for it corresponds exactly with the difference in Mesopotamian and Egyptian views as to the nature of kingship. The Mesopotamian ruler, though representing the god, was LÚ.GAL, "the Great Man"; but pharaoh was a god.

However this may be, it does not affect the present argument that the derivation of the recessed buildings of the First Dynasty from the royal palace is unsatisfactory because it merely displaces the problem to a category of buildings of which no example survives. The brick buildings in Egypt, tombs and cenotaphs, show recesses on four sides, corresponding in that respect to contemporary Mesopotamian temples. The *serekh* designs are no certain guide to the actual appearance of buildings, but in so far as they resemble contemporary small-scale drawings from Mesopotamia they suggest that somewhat similar buildings were erected at about the same time in both countries. The occurrence of towers raises a fine point of chronology. In the first place, the tower with a vertical side is a regular feature of Mesopotamian architecture but a very unusual one in Egypt which developed the pylon, with a pronounced batter. Second, these towers do not occur on seals of the Uruk period but are known on a cylinder of the Jemdet Nasr period (Pl. I, *B*) and are a standing feature in the succeeding Early Dynastic period. The occurrence of *serekh* designs with towers (Pl. I, *A*) and without (Fig. 9, *left*) suggests therefore, again, the Jemdet Nasr period as the age of contact.

4. We can say that recessed architecture is a feature of brick-building in essence as well as in incidence. The point is strikingly illustrated by some temporary brick structures with which Shepseskaf

completed the unfinished valley temple of Mycerinus.³⁹ Recesses were not used in the stone architecture of the period; yet as soon as bricks were used in a funerary monument they reappear, obviously because they were a traditional method of enriching brick surfaces. In the stone-built tombs of the Old Kingdom we find, however, recessing in one particular instance: the "false door" which forms a feature of these tombs in one of its two usual forms consists of a monolithic slab showing a door flanked by a standardized set of recesses on either side (Pl. I, D). The other form, a narrow doorway in which the figure of the dead appears sometimes underneath a stone imitation of the rolled mat with which it could be closed,⁴⁰ need not be considered in this context.

It is clear that the elaborate form of the false door, the "great door" or "palace façade door," is related to the brick architecture of the First Dynasty; moreover, it is sometimes covered with a painted decoration⁴¹ which is similar to that used in some tombs with recessed walls (Fig. 12). But what is that relationship? Because the false door is a typically Egyptian feature of the tombs, well known and long in use, Egyptologists have often been inclined to make this the basis of an explanation of the First Dynasty tombs with recesses. This procedure is, however, illegitimate.

The "false door" or "Ka-door" served to mark the place where offerings to the dead were made and where, in a general way, communication with the departed took place. This idea is thoroughly Egyptian. In fact, it seems to go back to the broadest Hamitic substratum of Egyptian civilization, since North African tumuli have a niche which seems to fulfil the same purpose.⁴² Some very simple graves of the First Dynasty at Tarkhan (Fig. 7) have a pair of slits in the brick

³⁹ See n. 17.

⁴⁰ E.g., tomb of Mereruka at Saqqara, or Iduw at Gizeh (*Boston Bulletin*, XXIII, 13).

⁴¹ The best-known example is N. de Garis Davies, *The Mastaba of Ptahhetep and Akhetep*, Pls. XIX and XX-XXA.

⁴² Baumgaertel, *Dolmen und Mastaba* (1926), Figs. 4 and 7. Balcz's objections, endorsed by Wolf, are quite irrelevant. Of course, there is no direct connection between the prehistoric African stone buildings with Egyptian stone architecture. What matters, however, is the common idea underlying both—namely, that the structure of the tomb should mark in some way how communication with the dead is maintained; and, furthermore, the specific architectural form given to that idea in the arrangement of a niche in the tomb.

wall of their superstructure in front of the face of the dead, and the tomb chapel where the offerings are made is constructed at this place against the tumulus. An earlier expression of the same idea may be found in a burial jar from Maadi pierced by two holes.⁴³ A later instance is the serdab in Zoser's funerary complex at Saqqara, which shows two slits in front of the statue's face. Again, in the tombs of the Fourth Dynasty at Gizeh there are perforations in the large stone which closes the entrance to the burial chamber, and behind these openings is placed the "reserve head" of the dead.⁴⁴

All these cases evidently present expressions of the same idea which found its final form in the "false door." Even the fact that two of these doors are often found may well go back to the very primitive use of two slits corresponding with the two eyes. The presence of two doors is generally explained as an imitation of royal usage. The king of Upper and Lower Egypt would have required two false doors in his tomb. But two false doors are found already in tomb Q.S. 2105 at Saqqara, a private tomb of the First Dynasty (reign of Zer), and it seems hazardous to project the adoption of royal usages by private people, traceable from the Late Old Kingdom onward, back into the very period when the united kingship arose. The derivation from a pair of eyeholes of a prehistoric period seems less forced, and the retention of both doors (even after they had become large structures, placed at opposite ends of the wall) would be a characteristic manifestation of that curious Egyptian, or rather Hamitic, tendency toward a dualistic scheme of things.⁴⁵

Whether we find one or two false doors in any given tomb, whether

⁴³ *MDIAA*, V, Pl. XIX b 1.

⁴⁴ Junker, *Giza I*, p. 43.

⁴⁵ The dichotomy and polarity which Meinhoff (*Die Sprache der Hamiten*, p. 20, n. 1, *et passim*) found in the structure of Hamitic languages is also noticeable in such features as the consistent preservation, beyond all actuality or reason, of the view of Egypt as "the Two Lands"—whatever the historical basis of that designation may have been originally. It appears, moreover, in the manner in which the conflict between Horus and Set becomes the pretext for an exceedingly farfetched system of identification of a number of objects with one or the other of these protagonists (in which Osiris may take the place of Horus, of course). With this in mind a scene like Sethe, *Festspiel*, 141 ff., becomes most illuminating. It illustrates to a nicety the "polarity" which Meinhoff observed in Hamitic grammar. The relation between Ka and man, with the respective physical substrata, such as we find also in the treatment of body and placenta in Uganda (*Man*, 1911, pp. 97 ff.), and similar features, all deserve study as a manifestation of this curious fundamental dualism in the minds of the Egyptians.

they are of the simple or of the elaborate type, it is certain that they marked the place of communication with the dead. No architectural form could be less suitable to serve that purpose than the all-around recessing of the brick tombs of the First Dynasty. It is impossible to explain this type of building as a development of the "false door." The latter marks one particular spot as important; the other presents a series of identical features rhythmically articulating the whole of the available wall surface.

It has been suggested that the all-around recessing expressed the idea that the dead was entirely free to "come forth" and go wherever he wanted.⁴⁶ Even if the Egyptians of the First Dynasty had interpreted the complete recessing in this manner, it would leave us with the unsolved problem of their sudden ability to erect highly sophisticated structures of a material not used by them hitherto or, at least, not used on any considerable scale. On that assumption it would be easier to imagine that some such interpretation made foreign buildings of recessed brickwork attractive models for them to follow. But even this hypothesis has little in its favor, for we have no need to invoke the interpretation of "Ka-doors" to explain the appearance of recessing in Egypt. We have seen that many technical details suggest that it was in this accomplished form that the large-scale use of bricks became known in the Nile Valley.

Moreover, this symbolical function of marking the place of exit of the dead is by no means the most important role which the "false door" plays in tomb architecture. It marks the place of communion with the dead and has, therefore, its significance for the living as well as for the deceased. The perforations in the funerary pot found at Maadi, and those in the stones closing the burial chambers of the Fourth Dynasty tombs, are examples of purely symbolical features, since they appear in places to which the living did not have access. But in the superstructure of the tomb we must expect the main features not only to symbolize the status of the dead (namely, his ability to "go forth") but also to be related to the acts with which the living maintain their relationship with him. Their part in the communion also requires tangible expression. The two slits in the simple First Dynasty tombs of Figure 7 are as much determined by the require-

⁴⁶ Reisner, Balcz, and others.

ment of the celebrants of a formal indication in the structure itself that their acts and words are addressed to the relative buried within, as by the thought of the corpse's two eyes. But the "false door" represents the mature expression of this twofold significance of the place of communion. Standing out in the unbroken wall, it formed as compelling a focus for the ritual as the altar in a church. In its form as "door" it continually stimulated in those gathered in front of it the consciousness that they addressed one mysteriously surviving death. The impenetrable "Ka-door" is magnificently adequate to its function.

But the same reflection which reveals the adequacy of the "false door" shows that its efficacy would be diminished if it were placed within a wall composed of a series of ornamental recesses. If we should accept the theory that recessing is derived from the "false door" and that each recess in fact represents a possible place of exit for the dead, then we are led to the absurd assumption that the ancients had chosen to enact their ritual in front of a partition resembling a modern row of telephone booths—hardly conducive to the concentration of thought and feeling which the occasion demanded. We shall see that one hybrid form of this type is known, but no conceivable development leads from the "false door" to the recessed monuments of the First Dynasty.

Moreover, the latter antedate the appearance of the "false doors" by several reigns. If the underlying *notion* of the "Ka-door" is an immemorial part of Hamitic funerary requirements, its *form* is evidently derived from the recessed architecture which appears, ready made, at the very beginning of the First Dynasty. Thus the relation between "Ka-door" and recessed brick buildings is exactly the reverse of that usually assumed. In fact, we can follow in some detail how the Egyptians adapted the new architectural style to embody the idea of the "Ka-door" until, at a later time, they abandoned recessing as altogether unsuitable. We have at least two instances where we observe how the Egyptians, confronted with a building ornamented all around with identical and equivalent niches, singled out one to serve as a place of communication. At Tarkhan this has been accomplished by putting a wooden floor in one of the niches (Fig. 3). At Naga ed Deir one of the niches is more elaborate than the others, and the deepest

recess is painted red to indicate the wooden leaf of a door.⁴⁷ Both tombs belong to the First Dynasty.

These attempts to superimpose the "false door," required by indigenous custom, upon an architectural form which is characterized by the absence of emphasis on any one point of its circumference are easily explained by our view that Mesopotamian buildings, like our Figures 5 and 6, served as prototypes. For these were neither tombs nor cenotaphs but were accessible through doorways placed in some of the recesses. The Egyptians needed merely to make one of these doors into a "Ka-door." But it is interesting to note that this style of architecture was soon given up in Mesopotamia too, and evidently for the same reason as it was abandoned in Egypt. It was apparently felt to be unsatisfactory that the door, a feature of outstanding functional importance, did not receive any architectural emphasis but was hidden in one among several recesses. This interpretation of the change is no mere modern postulate, for the new Mesopotamian style, appearing by the end of the Jemdet Nasr period and characteristic for the Early Dynastic age, made the entrance the most striking detail in the building's silhouette. The all-around recessing was very much simplified, and the elaborate niches were reserved for the towers, which now flanked the entrance and thus accentuated its presence in the wall. The "Ka-door" required no less adequate architectural expression than the real doorways of the Mesopotamian temples, and it is clear that the makeshifts of Tarkhan and Naga ed Deir could not permanently satisfy the Egyptians. They found the solution in one or two "false doors" standing out in an otherwise unbroken wall surface.

The form of the "false door" derives from that of the early brick recesses, at least in the case of the "palace façade" variety (Pl. I, *D*), but it is important to realize that, architecturally, the tombs with "false doors" represent the total abandonment of recessed building. There is no gradual development from the one to the other as is so often assumed,⁴⁸ but, on the contrary, we observe the victory of one form over another which had descended from the very first monumental buildings erected (as far as we know) in the Nile Valley. The form

⁴⁷ Reisner, *Tomb Development*, p. 292.

⁴⁸ So Junker in *Giza II*.

with two "false doors" is known already in the First Dynasty,⁴⁹ and Reisner has proved that it remained continuously in use until it became the prevalent type in the mature Old Kingdom.⁵⁰ In other words, the method of all-around recessing is simply superseded.⁵¹

5. The issue is somewhat confused by the existence of a few tombs which show the old recessing, though on one wall only. And this wall is, moreover, mostly part of an inner corridor and not an outside wall. The tomb of Hesire at Saqqara is the best known of these atypical tombs, and there are some of Fourth Dynasty date,⁵² while in the provinces (notably at Denderah) they enjoy a local vogue down to the end of the Old Kingdom.⁵³ The first appearance as well as the distribution of this type shows that it is not part of the main stream of architectural development. It is not transitional but hybrid. The recessed wall in Hesire's tomb contains in the depth of each recess a finely carved wooden panel showing the standing or striding figure of the dead man. Since Egyptian relief does not distinguish between the two attitudes, it is possible to maintain that he is supposed to come forth from each recess. The disadvantages of such a fiction were discussed above, and, in any case, the need to mark clearly the focus of the ritual enacted in the tomb existed here, too, and was met by showing the dead man, not standing, but seated at his funerary meal, in the central recess of the wall. The difference from the other recesses is, of course, too slight to be really adequate; it seems that Hesire had been desirous of combining the rich effect of the recesses with the "Ka-door" as the place where the dead and the survivors communicated, though the two requirements are incompatible. For the significance of a door consists in its position within an impermeable wall, and the logic and

⁴⁹ Saqqara, Q.S. 2105.

⁵⁰ Reisner, *Tomb Development*, pp. 238-43.

⁵¹ There is no need to refer to the problem of the square stone showing the dead at table and which forms part of the "false door" in most Old Kingdom tombs, except for the fact that it is often treated together with the history of tomb architecture. Scharff, for instance, makes here again a distinction between Upper and Lower Egypt, while Reisner connects his "slab-stela" or "niche stone" with the Abydos stelae (both in *Studies Presented to F. Ll. Griffith*). See also H. W. Müller, in *MDIAA*, IV, 165 ff. and Junker in *Giza I*, pp. 13 f. and *Giza II*, pp. 14 f. *Annales du service*, Vol. XIII, Pl. IV shows how at Giza too the combination of the funerary chapel (made of bricks) and the mastaba produced the effect of a "false door."

⁵² Reisner, *Tomb Development*, pp. 282-87.

⁵³ Petrie, *Denderah*, Pl. 29.

clearness of the First Dynasty solution where two slits were made near to each other in a smooth wall (Fig. 7) and of the usual Egyptian usage of showing one or two "false doors" in an otherwise unbroken surface naturally ousted the recessing from tomb architecture.

It is significant that the one application of recessing which found a place in the normal Egyptian tomb served to isolate the "false door" and to enhance its effect. This use of recessing is found in the "Ka-door" in its elaborate "palace façade" form; it consists of a narrow slit, the actual door, flanked on either side by a standardized set of recesses, the whole cut into a monolithic slab of stone (Pl. I, D). A new, composite unit had been created, and the recesses were no longer by themselves the elements of decoration.⁵⁴ Thus the recessed building suddenly introduced under the first kings of the First Dynasty leaves its trace in the form of the "false door." To start from the latter, though it is better known, and to attempt to explain the First Dynasty building in that way, means putting the cart before the horse.

III

We have found the current explanations of the recessed architecture of the First Dynasty inadequate, even irrespective of the fact that they fail to account for the contemporary construction of similar buildings in Mesopotamia. If, on the other hand, we assume that the Egyptians of the First Dynasty derived from Mesopotamia the peculiar form of brick building which is the first to appear in Egypt, all facts fall quite simply into place and become understandable. Now this assumption does not need to be made *ad hoc*. There is a considerable number of phenomena which require for their explanation a somewhat sustained and elaborate acquaintance, on the part of the Egyptians, with Mesopotamian culture. Several of these have long been known; others have recently been placed in their true significance by discoveries in Mesopotamia.⁵⁵ We here enumerate them once more

⁵⁴ This new unit is, in its turn, used decoratively in some sarcophagi like that of Mycerinus, which are decorated all around with "false doors" of the elaborate type. Note that in contrast with the recesses in Hesire's tomb the decoration of these coffins had no ritual function, since the living never approached them.

⁵⁵ See Scharff in *ÄZ*, LXXI (1935), 89 ff. His synchronistic table can be simplified in the manner indicated in our text, and his interpretation of the evidence from the seals seems incorrect but can now be dispensed with in any case. It is unfortunate that the relief of shell which he discussed in a separate article (*MDIAA*, VI, 103 ff.) must be ex-

in a somewhat systematic form because their implications are clearer when it is realized that they do not represent a random collection of resemblances:

MESOPOTAMIAN INFLUENCE IN PRE- AND PROTODYNASTIC EGYPT

I. EVIDENCE OUTSIDE THE FIELD OF ART

- a) Mesopotamian objects found in Egypt:
 1. *Three cylinder seals* of the Jemdet Nasr period
- b) Mesopotamian usages temporarily adopted in Egypt:
 1. *Sealing* with engraved cylinders
 2. *Recessed brick-building* for monumental purposes
- c) Mesopotamian objects depicted on Egyptian monuments:
 1. *Costume* on the Gebel el Arak knife handle
 2. *Scalloped battle-ax* on fragment of late predynastic stone vase
 3. *Ships*, on Gebel el Arak knife handle, "decorated" vases, stone vase, and ivory labels of First Dynasty

II. EVIDENCE IN THE FIELD OF ART

- a) Mesopotamian motives depicted in Egypt:
 1. *Composite animals*, especially winged griffins and serpent-necked felines, on palettes and knife handles
 2. *Group of hero dominating two lions*, on Gebel el Arak knife handle and in tomb at Hierakonpolis
 3. *Pairs of entwined animals*, on knife handles and Narmer palette
- b) Mesopotamian peculiarities of style apparent in Egypt:
 1. *Antithetical group*, on knife handles and palettes
 2. *Group of carnivore attacking impassive prey*, on knife handles
 3. *Drawing of musculature*, on Gebel el Arak knife handle

In comment something has first to be said about the time at which the intercourse between the two countries took place. The imported Mesopotamian cylinder seals obtained in Egypt are particularly valuable in this respect⁵⁶ since they are typical for the Jemdet Nasr period and unknown in the preceding Uruk period of Mesopotamia. And

cluded altogether. It is certainly the copy in hard material of a tablet of the Jemdet Nasr period with two seal impressions and some numerals. But its connection with Egypt consists merely in the hieroglyph *mr* which Scharff saw in the engraved sign on one face. Closer scrutiny, however, reveals that it is no such thing, but an ordinary picture of the Mesopotamian plow! If the object is turned on its side with the left side as base, we clearly see on the right of the engraving the small crossbar at the end of the pole, below it the colter, and, on the left, somewhat more vaguely, the two handlebars and the seed funnel. Compare the clear renderings in, e.g., my *Cylinder Seals*, Pls. XXI f; XX d; XXI e.

⁵⁶ Frankfort, *op. cit.*, p. 293.

this useful distinction cannot be made in respect to most of the other features listed here which, as far as we know at present, may occur in either period.⁵⁷ We have, however, already seen that the presence of towers flanking the entrance is similarly unknown before the Jemdet Nasr period.

Moreover, there is entirely independent evidence to prove the synchronism. This derives from the work of the Syrian Expedition of the Oriental Institute and more especially from the stratigraphical work carried out by Mr. Robert Braidwood, to whom I am much indebted for permission to use the information. In certain layers at Tell Judeidh, seals of the Jemdet Nasr period were discovered, as well as pottery of the type which was found as importations in the tombs of certain First Dynasty kings in Egypt and notably of Zet. A great deal of corroborative evidence, involving Palestine also, cannot, of course, be discussed here, but the fact that the Jemdet Nasr period is contemporaneous with the Late Predynastic period and the early part of the First Dynasty must be considered proved.

It should be remembered that this conclusion is quite independent from the material which is the subject of this paper. It should also be remembered that Mesopotamian influence upon Egypt would be easier to explain in the Jemdet Nasr period than at any other time. For this was an age of great Mesopotamian expansion; its tablets and seals have been found not only in Elam but on the central Iranian plateau as well, at Siyalk near Kashan; its cylinder seals occur as far afield as North Syria, Anatolia (Alishar and Troy), and even in the Cyclades.⁵⁸ Thus their occurrence in Egypt, and in fact the very varied signs of Mesopotamian influence at this time, harmonize closely with the known trends of the period.

The evidence in our table which does not concern art is exceptionally important because stylistic testimony is often subject to suspicion. It might be argued, for instance, that the antithetical group was so well in keeping with the general tendency toward a stricter order in politics, art, and religion that its temporary adoption needed no fur-

⁵⁷ The group of the hero between animals is not known on cylinder seals before the Second Early Dynastic period but occurs in exactly the same form as on the later seals on a sculptured stone vase of the Jemdet Nasr period found by us at Tell Agrab (*Illustrated London News*, September 12, 1936, p. 134, Fig. 16).

⁵⁸ Frankfort, *op. cit.*, pp. 227 ff.

ther explanation. This point of view cannot account, however, for the appearance of a hero in purely Mesopotamian costume on the Gebel el Arak knife handle⁵⁹ nor for the scalloped ax which appears on a sherd of a late predynastic stone vase⁶⁰ but which was only adopted in Egypt by the end of the Old Kingdom although it was known in Mesopotamia from the beginning of the Early Dynastic period, and possibly used earlier; nor, as we have seen, for the sudden appearance of a sophisticated form of architecture not used in Egypt on any scale before this age, while it was indigenous from very early times in Mesopotamia.

But even the purely stylistic resemblances are in their totality more significant than a mere accidental group of resemblances. If we exclude the rendering of musculature which distinguishes the hero on the Gebel el Arak handle from the undifferentiated limbs of the usual predynastic figures, we notice that the features which we claim to be derivative in Egypt not only resemble individual Mesopotamian designs but possess one feature in common which is as characteristic for Mesopotamia as it is alien to Egypt: they are all pronouncedly unrealistic. By their predominance of imagination or design over probability or nature, they illustrate the remarkable power of abstraction of the Mesopotamians. The same mentality made the pictograms which underlie Mesopotamian writing lose almost at once all resemblance to the objects which they depicted, to end in the entirely abstract wedge groups of cuneiform writing. Egypt, starting also from pictograms, retained throughout the clear pictures which we call hieroglyphs and even exploited their representational character in later times by puns or "enigmatic writing."⁶¹

The disappearance of the features enumerated in our table becomes understandable also if we remember that they are derivations. Already in the Third Dynasty, brick gives way to stonework for monumental funerary structures. The seal cylinder is replaced by the scarab for all practical purposes after the Old Kingdom. Of the other features in our list, not one survives the early reigns of the First Dynasty.

⁵⁹ For this monument, and all others referred to, see Scharff's article quoted in n. 55.

⁶⁰ Scharff, *Mitteilungen aus der aeg. Sammlung*, Vol. V, Pl. 22, No. 108.

⁶¹ Enigmatic script: E. Drioton, "La Cryptographie égyptienne," *Revue Lorraine d'anthropologie*, Vol. VI; puns: e.g., Griffith, *Beni-Hassan*, Vol. III, Pl. VI, 82.

Contact with Mesopotamia may have stopped then, and in any case the vitality of Egypt was such that it would certainly not retain unassimilated foreign matter. It would be absurd to consider Egyptian civilization in any way derivative. There is no necessity to assume Mesopotamian influence in order to explain the development of pharaonic civilization, but it so happens that we have evidence that such influence was, in fact, exercised. Our view is not, therefore, a theoretical postulate but a conclusion based on observed facts which seem to suggest it.

This influence from the east may well have worked merely as a catalyst. It is curious that there is no evidence of contact on the Mesopotamian side. Since it was exercised, apparently, via the Red Sea route and affected Egypt most noticeably in the region around the terminus of the Wadi Hammamat, it is possible that some thoroughly "Mesopotamian" culture on the Persian Gulf or Arabian coasts was the center involved on the Asiatic side and that such signs of intercourse with Egypt as might be preserved there never reached Mesopotamia proper and have also evaded the modern excavator. However that may be, there can be no doubt that intercourse did take place and that it greatly stimulated Egypt during the formative phases of its own culture.

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THE OLD ARAMAIC ALPHABET AT TELL HALAF

THE DATE OF THE "ALTAR" INSCRIPTION

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In 1931 Baron von Oppenheim, reporting on his excavation at Tell Halaf, wrote: "In der Kaparaschicht fanden wir ein kleines Kalkstein-altärchen mit einigen wenigen, leider nicht entzifferbaren altsemitischen Schriftzeichen."¹ No further stratigraphic detail has been released on this piece which, therefore, must be discussed apart from any exact archeological context. Meissner, from the character of the cuneiform signs, peculiarities of language, and the point of view expressed in the short cuneiform inscriptions of Kapara, feels that a date in the twelfth century B.C. is fitting for the Kapara stratum.² Von Oppenheim insists that the date of the small finds of the stratum and the views of B. Landsberger with regard to the earliest possible presence of Aramaeans in Mesopotamia also support the twelfth century B.C. date.³ The twelfth-century date has been widely accepted⁴ but not without considerable opposition.⁵

The publication of the Kapara Aramaic inscription, long awaited, has finally appeared as the work of Johannes Friedrich.⁶ A hand copy of the inscription is given (Fig. 1) but, unfortunately, no photograph. Friedrich transliterates it as כרר | זי | [[(x)צ] בעמ | זדמת, with re-

¹ M. F. von Oppenheim, *Der Tell Halaf* (Leipzig, 1931), p. 65; cf. English translation by G. Wheeler, p. 70. The expanded French edition, which I understand is in preparation, is not available.

² B. Meissner, *apud* von Oppenheim, *op. cit.*, p. 266 (English ed., p. 316). S. Langdon, *Oxford Magazine*, June 15, 1933, p. 812, on the basis of the cuneiform signs, dated the Kapara inscriptions to the tenth century B.C. There can be no certainty in dating based on cuneiform script alone.

³ Von Oppenheim, *op. cit.*, p. 64 (English ed., p. 68).

⁴ Cf. W. F. Albright, "The Present State of Syro-Palestinian Archaeology," in *The Haverford Symposium on Archaeology and the Bible* (New Haven, 1938), p. 27. Albright, however, modifies the date to "twelfth or eleventh century."

⁵ For a summary and bibliography of some of the outstanding opponents of the Meissner-von Oppenheim date see E. Herzfeld, "Der Tell Halaf und das Problem der hettischen Kunst," *Archaeologischen Mitteilungen aus Iran*, VI, Part 3/4 (1934), 112 ff.

⁶ J. Friedrich, A. Ungnad, G. R. Meyer, E. F. Weidner, *Die Inschriften vom Tell Halaf* (Beiheft zum *Archiv für Orientforschung*, Vol. VI [Berlin, 1940]), pp. 68-70 and Pl. 29.

gard to translation, says: "Die Kürze des Textes und einige Beschädigungen erschweren die Lesung, eine Übersetzung der paar zusammenhängenden Zeichengruppen ist leider nicht möglich."

Concerned about the distribution of letters in the first group, he suggests that the first letter might be the demonstrative pronoun and the remainder the Akkadian *dimtu*, "pillar"; but he is troubled by disagreement in gender. For the second group of letters he has no suggestion but remarks that, after the *mem*, "Dann beginnt die Zerstörung um die Kante herum, in der etwa ein bis zwei Zeichen Raum hätten, doch ist möglicherweise auch diese Kante ebenso schriftfrei gelassen worden wie die folgende." His copy, however, shows no such extensive damage. He rightly reads the next letters as the Aramaic relative pronoun, which classifies the language of the inscription. The final

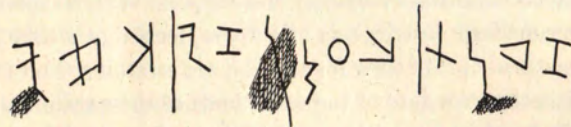


FIG. 1

group he reads as two words and translates "denn lebend" or "denn erlebte."

The decidedly anachronistic forms of some of the letters disturb Friedrich. His *beth* he recognizes as much too late a form for the *delta* and *mem*, which suggest the old Phoenician characters from Byblos. He was tempted to read an archaic *aleph* retrograde or possibly a *kaph*, such as occurs later in the inscription, but finally abandoned both as less likely than an open-headed *beth*. The next to last letter he regards as an unusual *heth* bearing only two bars. He should rightly be concerned over such troublesome letters, for they certainly suggest that the relationships with the Byblian script, proposed for the remainder of the inscription as supporting the doubtful date of about 1200-1100 B.C. for Kapara, must be called into question.

The results I achieved when I copied the same inscription in the Tell Halaf Museum, through the kind permission of its owner, were quite different (Fig. 2) and would lead to another conclusion with regard to its date. My transliteration reads⁷ כתי | די | כתי .

⁷ The reading *beth* is discussed below.

It will be noted that in my copy those anachronistic letters which seemed to link this piece with the earlier inscriptions from Byblos disappear completely. Zigzag lines, which would produce the "Byblos" *mem*, are, as epigraphers know, perhaps the most common of copyist errors.

There remains doubtful, however, the second letter, which is incomplete. The form at once suggests that it should be completed as a *delta*-form *daleth*. Such a letter, when compared with the others, would be slightly anachronistic, although not absolutely impossible. It is significant for dating that such a *daleth* is found in Phoenician inscriptions as late as the eighth century B.C. along with forms showing an incipient staff. Such letters are found on the bronze fragments of the Ba'al Lebanon bowl from Cyprus which mentions Hiram (II) of Tyre.⁸ By

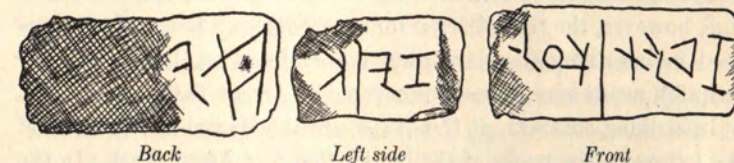


FIG. 2

this period the earliest Syrian inscriptions from Zenjirli, written in a much more cursive style, have forms with a very short staff.

The defective letter might be completed in another fashion, however. Only a short horizontal stroke, easily omitted by a careless writer, needs to be added to convert the letter into a *beth* that would be consistent with all the other letters of the inscription. This single stroke would at the same time convert an awkward combination of letters into an intelligible and plausible word. It is highly probable that the letter is to be read as *beth* rather than as *daleth*.

More damaging for a theory of an early date for this writing is the form of the *kaph*. The letter appears in the early Byblos script as a fan-shaped triad of equal lines joined at a point below. This form oc-

⁸ CIS, I, 5, Pl. IV, Frags. A, D, and F of the best-preserved bowl. This inscription has been dated by Lidzbarski to a period close to the second millennium B.C. (*Handbuch der Nordsemitischen Epigraphik* [Weimar, 1898], p. 176). Z. Harris, *A Grammar of the Phoenician Language* (New Haven, 1936), p. 157, dates it with some question to the ninth century B.C.; while A. T. Olmstead, *History of Palestine and Syria*, p. 434, and *History of Assyria*, p. 183, places the bowl in the time of Tiglath-pileser III.

curs regularly as late as the Eli-Ba'al inscription (925-889 B.C.) from Phoenicia⁹ and would be expected in the Kapara inscription if it showed affinity with the early alphabet. The Kapara forms most closely resemble the early Greek *kappa*'s from the island of Thera, dated to the eighth and seventh centuries B.C.¹⁰ Similar forms, but with longer staffs, are found on the Lebanon bowl mentioned above. The cursive *kaph*'s of the Mēsha' and Kilamū inscriptions are morphologically a bit later in their development than that of the Kapara legend, since the trend appears to be in the direction of a longer staff.

Most critical for dating, too, is the *heth* with two bars¹¹ which Friedrich recognizes as having no parallel in the early Byblos inscriptions, where a three-barred variety prevails.¹² In Moab the two-barred form is found as early as the last half of the ninth century B.C. and is regularly so written in the Moabite stone. In Syria and Palestine, however, the three-barred form persists for a longer time. It is used in inscribing the Samaritan ivories¹³ and on the Aramaic inscription mentioning Hazael discovered at Arslan Tash.¹⁴

In striking contrast, at this point, are the Hazael inscription and the letters on the backs of the ivories found at Arslan Tash. In the small group of the latter there are three *heth*'s of the two-barred variety to one with the three bars.¹⁵ If all these ivories are assumed to belong to the same piece of furniture, they would form a transitional stage in writing such as is represented by the Samaritan ostraca where, however, the three-barred form is still the most frequent.¹⁶ The recent tendency to date the ostraca to the eighth century B.C.,¹⁷ rather than

⁹ R. Dussaud, *Syria*, VI (1925), Pl. XXV and copy on p. 109.

¹⁰ H. Roehl, *Imag. Inscr. Graec. Anti.* (3d ed., 1907), I, No. 6, 2.

¹¹ In my copy the mark within the letter indicates a rather deep hole.

¹² There is the possibility of a single two-barred form in the Yehimilk inscription from Byblos of the twelfth or eleventh century B.C. (cf. M. Dunand, *Revue biblique*, Vol. XXXIX [1930], Pl. XV, l. 1), but in the light of the preponderance of evidence, even within the inscription itself, this isolated form must be considered an error.

¹³ E. L. Sukenik, "Notes on Hebrew Letters on the Ivories," *apud* J. W. and G. M. Crowfoot, *Early Ivories from Samaria* (London, 1938), p. 7.

¹⁴ F. Thureau-Dangin, A. Barrois, G. Dossin, and M. Dunand, *Arslan Tash* (Paris, 1931), p. 135, Fig. 49 and Pl. XLVII, No. 12.

¹⁵ *Ibid.*, p. 91, Fig. 33, Nos. 2, 26, 27. Only No. 5 has three bars.

¹⁶ G. A. Reisner, *Israelite Ostraca from Samaria* ("Harvard University Palestinian Expedition"), Pl. VI, Nos. 22 and 24; Pl. VII, No. 26; Pl. IX, No. 31.

¹⁷ Cf. R. Dussaud, *Syria*, XVI (1935), 211; H. L. Ginsberg, *Archiv orientální*, VIII (1936), 145; W. F. Albright, *From the Stone Age to Christianity* (Baltimore, 1940), p. 314, n. 17.

to the ninth, is interesting as suggesting a probable date for the letters on the Arslan Tash ivories.

The ninth-century date for the inscribed ivories of Arslan Tash, suggested by Sukenik on the basis of their association with the Hazael inscription,¹⁸ deserves to be reconsidered. Not all the ivories were part of the same piece of furniture, datable by the Hazael name. Only one of the inscribed ivories, and that without a *heth*, is definitely mentioned by the excavators as being found in proximity to the Hazael inscription and the remains of the couch dated by it. There is a distinct difference between the forms of the *heth*'s of the inscription and those of the letters on the backs of some of the ivories. The latter seem to be later than those on the Hazael ivory. The furniture from which the inscribed ivories came may have been made later than the days of Hazael and may have been brought to Arslan Tash, then the royal city Hadatu, after the fall of Damascus in 732 B.C.

The suggestion that the Arslan Tash ivories may have come from the west, possibly from Cyprus, recalls the fact that the earliest evidence for the two-barred *heth* in the Greek inscriptions comes from the texts written in Eleutherna, Crete, in the seventh century B.C.¹⁹

In Syria proper, at Zenjirli, the three-barred *heth* persisted until the time of Bar Reku (ca. 725 B.C.), in whose inscriptions the double-barred type is the rule.²⁰ The definite two-barred form in the Kapara inscription, together with the other evidence mentioned above, definitely eliminates the Kapara letters from being considered as contemporary in date with the early Byblos script.

What, then, is the date of the Kapara inscription? As indicated above, the letters critical for dating are the *kaph*, *heth*, and possibly the second letter, even if it be completed as a *daleth*. Comparison with the letters on the Moabite stone suggests a date in the last half of the ninth century B.C. If, however, the geographically more remote Moabite letters be discarded in favor of Syrian and Palestinian letters as criteria for dating the Mesopotamian forms, a definite lag in the development of some forms of the letters must be considered, particular-

¹⁸ Sukenik, *op. cit.*, p. 8.

¹⁹ F. Halbherr and D. Compagetti, *Mus. ital. ant. class.*, II (1888), 162-63, Nos. 1a, N2 and N3.

²⁰ F. von Luschan, *Ausgrabungen in Sendschirli*, IV (1911), 379, Fig. 275.

ly with that of the *heth*. Under such circumstances comparison would be with materials of the eighth century B.C., some of which are even as late as the time of Tiglath-pileser III. If the second letter be completed as a *beth*, the argument for the later date would be strengthened. It would be safe, therefore, to date the Kapara inscription to the last half of the ninth or to the beginning of the eighth century B.C.

Mr. Braidwood, of the Oriental Institute, whom I have consulted on the possible date of the Kapara material in the light of Syrian stratigraphy, writes:

In view of the fact that the great bulk of late material excavated by the Oriental Institute's Syrian Expedition in the Plain of Antioch is still not ready for publication, I venture to append this note to Dr. Bowman's argument. Unfortunately, the reader has no choice but to accept my judgment in comparative archeology in this matter until the Syrian Expedition publication appears. It is, however, possible to say quite categorically—as will be evident when this material is published—that the bulk of the Tell Halaf small finds, published as of the "Kapara layers" or so noted in the Tell Halaf Museum in Berlin, correspond exactly with the material of Amouq phase O (=Judaidah IV), and especially with the later aspects of the phase known best on Tell Tayinat (*AJA*, XLI [1937], 8-16; *OIP*, XLVIII, 6-7). The comparisons involved are matters of detailed similarities, not merely superficial likenesses. The following list of comparisons which are to my judgment valid, present on Tell Halaf ("Kapara layers") and our site, must suffice for the moment.

1. Architecture: "palaces" of the normal "Hilani" plan, embellished with architectural sculpture. Wooden timbers used to bond thick mud-brick walls (cf. *Tell Halaf*, p. 80; p. 82 for plans). Similar gate constructions. A great terrace-like mass of mud bricks, appended to the second phase of the "Hilani."

2. Graves and burial customs: General use of cremation with ashes put in pots along with gifts (by inference at our sites; no burials at all were found, but cf. Hama E-F and Carchemish Iron Age graves). Later in the period inhumation practiced, at least in part.

3. Ceramics: A red-slipped and burnished series in which appear tripod bowls, the mesomphalic phiale form, the horizontal bar handle, characteristic rim profiles, some incised decoration, and a black-ware variant. Painted wares of Cypriote Iron Age type. Painted wares in which the sub-Mycenean motifs are localized (cf. Hama F and Carchemish Iron Age). Simple wares of "Assyrian" type, some very fine and with intentional depressions in the surface, or decorated with "jewelry" impressions. Assyrian glazed wares in two or three colors. A geometric petal motif. A frit bowl of gadrooned phiale

form (cf. Tayinat in red-burnished pottery, Hama E and Carchemish graves, in bronze). "Zigattu" decorative knobs (cf. *Tell Halaf*, Pl. LV and Fig. 4, p. 310).

4. Metals: The first general use of iron in Syria. Bronze bowls, some fluted (gadrooned phiale?), small cups which flare slightly at the lip. Fibulae. Hollow gold earrings with "knobs." Stone-set rings with granulated setting borders (cf. *Tell Halaf*, Pl. LVII, Pl. III [color]).

5. Stone objects: Basalt bowls with tripod legs and fluted sides. Tall "censer" shapes. Duck weights. Rectangular bowls with animal heads (cf. Khorsabad; *Tell Halaf*, Pl. XLIX).

6. Bone: Decorated cosmetic box lids. Boxes with five compartments (in steatite at Tayinat) (cf. *Tell Halaf*, Fig. 21b, p. 221).

Certain points pertinent to the chronology should be noted. Baron von Oppenheim and his colleagues have not yet offered their definitive publication, but in *Tell Halaf* they refer all the material listed above to the "Kapara layers," sometimes qualifying the latter by adding "and their Assyrian successors." They go beyond this generalization in the matter of the graves, which they explicitly state were used by Kapara and collateral personalities. It is therefore important to note that a good proportion of the items from *Tell Halaf* listed above come from the graves. Two shaft graves were actually sealed by a great mass of mud bricks southeast of the Halaf "Hilani" or "Temple-Palace." Since both of the latter structures also are called "Kapara dynasty" in date, we may be quite certain that—in the minds of the excavators—the tomb material is of Kapara date with no admixture from the "Assyrian" or "Guzana" layers. Schmidt (*Tell Halaf*, p. 311), who is quite specific about the ceramic content of the tombs, describes essentially what is listed above.

There is excellent evidence that the material from Tayinat listed above covers the range between 850 and 600 B.C. It is certainly from the middle-later part of our phase O (=Judaidah IV). The material of the earlier part of that phase is distinguishable from it on clear typological and stratigraphic grounds, and it is absolutely nonexistent in the previous phase N (=Judaidah V), dated by us to between 1200 and 1000 B.C. on the basis of a splendid series of "Sub-Mycenean" pottery. The Tayinat material of mid-late phase O comes from a time when that town came under Assyrian influence and even occupation, and the Assyrian objects found are in no way disturbing.

It is, of course, possible that the complex of objects listed above could be slightly earlier in the Syrian hinterland than in the Plain of Antioch, where the period from 1200 to 1000 B.C. was so thoroughly taken up by the "Sub-Mycenean" peoples. A case in point would be Hama F, which shows painted pottery (with motifs copied from the "Sub-Mycenean" tradition) and the practice of cremation (heretofore strange in Syria and certainly another "Sub-Mycenean" trait) appearing together with the earliest red-burnished

wares which could not be from before 1000 to 950 B.C. in the Amouq plain. A similar material culture of mixed traditions seems to have been present at Tell Halaf. The more advanced forms, which are listed above and check in such detail with those of Tayinat (and, evidently, with Hama E), could not, in my opinion, be before 900 B.C. at the earliest. I strongly contend that the published material of the "Kapara layers" at Tell Halaf, sculpture and all, is almost directly contemporary to the period of large public buildings on Tell Tayinat, and that, like Tayinat, there is no gap whatsoever between it and the appearance of Assyrian material which is dated at Tayinat by an inscription of Tiglath-pileser III.²¹ Assyrian material is simply added to the sum total of the culture. My own opinions aside, this note needs do little more than remind the reader that Dr. Bowman's evidence will acquire impressive support when the later Plain of Antioch volumes appear.

Thus, by independent evidence, epigraphic and archeological, the Kapara stratum at Tell Halaf can be checked and dated.

The inscription on the *altärchen* is not illegible. There is little doubt about the majority of the letters. But it is difficult to be certain regarding the meaning of the combinations of letters. The initial letter is doubtless a prefixed demonstrative pronoun, but the group that it precedes is baffling if its first letter is to be read as *daleth*. The demonstrative suggests that the name of the object should follow. Unfortunately, the top of the object is missing, and there seem to be no parallels usable for its identification. That it is a miniature altar is possible. It may be a small household *massebah*.²² Such an object might be described as the "abode" of the god,²³ a reading that would result from calling the incomplete letter of the difficult group a *beth*. The next word would then be, by context, some divine name for which I can offer no parallel or identification. Then follows the one certain group, the Aramaic relative pronoun.

More difficulty is encountered with the final group, which rounds the corner. Should the letters be regarded as forming a single word?

²¹ Dr. Calvin McEwan calls attention to the fact that an inscribed fragment from the stratum at Tayinat bears the name of Kalparunda of Hattina, a contemporary of Shalmaneser III (858-824 B.C.). This find (cf. I. J. Gelb, *Hittite Hieroglyphic Monuments* [OIP, Vol. XLV], p. 39, No. 52) would push back the possible date of the stratum to a point where it would be in complete agreement with the earliest possible date for the Aramaic inscription under discussion.

²² A similar but much larger altar, also inscribed on its base, probably contemporary with the Tell Halaf piece, was found *in situ* just outside of a cult building on Tell Tayinat (cf. I. J. Gelb, *op. cit.*, Pl. LXXXIV, Fig. 53).

²³ Gen. 28:22.

If so, one at once thinks of the Hebrew *kōhî*, "my strength," which would produce an intelligible and appropriate, if somewhat exalted, sense, "This (is) the abode of . . . who (is) my strength." But *kōhî* is not found elsewhere in Aramaic except in the later Jewish Aramaic, where it doubtless occurs under Hebrew influence. If the meaning can be derived from the Ethiopic word "rock" (*kuakueḥ* < **kuahkueḥ*), sometimes cited as a cognate to the foregoing Hebrew word,²⁴ another fitting translation will ensue, "This (is) the abode of . . . who (is) my rock."

But the problem of translation is not so easily solved. If the *kaph* be regarded as a separate unit, as its presence on the side surface rather than on the back with the following letters might suggest, there would be scarcely another alternative to Friedrich's translation of the group as mentioned above. The matter is complicated by the fact, not indicated in the published copy but clearly suggested by my own, that there seems to be extensive damage to the back of the stone immediately after the last legible letter. We may have to translate, "This (is) the abode of *K^cI* who is like *ḤI*. . . ." One may have to reconstruct the last word, if it is incomplete, as a verb, but the radicals suggest nothing appropriate; one can only conjecture what it might be.

It is unfortunate that the meaning cannot be easily and unquestionably recovered. But translation in this case is not the most important factor. It is in the interest of establishing the text and its probable date that this is written. The piece is important beyond its mere text. Let us hope that the future will see both the significance of this inscription and the role of Kapara in the later Assyrian empire clarified.

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²⁴ Cf. Ges.-Buhl, *Handwörterbuch* . . . ¹⁴, p. 306.

THE STORY OF JERICHO

FURTHER LIGHT ON THE BIBLICAL NARRATIVE

JOHN GARSTANG

Revision in the dating of certain types of pottery from Beisan,¹ as proposed by Dr. Albright and accepted in principle by Rowe and FitzGerald, the excavators of that site, calls for a correlation of results elsewhere, including Jericho. Here it involves in chief a class of painted wares² decorated in monochrome or shaded duochrome with linear or triangular devices, which was formerly assigned to the first half of the Late Bronze Age but is now ascribed to the second half of that period, from the time of the pharaoh Seti I (ca. 1320 B.C.) into the next century.

Personally, I have always found it difficult to class these painted wares, and the common pottery associated with them, in the same culture phase as those which belong to the age of Thutmés III, as seen, for example, at Megiddo.³ The painted lines seem relatively thin and spidery and the original motives are tending to be lost: the snake, for instance, is often rendered quite conventionally by a wavy line. The tones used are more frequently only shades of a single color rather than contrasting and distinct; while the pottery itself, though for the most part well made, is thinner on the whole and of a paler shade and was made apparently on a faster wheel. The common pottery shows also noticeable variations: the tapering vessels are fatter and less elegant than of old, while the lamps show pronounced tendency toward reinforcement of the rim. I was also perplexed by the fact that none of these painted wares appeared in the tombs; and I did in fact class them at first as LB II; but the evidence from Beisan then generally accepted made it necessary to find a place for this group before the close of LB I. My misgivings were revived in 1939 by a further study

¹ Albright, *AASOR*, XVII, 86; FitzGerald, *PEFQS*, 1940, p. 81; Rowe, *Four Canaanite Temples*, I, ix.

² *PEFQS*, April, 1935, Pl. I and p. 68; *AAA*, Vol. XXI, Pls. XXX-XXXIX.

³ Engberg and Shipton, *SAOC*, No. 10, in Strata VIII and IX.

of the Jericho materials with a view to a final publication of our results, hitherto confined to progress-reports in the University of Liverpool's *Annals of Archaeology and Anthropology*. With the specimens in question thus clearly in mind, though without access to my notes and records at the time of writing, I have no hesitation in accepting the proposed revision in principle, and like other students I shall be glad to see an agreed reclassification made in due time by the excavators themselves together with the publication of their very interesting fifteenth-century pottery, which is as yet known only to a few privileged friends.

The later painted fabrics were found by us at Jericho in considerable quantity upon the small mound overlooking the spring during the excavation of the Middle Building, a structure so named because it stood in apparent isolation, stratified between the so-called Hilani above and the older palace storerooms below.⁴ In previous excavations Dr. Watzinger had also found specimens on the same spot. His last-minute excavation of the Hilani had penetrated to its foundations, which were exceptionally deep in proportion to its massive character, and so disclosed some underlying walls of the Middle Building, as shown in his published plan,⁵ and with them some of its pottery. His personal records, which he kindly communicated to me in reply to my inquiries, left no doubt that he had reached in places and probed the upper levels also of the palace storerooms. Thus is explained how fragments of the older painted fabrics came to be found in the debris of the Middle Building and how a number of these fitted together, though found by us in different rooms or areas.⁶ Notwithstanding the evident disturbance of the strata, the original stratification of the pottery under discussion is clarified by the present recognition of its later and earlier elements. The later series clearly belongs to the Middle Building in which it was found exclusively; and this was confirmed by my recent re-examination of the materials which include a number of tell-tale pieces. The earlier series must perforce be assigned to the underlying stratum, and this also seems to be confirmed by the discovery of two whole vessels of this class in the topmost levels of the palace storerooms—in spots, it should be noted, that lay outside the

⁴ *The Story of Jericho*, Figs. 3 and 4.

⁵ *Jericho*, Table IV.

⁶ *AAA*, Vol. XXI, Pl. XXXIX, and p. 103.

area of the Middle Building and were accordingly freer from disturbance.⁷ These vessels are similar to those from Tomb 5, where they seem to date their incidence to the age of Thuthmes III. Another type of painted wares—the last of our LB I series—is represented by a number of long-necked jugs found in Tomb 4; they appear to imitate in a way the larger type of Cypriote jugs of *bil-bil* fabric; and their introduction as a type into the repertory of Bronze Age Canaan seems to be dated both at Jericho and at Lachish⁸ by contemporary scarabs of Amenhetep III.

A few specimens of the later class of painted wares that we have been discussing were also found, by both myself and Dr. Watzinger, at the north end of the site, outside the limits of the Fourth City. Ours came from a group of rooms against the Hyksos defensive parapet, and it was with interest I noticed that I had in fact dated them at the time to LB II.⁹ So far as I am aware, none of this distinctive pottery was found by either expedition elsewhere on this site. Certainly, none was found in association with the main walls of City IV, and, as already stated, none was found in any of the tombs. The evidence for dating the occupation and destruction of the Fourth City remains thus unchanged by this revision: the Middle Building is simply dissociated from the period when that city was in being.

Though as a result of this revision the Middle Building cannot have been the residence of the last kings of Jericho (as the indications and criteria led us to suppose at the time of its discovery)¹⁰ a reinterpretation of the evidence in the light now available discloses a possibility of high interest. Our excavations, logically interpreted, point to the fall of the city in the reign of Amenhetep III (ca. 1400 B.C.), possibly late in his reign (which is well represented), but before that of his successor Akhenaton, of whose period there is no trace—no royal signet, no influx of Early Mycenaean pottery, and no mention of Jericho in the

⁷ Cf. report for 1933 in *ibid.*, Pl. XXIV, No. 6, found with a Cypriote milk bowl in the top layer of house-room No. 60; and Pl. XXVII, 15, from the uppermost layer of spot 17a, in square I.6; also cf. Pl. XX, No. 1, from Room 39.

⁸ Jericho: *AAA*, Vol. XX, Pl. XI, and p. 26; Lachish: *The Fosse Temple*, Pl. LI.B.ii.

⁹ *PEFQS*, 1930, pp. 130 ff., and *Joshua and Judges*, pp. 146–47.

¹⁰ The caption to the photograph of the Middle Building in *The Story of Jericho*, Pl. XVIa, needs to be corrected in the light of this revision. So also does the opening paragraph on p. 118; the fragments of painted pottery like that from Tomb 5 belong in all probability, as we have seen, to the underlying stratum (cf. *AAA*, Vol. XX, Pl. XXV).

Amarna letters. But we have recognized traces of a partial and intermittent occupation of the site, with a few intrusive burials in the tombs,¹¹ during the five hundred years that the city itself lay in ruins. To these traces must now be added the Middle Building and its contents.

Looking at the plan of this structure, we find certain curious features: though clearly a residence (for it had both hearth and oven), one long room in the main block was like a stable; and it was also provided with its own stout inclosing wall which was laid out noticeably askew from the old lines of the city. What can this strange intrusion signify? To what alien occupier can it be attributed, who secured for his dwelling the most favored position on the site, but who apparently made no use of the tombs? The Bible itself provides the answer. In *Judg.* 3:12–14 we read:

. . . . The Lord strengthened Eglon the King of Moab against Israel. . . . And he gathered unto him the children of Ammon and of Amalek. And he went and smote Israel, and they possessed the City of Palm Trees. And the children of Israel served Eglon the King of Moab eighteen years.

The approximate date of this episode is not difficult to compute if we accept as basis the date of the fall of Jericho. This, according to biblical tradition, would fall about 1400 B.C. or just before, a result in very close agreement with that of our excavations which, we have seen, point to 1400 B.C. or just later. Both sources indicate indeed the same reign (that of Amenhetep III); and the central figure of 1400 is the nearest approximation to the date that evidence can support. With this as basis, the date of the oppression by Eglon, according to data given in the Bible (which include the period of Joshua and the Elders, the eight years of oppression by Cushan, followed by forty years of Rest),¹² would fall wholly or mostly within the reign of Seti I. But this is the very period to which archeologists now ascribe the incidence of

¹¹ Tombs 4 and 13. Wace has shown that the pseudo-Mycenaean vases from the latter cannot be earlier than 1350 B.C. (cf. *Annual of the British School in Athens, 1940* [Session = 1936–37], pp. 259 ff.).

¹² The biblical traditions as to dates are discussed in *Joshua and Judges*, pp. 60–62, and a table of approximate and relative dates based on Egyptian chronology is given in *ibid.*, p. 243. The general estimate of 480 years (I Kings 6:1) from the Exodus to the fourth year of Solomon is controlled by the figure of 300 years from Heshbon to the first year of Jephthah (*Judg.* 11:26) and harmonizes with it if in both cases we regard the Minor Judges as contemporary with and not independent of the Major Judges—a point upon which most students are independently agreed.

the pottery we have discussed above. That this pharaoh did repress disturbances in the Jordan Valley is known independently from an inscription discovered at Beisan; and that the disturbances extended farther south is recorded in the Egyptian archives of his reign: "The vanquished Shasu [Beduin] plan rebellion: their tribal chiefs are gathered together, rising against the Asiatics of Southern Palestine [Kharu] . . . and they disobey the laws of the Palace."

It is instructive to compare closely the text of this historical record with that of the biblical episode previously quoted. The parallelism is complete. Each record tells of a combine of border tribes from the southeast and beyond Jordan menacing the southern highlands. Moreover, they agree closely in date if we accept the basis of Israelite tradition, and they may well refer to one and the same episode, to which the ruins of the Middle Building on the site of old Jericho bear material witness. The scholars who have revised the dating of Late Bronze Age pottery have also restored a fragment of Bible narrative to its rightful place in history.

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DARIUS AND HIS EGYPTIAN CAMPAIGN

RICHARD A. PARKER

The verso of the so-called "Demotic Chronicle" contains, among others, the well-known statement of the codification of Egyptian law under Darius. Following the translation of Spiegelberg, later writers have consistently given *year 3* as the one in which Darius sent *to his satrap* in Egypt, ordering the compilation of the laws.¹ Actually, the year number is *4*, as was recognized by Spiegelberg himself in his glossary, although he failed to correct the translation.² It was in 518 B.C., therefore, that Darius sent the command to his satrap.³

According to the Behistun inscription (§ 21), Egypt revolted while Darius was fighting Nebuchadnezzar III, but no campaign against it is recounted.⁴ Herodotus tells us that Aryandes, the satrap whom Cambyses had appointed, was executed for assuming the royal prerogative of coining money, though Darius charged him only with rebellion.⁵ Whether it was the coinage of money, the ill-fated expedition against Barce, or other events unknown to us, the fact that Egypt was considered a rebel, and that Aryandes was put to death as one, suggests that Darius, a young, vigorous, and energetic king, took care of Egypt at his earliest opportunity. Furthermore, one may conjecture that the use of *his* satrap in the passage referred to above means a man appointed—or, at least, confirmed in office—by Darius. This could not have been true of Aryandes.⁶ Nor does it seem likely that

¹ W. Spiegelberg, *Die sogenannte Demotische Chronik* (Leipzig, 1914), p. 31; E. Meyer, *Ägyptische Dokumente aus der Perserzeit* ("Sitz. d. König. Pr. Akad. d. Wiss. phil.-hist. Kl.," 1915), p. 308; N. J. Reich, "The Codification of the Egyptian Laws by Darius and the Origin of the 'Demotic Chronicle,'" *Misraim*, I (1933), 180; G. Posener, *La première domination Perse en Egypte* (Le Caire, 1936), p. 175.

² *Op. cit.*, p. 144. The difference between the signs for 3 and 4 is clear, though the significance of the small oblique stroke over the sign in the text is puzzling.

³ His fourth year began December 31, 519, in Egypt.

⁴ I have discussed this paper with the editor, who has given me a number of suggestions and references to Persian material.

⁵ iv. 166. No coins definitely ascribable to Aryandes have yet been found. R. W. Rogers (*A History of Ancient Persia* [New York, 1929], p. 98) revives an old error in assigning to him coins long before recognized as belonging to the kings of Sidon (cf. E. Babelon, *Traité des monnaies grecques et romaines* [Paris, 1910], II, No. 2, 546).

⁶ We know that Cambyses appointed him (Herodotus, *loc. cit.*) and that Egypt was considered in revolt before three months had elapsed after the death of Bardiya and the accession of Darius (Beh. § 21, and Cameron, "Darius and Xerxes in Babylonia," *AJSL*, LVIII [1941], 314 ff.).

Darius would have concerned himself with the codification of the laws of his empire, before that empire was completed by the reacquisition of Egypt, probably its most valuable province.⁷ It is possible to conclude, therefore, that this message was sent after Darius had taken Egypt, killed Aryandes, placed a new satrap in power, perhaps Pharandates, and returned to Asia.

With the latter part of 518 as an *ante quem* for Darius' Egyptian campaign, what can be said about a *post quem*? As the pacification of Egypt is not mentioned in the Behistun inscription, it is logical to assume that it took place after the last event recorded in that inscription. Furthermore, the march must have been through Palestine, and, as this was the period of the rebuilding of the temple in Jerusalem, we should expect to find some echo of the Persian passage in the Bible.

Behistun § 71 begins with the statement of Darius: "This is what I did . . . after I became king." The most plausible reconstruction of the broken passage is that of Tolman,⁸ who suggests: "during both the second year and the third year."⁹ Two events follow, and it seems reasonable to conclude that the revolt of the Elamite Atameta¹⁰ took place in the second year and Darius' expedition against the Saka¹¹ in

⁷ The "restoration of order" inscription of Darius, Susa *e* = No. 15 (cf. Kent, *JAOS*, LIV [1934], 40 ff.; *ibid.*, LVIII [1938], 112 ff.; Weissbach, *ZDMG*, XCI [1937], 80 ff.) certainly indicates law codification throughout all the empire. Its date remains uncertain. Judging from the provinces named, it must come from before the European Saka venture but after the Persians had learned of the two types of eastern Saka (cf. n. 11 below). It does not seem possible to tie Susa *e* in with the Egyptian order of year 4. The latter must represent only the initial step, whereas the former more probably has to do with the final promulgation of the codified laws.

⁸ *Cuneiform Supplement* (New York, 1910), pp. vi, 39. Cf. also Kent, *JAOS*, LVIII (1938), 675 f. Poebel, *AJSL*, LV (1938), 293 ff., places the Elamite revolt and the expedition against the Saka in the same year, the second. This seems possible, chronologically. Hinz, *ZDMG*, XCIII (1939), 370 ff., restores "in that fifth year," but to this there are chronological objections. By the fifth year, Egypt had been pacified, and failure to include this accomplishment in the inscription would be difficult to explain. Cf. also n. 15 below.

⁹ Darius is not reckoning here in Babylonian regnal years but means the second and third year-periods after September 29, 522 B.C., when he killed Bardiya. This is certain from his claim in §§ 52, 57, 59, and 62 that the events in the main inscription took place in one year (cf. Poebel, *AJSL*, LV [1938], 298). Actually, he did not defeat Nebuchadnezzar IV until November 27, 521. The appendix, then, covers roughly the period from November 28, 521, to September 28, 519.

¹⁰ The restoration of F. W. König, *Relief und Inschrift des Königs Darius I am Felsen von Bagistan* (Leiden, 1938), p. 78.

¹¹ These must be the *Sakā tigrakhaudā*, the "Pointed-hat" Saka, as indicated by line 22 and the sculpture of Skunkha (see also Hinz, *op. cit.*, pp. 364 ff.). In Beh. §§ 6 and 21, the land of the Saka is grouped with eastern and northeastern countries, and in Darius, Pers. *e*, § 2, it is placed definitely in the east. The Naqsh-i-Rustam *a* inscription of Darius locates the *Sakā haumavargā* and the *Sakā tigrakhaudā* in the east, and the *Sakā tyaiy paradraya*,

the third. His aim would be to secure part of his northeastern frontier before he undertook the long march to Egypt, and the spring or summer of his third year would have been the logical season for a northern campaign. If he then left Mesopotamia before the rainy season began in December, the winter of that year could have seen him on the road to Egypt, whence he might return in the spring of the following year.

The biblical evidence offers some support to this outline. After construction of the temple began in the sixth month of Darius' second regnal year,¹² there follow five prophecies, two by Haggai and three by Zechariah. Four of these date to the last half of *year 2*, and, because of the threat to the rebuilding of the temple arising from the visit of Tattenai (Ushtannu) or possibly because of nationalistic aspirations, they are tinged with revolt. The last dated prophecy of Zechariah, in the ninth month of *year 4*, has no hint of revolt. In this interval the decree of Darius in favor of the Jews was issued and the march to Egypt took place, the combination of which would effectively quiet any Jewish inclination to revolt.

A small amount of other evidence, mainly Egyptian, remains to be considered. A demotic papyrus from the Golenischeff collection,¹³ a document of the temple of Horus at Edfu, records in line 15 a date for Darius of *year 3, second month of the first season* (February, 519). Preceding this date, in lines 2 and 4, there occurs *second month of the second season*, which presumably is *year 2* (June, 520). On the basis of the biblical evidence, Darius could not have been in Egypt so early in his third year, and we may suppose either that Aryandes had embarked on a policy of appeasement after he had seen the rapid rise of Darius'

"the Saka, those beyond the sea," in the west. It would appear, then, that when Darius came to the throne the only Saka of whom the Persians had knowledge were those living on the northern frontier, probably to the east of the Caspian Sea, and that it was only later, after his Indian campaign and the campaign against Scythia recounted by Herodotus, that it became necessary to distinguish by appellatives the various tribes of the Saka people. The canal stelae of Darius in Egypt, written after the Indian but before the Scythian campaign, know only eastern Saka, who are divided, according to the Egyptian, into the "Saka of the marshlands" and the "Saka of the plains." These Posener identifies with the *Sakā tigrakhaudā* and the *Sakā haumavargā*, respectively (*op. cit.*, pp. 184 f.). To account for the fact that Pers. *e* includes India in the empire, but fails to distinguish between the two eastern Saka, it may, however, be necessary to posit a northeastern campaign after the Indian one and before the erection of the canal stelae. On such a campaign Darius could have conquered the *Sakā haumavargā*.

¹² Hag. 1:15.

¹³ A hand copy of part of this papyrus was published by Revillout in *Rev. Eg.*, Vol. III, No. 2 (1883), Pls. 1-2. Griffith, working from photographs, assigned the document to the reign of Darius I (cf. *Ryl.* III, 25).

power or that the papyrus was written sometime later in the reign and dated retroactively.

A passage in Polyaeus¹⁴ tells how Darius arrived in Egypt during the mourning for the death of an Apis bull and offered one hundred talents for the discovery of another. Wiedemann has associated this passage with the Apis which died in the fourth year of Darius (518).¹⁵ But Apis died on the 4th of Pachons and was buried on the 13th of Epiphi, and this time of year, from August 31 to November 8, would be a most improbable time for campaigning against Egypt, with the inundation at its height in September. Cambyses, we recall, carried out his campaign in the late winter and early spring, and Darius was with him as a member of his bodyguard.¹⁶ The germ of truth in Polyaeus' story may be merely that Apis died in the same year that Darius arrived in Egypt (though that arrival was months earlier, in the winter) and that the new satrap offered a reward for a successor, in the name of Darius and in accordance with his conciliatory policy.

This stela of year 4 which records the death of Apis bears no Horus name on the banner behind the king, and Darius is entitled simply "King of Upper and Lower Egypt." This has led Posener to suggest that the stela may have been written before Darius' arrival in Egypt, at which time he presumably would have had a titulary composed for himself.¹⁷ Such a conclusion, however, is invalidated by the fact that on none of Darius' later monuments, with the exception of his temple in the Kharga oasis, is a full titulary, or even a prenomens and nomen, written.¹⁸

To summarize, then, there is no important evidence to militate against the theory that Darius left Persia in the late summer of 519 B. C.

¹⁴ vii. 11. 7.

¹⁵ *Geschichte Aegyptens* (Leipzig, 1880), p. 236. Through a blunder he gave the date as toward the end of 517, and unfortunately this error has been perpetuated by later writers, e.g., R. W. Rogers, *A History of Ancient Persia* (New York, 1929), p. 99; Swoboda, in Pauly-Wissowa, *Real-Encyclopädie*, VIII, 2189. Sarre-Herzfeld, in *Iranische Felsreliefs* (Berlin, 1910), p. 106, on the basis of the 517 date, argue that the Indian campaign took place in 518 and that work on the Persepolis terrace began 518/17. Hinz (*op. cit.*, p. 372) refers to Wiedemann in justifying his restoration of the date in Beh. § 71 (cf. n. 8 above), stating that the monument was completed before the return from Egypt. But Wiedemann's date should be 518, and Darius' fifth year from his accession to the kingship would be from September 29, 518, to September 28, 517. His fifth year according to Babylonian regnal year reckoning would be still later, 517/16. Posener (*op. cit.*, p. 181) seems not to have noticed Wiedemann's error.

¹⁶ Herod. iii. 139.

¹⁷ *Op. cit.*, p. 176.

¹⁸ Gauthier, *Le Livre des rois d'Égypte*, IV, 140 ff.; cf. also the canal stelae in Posener, *op. cit.*

on campaign against Egypt. Having reached there early in 518, and having disposed of Aryandes, he installed a new satrap and gave orders for the construction of the canal from the Nile to the Red Sea. After this he returned to Asia (in the spring[?] of 518), whence, before the end of 518, he ordered his satrap in Egypt to have the laws collected.

CHRONOLOGICAL TABLE

Event	Date B. C.	Regnal Year	Month	Day	Source
Death of Bardiya	522, Sept. 29	Acc.	VII	10	Beh. § 13
Revolt of Egypt					Beh. § 21
Last battle of Nebuchadnezzar III with Darius	Dec. 18		X	2	Beh. § 19
Defeat of Nebuchadnezzar IV	521, Nov. 27	(I)	VIII	22	Beh. § 50
Behistun inscription composed					
Ushtannu (Tattenai), first date	520, Mar. 21	I	XII	18	Strass. Dar., No. 27
Revolt of Atameta		(II)			Beh. § 71
Haggai, first prophecy	Aug. 29	II	VI	1	Hag. 1:1
Work begun on temple	Sept. 21	II	VI	24	Hag. 1:15
Visit of Tattenai to Jews, letter to Darius		II			I Esd. 6:3; Ezra 5:3
Haggai, second prophecy	Oct. 17	II	VII	21	Hag. 2:1
Zechariah, first prophecy	Nov.	II	VIII		Zech. 1:1
Haggai, third prophecy	Dec. 18	II	IX	24	Hag. 2:10
Temple foundation laid	Dec. 18	II	IX	24	Hag. 2:18
Zechariah, second prophecy	519, Feb. 15	II	XI	24	Zech. 1:7
Decree of Darius in favor of the Jews					I Esd. 6:27; Ezra 6:1
Expedition of Darius against Skunkha (<i>Sakā tigrakhaudā</i>)	(Summer)	(III)			Beh. § 74
Appendix added to Behistun inscription, by		(III)	(VII)		
Egyptian campaign of Darius	519/18	(III)			
	(Winter)				
Darius leaves Egypt	518	(IV)			
	(Spring)				
Death of Apis	Aug. 31	IV	IX	4	Louvre St. 357
Burial of Apis	Nov. 8	IV	XI	13	Louvre St. 357
Order for compilation of Egyptian laws		IV			Dem. Chronicle, v. c. 9-11

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HURRIAN CONSONANTAL PATTERN¹

PIERRE M. PURVES

I

In an article published in this *Journal*² the writer set forth his discovery that the pattern of voiced and voiceless stops noted by J. Fried-

¹ The problems treated in the beginning of this article were discussed by the writer in a paper entitled "Medial Pause in Hurrian," read April 15, 1941, at the joint meeting of the American Oriental Society and its Middle West Branch in Chicago.

In addition to the abbreviations in *AJSL*, LVII (1940), 162, n. 1, the following are used:

- CBS University Museum, University of Pennsylvania. Unpublished Nippur tablets collated by A. T. Clay and cited by him in *PNCP*.
- Ch.B. Chagar Bazar; applies also to names from that locality compiled by C. J. Gadd in *Iraq*, VII (1940), 35-42.
- HSS "Harvard Semitic Series."
- Mari Hurrian tablets published by F. Thureau-Dangin in *RA*, XXXVI (1939), 1-28.
- NDA Moshe Berkooz, *The Nuzi Dialect of Akkadian: Orthography and Phonology* ("Language Dissertations," No. 23 [Philadelphia: Linguistic Society of America, 1937]).
- PNCP A. T. Clay, *Personal Names from Cuneiform Inscriptions of the Cassite Period* ("Yale Oriental Series," *Researches*, Vol. I [New Haven, Conn., 1912]).
- RS Tablets found at Ugarit (Ras Shamra). RS 1-48 published by Charles Vroilleaud in *Syria*, Vol. X (1929), Pls. LXI-LXXV, after p. 308; RS 49, *Syria*, XV (1934), 153; RS 50, *Syria*, XII (1931), 389; RS 372, *Syria*, XX (1939), 125-29.
- Tuš. The Tušratta letter, written in Hurrian by Tušratta, king of Mitanni, to Amenhotep III of Egypt. All citations in this article are taken from J. Friedrich's transliteration in his *Kleinasiatische Sprachdenkmäler* (Berlin, 1932), pp. 7-32.
- Ug. Voc. The Sumerian-Hurrian vocabulary discovered at Ugarit (Ras Shamra) in 1930. Copied, transliterated, and discussed by Thureau-Dangin in *Syria*, XII (1931), 234-66; transliterated again by Friedrich, *Kleinasiatische Sprachdenkmäler*, pp. 149-55.

Rev. Dr. E. R. Lacheman, of Torrington, Conn., has kindly collated the immediately available SMN material which is cited in this article with the permission of Professor Robert Henry Pfeiffer, curator of the Semitic Museum at Harvard University.

The writer acknowledges that his researches would have been impossible without the names compiled by Clay, *PNCP*, Ebeling in *MAOG*, Vol. XIII, Heft 1 (1939), and in Gadd's Ch.B. list. In many instances the writer departs from the readings in these lists, as in the case of the Ch.B. names in n. 11. The same applies to the lists compiled by Clay and Ebeling. In citations of divine and personal names, initial letters are capitalized in order to distinguish them from citations of ordinary lexical material. In readings of Hurrian material from Hurrian sources, all stops are considered as voiceless. When this same material is provided by Semitic sources, stops occurring singly after vowels and next to sonants are considered as voiced. Thus, for example, the name of the chief Hurrian deity is cited as *Teššup* when found in Tuš. and other texts of Hurrian origin. When this same deity is mentioned in texts of Semitic origin, such as those from Dilbat, Nippur, Mari, and Ugarit, the name is read *Teššub*. For ordinary purposes the former reading is preferred.

² Entitled "The Early Scribes of Nuzi," *AJSL*, LVII (1940), 162-87, hereafter referred to as "Early Scribes."

rich and C.-G. von Brandenstein³ in Hurrian texts written in the alphabetic cuneiform script of Ugarit reflected the hearing of Hurrian by Semitic scribes.⁴ The basis of this discovery was the observation that the very same pattern occurred whenever Hurrian names were written by scribes whose native language was Akkadian in its several forms.⁵ The evidence, it is recalled, was drawn from Hurrian names on tablets found at Dilbat, Nippur, and Aššur, localities where the presence of Hurrian scribes is hardly conceivable,⁶ and from names occurring on the few Nuzi tablets which were written in good Akkadian by scribes with Akkadian names.⁷

The pattern concerned is generally understood as one in which stops were voiceless when initial and when doubled but voiced medially when occurring undoubled after vowels or adjacent to the sonants *l, m, n, or r*.⁸ These rules are not without their exceptions.⁹ Since this stop pattern apparently resulted from Semitic hearing of Hurrian, it was concluded that, phonemically, Hurrian had only one set of stops, the actual phonetic difference in voicing being unrecognized by speakers of Hurrian but readily perceived by Semitic listeners to whom difference in voicing was phonemic.¹⁰

In continuing his study of the subject, the writer must first deal with a notion of his own, shown to be erroneous by evidence yielded by additional Hurrian names written by Semitic scribes on the tablets found at Chagar Bazar which have been recently compiled by C. J. Gadd.¹¹ The stop pattern is so obvious in this material that a critical

³ See Friedrich, *An. Or.*, XII (1935), 130 f., and C.-G. von Brandenstein, *ZDMG*, XCI (1937), 574.

⁴ See "Early Scribes," pp. 183 f.

⁵ *Ibid.*, pp. 172-77.

⁶ However, a Middle Assyrian scribe *Ta-gu-uḫ-li mār Ew(ri)-ri-šu-ḫur-ni* (*KAJ* 167:24) is the Hurrian son of a Hurrian father. Nevertheless, he certainly would not have been able to follow his calling at Aššur unless he had been thoroughly Assyrianized.

⁷ "Early Scribes," pp. 180-83.

⁸ See Friedrich, *An. Or.*, XII, 131; and von Brandenstein, *ZDMG*, XCI, 574; and "Early Scribes," pp. 173 f., 180. Evidence for the voicing of stops after *m* and *n* in the alphabetic texts is rare, presumably because of the tendency of *m* and *n* to disappear in writing before stops (see "Early Scribes," p. 184).

⁹ See von Brandenstein, *ZDMG*, XCI, 574, and "Early Scribes," p. 174, n. 58, and p. 182.

¹⁰ See "Early Scribes," pp. 184 f. At present the writer's opinions diverge from the view expounded by Speiser in *Language*, XVI (1940), 319-40, who maintains that difference in voice existed phonemically in Hurrian.

¹¹ *Iraq*, VII (1940), 35-42. Other Ch.B. names have been published by Gadd, *Iraq*, IV (1937), 178-85. Since hardly any of the items in this publication are involved in this ar-

examination of qualifying examples is superfluous and, therefore, provides ample testimony of the handiwork of Akkadian scribes. Included are a number of names of a certain type which would have prevented the writer from going astray in the previous study. They contain the elements *-teššub*, *-tilla*, *-tirwi*, *-lašenni* in a phonetic context which apparently called for voiced initial *d* rather than a voiceless *t*. An attempt to resolve the dilemma assumed that *d* was actually pronounced.¹² Since these elements represented Hurrian deities well known even to Semites, the Akkadian scribes were erroneously believed to have expressed their etymological knowledge in writing. Thus, for example, the Akkadian scribe was understood to have heard *-deššub*; but, supposedly aware that the element involved the deity Teššup, the scribe was assumed to have exhibited his etymological knowledge by writing *-te-šub* and the like.

However, Ch.B. names contain final elements written *-ka-na-ze/zi*, *-ke-eš-še*, *-ki-ia-ze*, *-ta-na*, and *-tu-up-ke* in context apparently calling for *-ga-na-ze/zi*, *-ge-eš-še*, *-da-na*, and *-du-up-ke*, respectively.¹³ These elements are Hurrian words with whose etymology no Semitic scribe can imaginably have been familiar.¹⁴ Overlooked also was the element

ticle, the citations to Ch.B. will not refer to it unless specifically stated. The writer departs from Gadd's readings. What Gadd reads as *u* the writer understands as *ú*. At times where Gadd reads *b* the writer reads *p*. Many signs which Gadd reads as *hi*, *ki*, *ni*, *zi*, according to custom, the writer at times reads as *he*, *ke*, *né*, *ze*. Where Gadd reads *u* the writer reads *úš*. With this understanding the reader should have no difficulty in checking the material which the writer has drawn from Gadd's invaluable contribution.

¹² "Early Scribes," pp. 177, 181 f. The writer retracts his suggestion of the value *dil* for Nippur *BE/TIL*, *ibid.*, p. 180. In "Early Scribes," p. 179, the citation of *Še-na-til-la* (VAS, VII, 147:9) was quite unwise, for it is really *Še-na-be-la-šu*, an Akkadian name. The writer followed Ungnad's former misreading in *BA*, VI, Heft 5 (1909), 13 f. For the correct reading see J. Kohler and A. Ungnad, *Hammurabi's Gesetz*, V (Leipzig, 1911), (No. 1397).

¹³ As in Ch.B. *Na-wa-ar-ka-na-zi*, *Ša-du-um-ke-eš-še*, *Ša-zu-um-ke-eš-še*, *Me-me-en-ki-ia-ze*, *Pu-zu-um-ki-ia-ze*, *Aš-tu-a-ta-na*, *Ša-aš-tu-a-ta-na* (questionable, as Gadd notes), *Tu-up-ki-ta-na*, *Na-wa-ar-tu-up-ke*, where the initial consonants of the final elements, appearing as they do after vowels and sonants, are actually voiceless instead of being voiced as expected.

¹⁴ Hurrian *kešše* occurs as *ke(α1)-eš-še* (*KUB*, XXVII, 1 ii 30, 31, 70; 6 i 31), *ki-iš-še* (*KUB*, XXVII 4:7; 8 rev. 7), and in the obscure [G]IŠ.SÚ.A-*hi* (*KUB*, XXV, 44 ii? 5), which suggests the meaning "throne." See von Brandenstein, *ZDMG*, XCI, 569. The writer does not know of the instance alluded to without reference by von Brandenstein in which *kešše* begins with *h*. Hurrian *kiaše* occurs as *ke(α1)-e-a-ši* (*KUB*, XXVII, 42 rev. 22), *ki-i-ša-ši* (*VBoT*, 59 ii 4, 10, iii 9), and *ke(α1)-e-a-še-ne* (*KUB*, XXVII, 42 obv. 7, 8). It occurs as *ke(α1)-ia-še-ne-wee-na-šal-še* in *KUB*, XXVII, 38 i 4. Since it bears only once the divine determinative, *kiaše* must be a divine epithet rather than a deity, as L. Oppenheim proposes in *AOF*, XII (1937-39), 36 f. For *kiaše* as a final element in feminine Nuzi names see Oppenheim, *loc. cit.* The element written *ta-na* recalls the Nuzi name *Ta-na-e*

-tatta in Nippur *Erme-tatta*,¹⁵ comparable to Nuzi **Erwi-tatta*, which should have given warning. These examples indicate that the unexpected voicelessness in initial stops in final elements of Hurrian personal names was a Hurrian speech habit faithfully recorded by Akkadian scribes.

A ready explanation for this phenomenon is to be found in the well-known Dilbat name *Te-eš-šu-ub-a-ri* (VAS, VII, No. 72:10),¹⁶ which, when divided into elements, is *Teššub-ari*. The written occurrence of the name shows that there was a pause between the elements so perceptible that the Dilbat scribe took it for an actual aleph. The procedure was very much the same at Ch.B., where aleph is expressed in the writings *Am-ma-an-e-še*, *An-da-ar-e-še*, *A-ra-an-zi-iš-a-ri*, *A-rum-a-ri*, *A-ga-ab-e-li* (<f> *A-ta?-ab?-e-li* in Ch.B. list), *A-we-en-ú-bi*, *E-de-en-e-li*, *Ha-zi-ib-iš-ša-ra*, *Ki-lum-al-la-i*, *Ki-ri-ib-e-li*, *Ki-ri-ib-ul-me* (*D(K?)i-ri-ib-ul-me-ak* in Ch.B. list), *Ku-zu-ub-a-ri*, *Mu-zu-um-a-ri*, *Ši-in-ap-ze/sé*, *Te-eš-še-en-a-ri*, *Ú-na-ab-a-ri*, *Ú-nu-úš-ú-mar*. These names represent the respective Akkadian versions of Hurrian *Amman-ešše*, *Antar-ešše*, *Arans/iš-ari*, *Arum-ari*, *Akap-elli*, *Awen-umpi*,¹⁷ *Eten-elli*, *Hašip-iššara*, *Kelum-allai*, *Kirip-elli*, *Kirip-ulme*, *Kušu-ari*, *Mušum-ari*, *Šin-apse*, *Teššen-ari*, *Unap-ari*, *Unuš-umar*. The aleph represented in these writings—in which signs ending in a consonant are immediately followed by signs beginning with a vowel—occurs between the elements. It shows that a distinct pause in pronunciation took place at that point. The pause occurs between the elements as if they were words or free forms. As a

(*HSS*, V, 104:15; *AASOR*, XVI, 48:22, 41). Apparently it is to be associated with Hurrian *tan*, "do." The word *tupke* is discernible in *tú-up-ká-e* (*KUB*, XII, 44 ii 22), *tup-ki-ia-a-wee* (*KBo*, V, 2 ii 23); *tú-up-ki-ni* [...] (*KUB*, XII, 51 ii? 9); *tú-up-ku-un-na-a-ša* (*KBo*, II, 21:7); perhaps in *ki-ir-tú-up-ki-na-a-ša* (*ibid.*, 1, 6). The writer possesses no information on Ch.B. *ka-na-zi* but suggests that underlying Hurrian **kanaše* is involved.

¹⁵ Written *Er-me-ta-at-ta* (*BE*, XIV, 56a:7; *BE*, XV, 37:51; 53:7; 158:2), *Er-me-ta-ta* (*BE*, XV, 76:5; 187:2), *Er-mi-ta-at-ta* (*BE*, XV, 90:42). References in Clay, *PNCP*, p. 93, where it is taken as *Irmetatta*. Evidently the dialect of the Hurrians at Nippur shared with Nuzi Hurrian the form *erwi* which is the well-known variant of Hurrian *ewri*, "king."

¹⁶ Discussed by Ungnad in *BA*, VI, Heft 5 (1909), 8 f., and in *Subartu* (Berlin, 1936), p. 140. This name brings up the annoying problem of reading *atal* as opposed to *ari* (see L. Oppenheim in *RHA*, IV, Fasc. 26 [1937], 66).

¹⁷ In the Ch.B. list, p. 36, n. 11, Gadd associates *-ú-bi* with Opis. The writer, in line with his association of *ibnkl* (RS 4:47, 48) with the goddess Umpi-nikkal in "Early Scribes," p. 184, sees in *-ú-bi* a derivation from Hurrian *umpi*. In the controversy between A. Goetze and H. L. Ginsberg (see *Orientalia* [N.S.], IX [1940], 223-28, 228 f.) the writer finds himself an ally of the latter.

matter of fact, that is precisely what they were. Accordingly, any stop occurring as the initial sound of a final element had to be voiceless according to the pattern referred to above, for it was the initial sound of an actual word and was treated as such in speech.

This procedure applies also to the Nuzi names composed with *-teššub* and *-tilla*, mentioned previously.¹⁸ Most important, the Nuzi name Ewara-tupi, written *E-ma-ra-tu-bi* by an Akkadian scribe there, turns out to be Hurrian.¹⁹ The pause between the elements kept the phonetic form *-tubi* from becoming *-dubi* as would have been normally expected otherwise. The writer, unaware of this development, tried to deal with it by a fantastic explanation which must be retracted.

When the initial element ended in a consonant, the pause in question must have been subject to adverse phonetic influences, for it was less frequent later on, during the Kassite period. Thus, for example, the Hurrian names Matip-apu and Tatip-apu occur in a Nippur text as *Ma-di-ba-bu* (PBS, II, Part 2, No. 84:8) and *Ta-di-ba-bu* (*ibid.*, l. 7), writings which openly ignore the pause between elements. In Ch.B. of the Old Babylonian period they would have been written *Ma-di-ib-a-bu* and *Ta-di-ib-a-bu*. Exceptional at Nippur are writings observing the medial pause as in the case of *Ak-kul-en-ni* (PBS, II, Part 2, No. 84:26) for Hurrian Akkul-enni. The same situation is quite evident in Nuzi, where Hurrian Tehip-apu, for example, exists under conditions in which the writing *Te-ḫi-pa-pu* vies with *Te-ḫi-ip-a-pu* and finally prevails.²⁰

¹⁸ See again "Early Scribes," pp. 177-80, 181 f. The writer still adheres to his analysis of *-de(ja) < -te(ja) < -teššub* in "Early Scribes," p. 179.

¹⁹ Written *E-wa-ra-tù-pi* (JEN 566:24; JENu 412), *I-wa-ra-tù-ù-pi* (HSS, V, 63:2). The writing *E-ma-ra-tu-bi* (SMN 3094) by the Akkadian royal scribe Apil-sin has already been alluded to in "Early Scribes," p. 183. Hurrian origin for *ewara* seems probable in view of *e-wa-a-ra-ti* (KUB, XXVII, 29 iv 1). Dr. Robert S. Hardy has called the writer's attention to *E-wa-a-ri-ša-tu-ni* (2 Botu 12 A i 26; 12 B i 2). These examples establish the vocalization *e-wa-ra-* or *e-wa-ri-* wherever encountered. This applies to the famous name *E-wa(pi)-ri-šar-ri*, also ideographically expressed EN.LUGAL, from Qatna (see Virolleaud, *Antiquity*, III [1929], 315, and *Syria*, XI [1930], 313, l. 44), which has been generally misunderstood as *E-w(i)-ri-šar-ri* and confused with *Ewri-šarri*.

Nuzi Ewara-tupi is certainly Hurrian. In view of Ug. Voc. ii 23, *tupi* must mean "mighty," and on the basis of the equation *ewari* = EN in the Qatna version, Ewara-tupi must mean "(My) lord is mighty."

²⁰ The first type in which medial pause is not indicated is represented in *Te-ḫi-pa-pu* (e.g., JEN 310:34, 39), of which there are at least twenty-one instances; in *Te-ḫi-pá-pu* (HSS, V, 72:55); and in *De-ḫi-pá-pu* (HSS, V, 64:14). All in all there are, then, twenty-three instances of spellings ignoring the pause as opposed to nine instances where it is indicated, as in *Te-ḫi-ip-a-pu* (see, e.g., JEN 123:21, 26). This ratio seems to hold throughout in Nuzi names where the formative *p* before vowels is concerned.

When the pause disappeared between any two consonants comprising the final consonant of the first element and the initial consonant of the last element, retrogressive assimilation tended to take place between the consonants in question. This was the fate of the formative *p*, both at Nippur and Nuzi, which was frequently subject to total assimilation to following *ḫ*, *k*, *l*, *m*, *š*, and *t*. Before *n* the assimilation is often only partial, for the formative *p* becomes *m* in most cases.²¹

Also subject to assimilation under the same influences is the little-known formative *n*. While *p* made itself noticeable at Nuzi by its occasional resistance to assimilation by a following consonant, *n* lacked such integrity. As a result of the instability of the pause between elements during the Nuzi period, total assimilation of the formative *n* to certain following sonants and spirants was quite consistent. For this reason it has escaped the notice of most investigators. At Nuzi the formative *n* is noticeable chiefly between vowels, as in the names 'Ammin-ešše, Anin-api, Apen-ARI, Arpin-ARI, Erḫan-ARI, Erwen-ARI, Ithjin-ARI, Kulpen-ARI, Namḫen-ARI, Parḫen-ARI, Šelwen-ARI, Tirwen-ARI, 'Tirwen-elli, Umpin-api, and Wantin-ukur.²² Even so, the formative *n* is not readily apparent, because the spellings show that the pause between elements was well on its way out. Only once, to the writer's knowledge, is there to be found a writing which observes the pause, and that is *Nam-ḫé-en-a-ri*, in an unpublished text (SMN 2045). All other writings place *n* and the first vowel of the final element in the same sign, imparting to the casual observer the impression that final elements are ones in which the initial sound is *n*, and not the following vowel.

At Ch.B. *n* is quite evident as a suffix of initial elements in the writ-

²¹ See Berkooz, *NDA*, pp. 47 f., and "Early Scribes," p. 176, n. 66.

²² Observation of the formative *n* is to be credited to Oppenheim, *RHA*, IV, Fasc. 26 (1937), 61. The spellings in which it is involved are the following: *'Am-mi-né-éš-še* (SMN 3347:2), *A-ni-na-pi* (JEN 71:4), *A-pé-na-ri* (JEN 359:18), *A-pi-na-ri* (JENu 529a), *Ar-pi-na-ri* (JEN 240:19, 22), *Er-ḫa-na-ri* (JEN 278:15, 21, 23), *Er-ḫé-na-ri* (SMN 3328:1); *Er-we-na-ri* (SMN 3357:6); *It-ḫi-na-ri* (HSS, IX, 105:5, 6), *Ku-ul-pé-na-ri* (HSS, IX, 23:9), *Nam-ḫe-na-ri* (JEN 226:41, 43), *Na-am-ḫa-na-ri* (HSS, V, 77:8); *Na-am-ḫe-na-ri* (JEN 209:25, 36 [miscopied *Na-bi-ḫi-na-ri*]), *Nam-ḫé-en-a-ri* (SMN 2045); *Pár-ḫé-na-a-ri* (JEN 323:6), *[P]ár-ḫé-na-ri* (JEN 36:2), *Pár-ḫe-na-ri* (JEN 36:10, 12, 14, 17), *Pár-ḫa-na-ri* (G 72:12), *Pa-ar-ḫé-na-ri* (JEN 351:5, 9), *Pa-ar-ḫa-na-ri* (SMN 3082, 3094, 3101), *Še-el-we-na-ri* (JEN 60:30), *Še-il-we-na-ri* (JEN 251:2, 29), *Ti-ir-we-na-ri* (HSS, IX, 5:6), *'Ti-ir-we-né-el-li* (SMN 494:15 [in *JAOS*, LVII, 183]), *Um-pi-na-pi* (JEN 514:7), *Wa-an-ti-nu-kúr* (JEN 12:3), and *Wa-an-ti-nu-kúr*, (JEN 407:28).

ten forms of Hurrian 'Awen-u(m)pi, 'Eten-elli, 'Šin-aps/ze, Teššen-ari, the writings of which have already been cited to exemplify the pause between elements. In addition, Hurrian 'Memen-kiaše and 'Šinen-šalli are formally preserved in Ch.B. 'Me-me-en-ki-ia-ze and 'Ši(?)né(?)-en-ša-li. In Nuzi the existence of the formative *n* before following consonants is not so signally obvious. Before the stop *t*, it had a tendency to disappear utterly as far as writing is concerned. Thus Nuzi Umin-tanni is written variously as *Ú-mi-in-ta-an-ni* (JEN 397:6, 14; 546:5; JENu 877), *Ú-mi-en-ta-an-ni* (JEN 518:1), and *Ú-mi-ta-an-ni* (JEN 546:36). Before sonants and spirants its identity is concealed by its total assimilation to them.²³ Thus the form revealed as 'Šinen-šalli at Ch.B. becomes Šineš-šalli at Nuzi.²⁴ Initial *eten-*, as revealed by Ch.B. 'Eden-elli, becomes *etem-* in Nuzi 'Etem-menni²⁵ and *eteš-* in the very common Nuzi name *Eteš-šenni*.²⁶ It was suggested above that Ch.B. 'Amman-ešše probably resulted in the form 'Ammin-ešše at Nuzi. Under these circumstances Nuzi 'Ammiš-šalli²⁷ can safely be regarded as a development from *'Ammin-šalli if not ultimately from *'Amman-šalli.

In addition to its becoming weakened, there were conditions under which the pause between elements was completely eliminated. In each instance this loss occurred when the name was shortened. For instance, the final element *-teššup* assumed the hypocoristic form *-teja* and the shortened form *-te* as well.²⁸ At Nippur, *-teja* and *-te*, when following an initial element ending in a sonant or a vowel, were found to occur as *-deja* and *-de*.²⁹ In other words, the pause between elements was lost and the stop after a vowel became voiced in accordance with the stop pattern. This process may be observed in Ch.B. of the Old

²³ But remains unassimilated in Nuzi Šatin-šub and Tain-šub. The writer suspects that *n* in these examples is a derivation from some other sound, presumably *m*; cf. 'A-ri-im-tu-ri (HSS, V, 76:4, 6) and 'A-ri-in-tu-ri (HSS, V, 11:1), both linked by the same genealogy.

²⁴ Written 'Ši-ni-iš-ša-el-li (HSS, IX, 38:30), 'Ši-ni-ša-al-li (SMN 17), 'Ši-ni-iš-šal-li (SMN 50), 'Ši-ni-iš-šal-li (SMN 135), 'Ši-ni-ša-li (SMN 214), 'Ši-né-eš-šal-li (SMN 395); 'Ši-ni-iš-šal-li (SMN 642); 'Ši-ni-šal-li (SMN 654).

²⁵ Written 'E-te-em-me-en-ni (HSS, IX, 38:34).

²⁶ Mostly written *E-te-eš-še-en-ni* (e.g., JEN 6:18).

²⁷ Written 'Am-mi-iš-šal-li (SMN 394); 'Am-mi-šal-li (SMN 126).

²⁸ On which see Oppenheim, *WZKM*, XLIV, 203 f., n. 1, and writer, *JAOS*, LVIII (1938), 465 ff.

²⁹ "Early Scribes," p. 179.

Babylonian period, where Nuzi 'Twi-tilla³⁰ can be shortened to 'Twidil, as attested by the Ch.B. writing 'I-wi-di-il. But, as this writing shows, the shortening of *-tilla* to *-til* was similarly accompanied by voicing of the dental owing to the absence of the expected pause between the elements.

The final vowel of an initial element was frequently lost, presumably by syncope. When that vowel happened to be *a*, the pause between elements was lost along with it. Such shortening is attested at Nuzi where the element *enna-* frequently becomes *en-* or *enn-* as in the name Enna-šukru which has a variant En-šukru.³¹ By this token the Nuzi writing *Še-en-tá-ti* (SMN 3115) must express a similar shortening of the Nuzi name Šenna-tati.³² But the Akkadian version of this shortened form is *Še-en-da-da* (CBS 3480).³³ Here the loss of *a* is accompanied by absence of the pause between elements, a development resulting in the voicing of the dental stop after *n* according to pattern. In his last article the writer noticed but inadequately explained the Nippur writing *Ha-'bi-ir-di-il-la* (*PBS*, II, Part 2, No. 89:2) corresponding with the Nuzi name *Ḥapir-tilla*.³⁴ In view of the shortened form Šen-dada, Nippur *Ḥapir-dilla* must have been a shortening of original **Ḥapira-tilla*. This seems quite likely, in view of Nuzi *Ḥapira*.³⁵

II

The process just discussed has ramifications which lead the investigation into a much broader field. The Nippur writing *En-zu-ug-ri* (CBS 3480)³⁶ obviously corresponds to Nuzi En-šukru³⁷ which, as demonstrated above, is a modification of Enna-šukru. However, else-

³⁰ Written 'I-wi-ti-il-la in SMN 403, 3230. Shortening of names ending in *-lla* by the dropping of *-la* is demonstrable in SMN 2597, where the name 'Šarum-alla, written 'Ša-ru-ma-al-la, variously occurs as 'Šarum-al, written 'Ša-ru-ma-al.

³¹ Cf., e.g., *En-na-šuk-rù* and *En-na-šuk-ru* (JEN 424:2, 11, 14, 16) and *E-en-šuk-rù* (JEN 584:26, 43), all with the same genealogy. See also Berkooz, *NDA*, p. 29.

³² Written 'Še-en-na-ta-ti (*RA*, XXVIII, 7:15, on p. 38).

³³ Collated by the writer at the University Museum in Philadelphia.

³⁴ See "Early Scribes," pp. 172, 173. Cf. Nuzi *Ha-pi-ir-ti-il-la* (HSS, V, 55:38, 41). The writer retracts his unsuccessful attempt ("Early Scribes," p. 180) to explain the phonetic activity involved.

³⁵ Written, e.g., *Ha-pi-ra* (JEN 6:16).

³⁶ Collated by the writer at the University Museum in Philadelphia. Transliterated *En-(Bēl)-zu-ug(uk)-ri* in Clay, *PNCP*, p. 75.

³⁷ See "Early Scribes," p. 175, n. 60.

where at Nippur Hurrian *šukr* occurs with initial *š* and not initial *z*, for Nippur *Šu-gur-te-šub* (*PBS*, II, Part 2, No. 84:10) corresponds to the identically written Nuzi name *Šukur-teššup*, which in turn proves to be a variant of Nuzi *Šukri-teššup*.³⁸ Akkadian texts from Mari express this name as *Šu-ug-rù-te-šu-ub*.³⁹ Since it has been shown that initial stops of final elements become voiced when the initial element is thus shortened, it stands to reason that the sibilant which consistently appears in native Hurrian sources as *š*,⁴⁰ had a voiced aspect which, while not phonemic to Hurrians, struck the ear of the Babylonian scribe as a somewhat different sound approximating Akkadian *z* or perhaps *ṣ*. Similar to En-zugru of Nippur is Ch.B. *ʿAš-tu-za-ar*, which corresponds to Nuzi *ʿAštua-šar*.⁴¹ The shortening from original *ʿAštua-šar* in Ch.B. itself becomes quite evident in view of Ch.B. *ʿAš-tu-a-ta-na*.⁴² Thus the pattern of voiced and voiceless sounds, which so far in this article has been confined to stops, appears to embrace sibilants and, by the same token, spirants also.

Such a state of affairs undoubtedly may have been noticed by the reader in Part I of this article, where the Ch.B. elements *ħazib-*, *kuṣuḫ-*, and *muzum-* were compared with Hurrian *ħašip-*, *kuṣuḫ-* and *mušum-*. In addition, the Ch.B. element *-kiyaze* also alluded to cannot be disassociated from Hurrian *-kiaše*.⁴³ But Ch.B. names furnish also the elements *-muze*⁴⁴ and *-nirze*,⁴⁵ identical with Hurrian *-muše* and

³⁸ Cf. *Šu-kúr-te-šup* (SMN 3657) and *Šuk-ri-te-šup* (*AASOR*, XVI, 55:2, 4, 7, 8 ff.), with the same genealogy.

³⁹ See Dhorme, *RA*, XXXV (1938), 184, and *Comptes rendus* (1937), p. 3. The writer is indebted to Dr. Gelb for these references.

⁴⁰ That is, in texts which can be shown to be written by the Hurrians, such as the Nuzi documents, and above all the Tušratta letter. Probably all the Hurrian texts in Boğazköy were written by Hittites, a consideration making advisable some caution in dealing with these sources. However, on the possibility that the Hittite syllabary is truly representative of the Hurrian phonetic system see the last section of this article.

⁴¹ Written *ʿAš-tù-a-šar* (*TCL*, IX, 22:4). Possibly *ʿAš-tù-a-ka* (G 82:25) is a poor writing of this form.

⁴² To be divided *ʿAštua-tana*; cf. Ch.B. *Tupki-tana*, written *Tu-up-ki-ta-na*.

⁴³ Semitized form *kiyaze* occurs in Ch.B. *ʿAt-tap-ki-ia-ze*, *ʿMe-me-en-ki-ia-ze*, *ʿPu-zu-um-ki-ia-ze*, and *ʿU-nu-úš-ki-ia-ze*. See n. 14 for *kiaše* in Hurrian texts.

⁴⁴ In Ch.B. *ʿA-we-eš-mu-ze*, to be directly associated with Nuzi *ʿAweš-muše*, written *ʿA-wi-iš-mu-še* (JEN 501:19) and *ʿA-we-eš-mu-še* (SMN 352). On *muš* see Thureau-Dangin in *RA*, XXXVI (1939), 22 f.

⁴⁵ In Ch.B. *ʿAt-ta-i-ni-ir-ze*. Nuzi names with this element are *Erwen-nirše*, written, e.g., *Er-we-en-ni-ir-še* (JEN 29:7, 42); *Tai-nirše*, written *Ta-i-ni-ir-še* (JEN 487:25, 35); and *ʿSilip-nirše*, written *ʿSt-lip-ni-ir-še* (SMN 403, 599). Cf. perhaps *ni-i-ir-ša-e* (Tuš. iv 66).

-nirše, along with *puzum(-)*, *šazum-*, and *-uzuwe*, which are perhaps identical with Hurrian **pušum-*, **šašum-*, and *-*ušue*, respectively.⁴⁶ In addition, Hurrian *šunš* in the Nuzi name *ʿŠunšun-na-ia*, written *ʿŠu-un-šu-un-na-a-a* (SMN 3506), appears as *šunz* in the Ch.B. form *ʿŠu-un-zu-na-ia*.

But this treatment of the sound which the Hurrians represented by *š* is not limited to the Ch.B. scribes when it occurs after vowels and sonants. The Nippur name *Arik-kazu*, discussed in a previous article, now seems to represent underlying Hurrian **Arip-kašu*.⁴⁷ Furthermore, Hurrian names from Nippur exhibit evidence that the sibilant which the Hurrians wrote with *š* was subject to voicing when it occurred beside *n* or *r*, just as the stops were. Thus Nippur *Še-er-zi-ia* (CBS 3480:22)⁴⁸ is obviously identical with Hurrian *Šeršija*,⁴⁹ while Hurrian *-mušni*⁵⁰ is represented at Nippur in *Aram-muzni*.⁵¹ That it was attempted to represent this sound by *š* is shown by the Nippur writing *Ḫa-ši-ib-til-[a]* (*PBS*, II, Part 2, No. 84:22, 35), which is almost beyond doubt identical with *Ḫašip-tilla*.⁵²

⁴⁶ Ch.B. *ʿPu-zu-um* and *ʿPu-zu-um-ki-ia-ze* contain *puzum*, possibly reflected in Nuzi *Puš-teja*, written *Pu-uš-te-ia* (G 72:3); *ʿAla-puša*, written *ʿA-la-pu-ša* (JEN 174:3); *ʿHalpa-puša*, written, e.g., *ʿHal-pa-pu-ša* (*AASOR*, XVI, 42:3, 8 ff.); and *Puškka(?)*, written *Pu-ši-ik-ka-i(?)* (JEN 301:21). For *šazum* in Ch.B. *Ša-zu-um-šar(-ri)* cf. *šašu* in *Šašu-teššup*, written *Ša-šu-te-šup* (SMN 3241:6) and *šaš* in Nuzi *ʿŠaš-kiyāše*, written *ʿŠa-aš-ki-ia-še* (JEN 218:3, 5, 8, 11); *Šaš-naihe*, written *Ša-aš-na-i-hé* (JEN 218:1); *Šaš-tae*, written *Ša-aš-ta-e* (e.g., JEN 383:1, 5 ff.). Cf. also *ʿŠašua*, written *ʿŠa-šu-ú-ia* and *ʿŠa-šu-ia* (JEN 445:1, 5).

Ch.B. *ʿPa-aḫ-ri-ú-zu-we* and *ʿTa-da-ab-ú-zu-we* contain a final element unknown in Nuzi names to the writer, who tentatively takes recourse to *u-šu-ni* (*KUB*, XXVII, 21:3), *u-šu-un-ni* (*KUB*, XXVII, 4:4; 8 rev. 4), *u-šu-un-na-a-ši-na* (*KUB*, XXVII, 42 rev. 12), and *u-šu-un-ni-pi-na* (*KUB*, XXIX, 8 iii 31).

⁴⁷ See "Early Scribes," p. 176, concerning the Nippur name variously written *A-ri-ka-zu* (*PBS*, II, Part II, 9:7), and *A-ri-ik-ka-zu* (*ibid.*, 132:7). An explanation of Nippur *kazu* may be sought in Nuzi *kašu*, as in *ʿKašum-menni*, written *ʿKa-šu-um-me-en-ni* (G 9:2, 6 ff.). Relationship with Nuzi *Kazuḫe*, written *Ka-zu-uh-ḫé* (JEN 501:26), *Ka-zu-uh-ḫe* (JEN 557:4, 14), *Ka-zu-uh-ḫe* (HEN 624:3, 15), is not so much obvious as apparent, for in Nippur it may perhaps have been written **Ka-su-uh-ḫe*; see Part IV of the present article. The writer takes this opportunity to disavow his association of Nippur *kuša* in "Early Scribes," p. 177, n. 68, with the deity *Kuṣuḫ*. He now sees in it underlying *kušša*.

⁴⁸ *Še-ir-zi-ia* in Clay, *PNCP*, p. 131, collated by the writer at the University Museum in Philadelphia.

⁴⁹ Various written *Še-er-ši-ia* (e.g., JEN 23:25, 43), *Šér-ši-ia* (e.g., JEN 246:16, 26), *Še-er-še-ia* (JEN 47:23), *Še-er-ši-a* (JEN 119:23). Cf. Hurrian *še-er-še* (*KUB*, XXVII, 38 ii 12, 18).

⁵⁰ On the element and word *mušni* see Thureau-Dangin, *RA*, XXXVI, 22 f.

⁵¹ Written *A-ra-mu-su-ni* (*PBS*, II, Part II, 110:9). On the conflicting evidence of *A-ra-mu-su-ni* from Tell Atchana see n. 58.

⁵² Very common at Nuzi, where it is written throughout as *Ḫa-ši-ib-til-la* (see, e.g., JEN 85:35, 39).

In "Early Scribes" the writing of the Hurrian texts at Mari was attributed to native Semitic scribes, for there the stop pattern predominates throughout.⁵³ Consequently, the sibilant uniformly represented by the Hurrians as *š* should and does occur as *z* in Mari when intervocalic. Thus *ha-za-aš-ta-ri* (Mari 3:18, 19) seems to be formed on Hurrian *haš*, "listen." But conclusive is *ki-ia-ze-ni* (Mari 5:19), which, like Ch.B. *kiyaze* alluded to above in this section as well as in Part I, cannot avoid comparison with Hurrian *kiaše*. Likewise *pa-za-la* (Mari 6:12) seems to be Hurrian **pašala*,⁵⁴ and *pu-za-al* (Mari 6:11), like Ch.B. *puzum*, seems to reflect Hurrian *puš*. Hurrian *taše(n)ni*⁵⁵ is quite certain in *ta-ze-né-e* (Mari 1:7), *ta-ze-né* (Mari 1:9), *ta-ze-ni-ti* (Mari 1:8). Hurrian *tiš*, "heart," may very well be sought in *ti-za-da!* (Mari 6:5), *ti-za-we* (Mari 6:3), and *ti-zi-in-ni-^{en}* (Mari 6:6). Finally, *wa-zu-um* (Mari 3:23, 24) is clearly formed on Hurrian *waš*.⁵⁶

Interpretation of this Hurrian sibilant as *š/z* was not constant, for the Akkadian scribes at Nuzi mostly transcribe it as *š*.⁵⁷ In other rare

⁵³ "Early Scribes," p. 184.

⁵⁴ The root evidently being *paš*; cf. *pa-ša-a-e* (KUB, XXIX, 8 iii 14), *pa-ša-la-a-e* (KUB, XXIX 8 iii 14), *pa-a-ša-na-e* (KUB, XXIX, 8 iii 5, 14), *pa-a-ši* (KUB, XXIX 8 iv 24), *pa-a-ši-pa* (KUB, XXIX, 8 iii 36, iv 18), *pa-a-ši-ta* (KUB, XXVII, 42 rev. 17), *pa-a-ši-tù* (KUB, XXVII, 42 obv. 11), *pa-a-ši-ú-z*. . . (KUB, XXVII, 34 iv 7).

⁵⁵ Perhaps a derivation of Hurrian *taše*. Cf. *ta-še-ni* (KUB, XXIX, 8 iv 3); *ta-še-e-né-e-we* (Tuš. i 91, 92, 99, 104); *ta-še-e-en* (Tuš. i 90); and *ta-še* (Tuš. i 85; KUB, XXVII, 1 ii 10, 11; 6 i 19); *ta-a-še* (KUB, VII, 56 i 22); and, finally, *ta-še-e-e-na*^{MEŠ} (Tuš. i 88). Another derived form occurs as the element *tašenni*, apparently identical with a place name and frequent in Nuzi Hurrian names (see Oppenheim, *AOF*, XII, 39; Purves, *JAOS*, LVIII [1938], 463, and "Early Scribes," p. 177 and n. 69). Oppenheim (*loc. cit.*, n. 55) and writer (*loc. cit.*) interpreted Nippur *Hu-di-ti-še-en-ni* (CBS 3480 iv 8) as derivation from underlying *Hu-tip-tašenni*. However, if such were the case, *Hu-di-ti-ši-zi-en-ni* would be the expected writing, and the suggested interpretation is perhaps wrong. Collation of the Nippur name in Philadelphia revealed it to be correctly transliterated by Clay (*PNCP*, p. 80). No opinion on this form can be ventured at present.

⁵⁶ Cf. *wa-a-ša*. . . (KUB, X, 63 ii 13), *wa-a-a-ši* (KUB, XXVII, 44:2), *wa-ša-i-na-an* (Tuš. iii 33), *wa-še-e-we* (Tuš. iii 112), *wa-a-aš-na-e* (Tuš. iv 64), *wa-a-a-šu* (KUB, XXIX, 8 ii 38), *wa-a-šu-ul-[i]*. . . (KUB, XXVII 37:10), *wa-a-a-šu-uš* (KUB, XXIX, 8 ii 43, 44). In Nuzi names cf. ¹Waš-elli, written ¹Wa-še-el-li (JEN 432:3, 6, 14 ff.); ¹Menni-waše, written, e.g., ¹Me-en-ni-wa-še (SMN 347, 635); and ¹Uwur-waše, written, e.g., ¹U-wu-ur-wa-še and ¹U-wu-ur-wa-še-e (SMN 347, 360).

⁵⁷ Perhaps because the Hurrian environment at Nuzi accustomed them to the true nature of the phoneme. *Tāb-milk(i)-abi* betrays his original background by what must have been a reversion to natural inclination by writing *Ha-zi-ib/ip-sē-[la] < ak > -ku* (JEN 568:31), a form which must express the underlying hybrid name **Hašip-silakku*. Cf. the Nippur Akkadian name *Ardi-šī-la-ak-ku* (OBS 11831 in Clay, *PNCP*, p. 58). Elsewhere in JEN 568 *Tāb-milk(i)-abi* does not revert to type, since he writes *Ha-ši-ia* in ll. 1, 8, and 11 instead of *Ha-zi-ia*, which appears in JEN 570:32, a tablet written by the unknown Akkadian scribe of Nuzi (on whom see "Early Scribes," pp. 171 f.).

In "Early Scribes," p. 184, the writer suggested that Ug. Voc. was written by a Semitic scribe in view of the stop-pattern which dominates it. But there is not the expected *š/z*

instances it seems to occur as *s*.⁵⁸ However, the distinction between the voiced and voiceless phases of the Hurrian sibilant known as *š* is strictly observed in the Hurrian texts written in the alphabetic script of Ugarit. In them the stop pattern was first observed, and on that basis the writer attributed them to native Semitic scribes.⁵⁹ In the Ugarit texts the sibilant known as *š* is alternately written with the puzzling sign generally transcribed as *z*⁶⁰ when the pattern calls for a voiced sound, and with the one recently termed *θ*⁶¹ when the pattern

variation. Perhaps the scribe who wrote it was a Hurrian attempting to emulate his Semitic preceptors in following the stop-pattern. In Nuzi a parallel is to be found in the style of Šumu-libši, son of Šamaš-bāri, discussed in "Early Scribes," p. 186. Yet again the Ugarit scribe may have been a Semite, who, like his compatriots at Nuzi, correctly understood the nature of the Hurrian *š*.

⁵⁸ The name apparently written ¹*Ha-si-AN.PA-ri-gi* (KAJ 192:12) may reflect Hurrian *haš*. A good variant of Nippur *Aram-muzni*, written *A-ra-mu-uz-ni* (*PBS*, II, Part II, 110:9) and reflecting Hurrian **Aram-mušni*, was brought to the writer's attention by Dr. Gelb. It is evidently written *A-ra-mu-su-ni* and occurs on a Tell Atchana tablet (see Sidney Smith, *Antiquaries Journal*, XIX [1939], 46), emanating from the Old Babylonian period. The writer is unable to offer an opinion on the factors involved in this evidence.

⁵⁹ "Early Scribes," pp. 183-85.

⁶⁰ Sign No. 27 in Gordon's list, *An. Or.*, XX (1940), 11. For discussions on this sign see Ginsberg and Maisler, *JPOS*, XIV (1934), 244 ff.; Harris, *JAOS*, LV (1935), 95-100; Friedrich, *An. Or.*, XII (1935), 129 f.; Speiser, *JAOS*, LVIII, 175-93; and Gordon, *loc. cit.*, paragraphs 4.1, 4.4, and 4.22. The writer uses the transliteration *z* employed by Harris. Its exact phonetic nature is not yet understood, but it has been shown that it varies with Ugaritic *z* in the local word for "breast" (see, e.g., Speiser, *op. cit.*, p. 178). This agrees with the demonstrable correspondence of *z* with *z/š*, employed by Babylonian scribes to represent the voiced phase of the sibilant appearing as *š* in Hurrian texts.

The writer diverges from the current view that *z* has basically a Hurrian origin. Although it occurs chiefly in Hurrian words, the fact remains that it does occur in Semitic words and may have had a Semitic origin. It represented perhaps an Ugaritic phoneme undergoing a change making it merge finally with Ugaritic *d*. See, e.g., Harris, *Development of the Canaanite Dialects* ("American Oriental Series," Vol. XVI [1939]), p. 36, and then Gordon, *loc. cit.*, paragraph 4.1, where a derivation from Arabic *dal* is discussed. Curiously enough, its voiceless counterpart in the Hurrian texts from Ugarit is *θ*, which is cognate with the corresponding Arabic voiceless sound *ṭa* (see following note).

⁶¹ Sign No. 29 in Gordon's list, *An. Or.*, XX, 11. The writer follows Speiser, who proposed *θ* in *JAOS*, LVIII, 175-79, to represent this sign. Its phonetic character is still a matter of debate. The underlying sound was a distinct phoneme in Ugaritic corresponding to the sound merging with *š* in the West Semitic languages spoken to the south of Ugarit (see Harris, *op. cit.*, pp. 40 f., and A. Goetze in *Language*, XVII [1941] 168 f.). However, that is because Ugaritic has a closer affinity to East Canaanite or Amurrite (see Goetze, *Language*, XVII [1941], 127-38), which also preserves the distinction between primitive Semitic *t* and *š*. The idea still prevails that *θ*, since it was a separate phoneme, had a dental rather than a sibilant character. However, the voiceless phase of the sound which appears in Hurrian writing as *š*, while rendered as *θ* by the Ugarit Semites, is variously rendered by *s* and *š* by Nippur scribes. In addition, *θbl* in the Hurrian text RS 4:27, 33 seems to vary with *sbl*, *ibid.*, l. 8. This speaks more for a sibilant than for a dental phonetic effect. In fact, the *s* in the Nippur writings referred to suggests that Akkadian *š* may at times have veered to *sin*.

Furthermore, the voiced counterpart of the sound represented as *θ* in Ugarit and as *s/š* at Ch.B. and Nippur is *q/z* at Ch.B. and Nippur. This relationship then seems to be,

calls for a voiceless sound. Ugarit \bar{z} , then, bears the same relationship to θ as z/\bar{s} does to \bar{s} in Hurrian names and words written by Babylonian scribes. Thus intervocalic \bar{s} is expressed as \bar{z} in the verbal morpheme $-u\bar{s}a$ in $\bar{a}r\bar{z}ln$ (RS 30:2, 4; 372 rev. 7, 15), $\bar{a}r\bar{z}nnk$ (RS 4:9, 16, 28, 34), and $t\bar{g}\bar{z}nnk$ (RS 4:49), which represent formations on $aru\bar{s}a$ - and $tehu\bar{s}a$ -, respectively.⁶² Similarly, Hurrian $enna\bar{s}a$ ⁶³ results in $\bar{e}n\bar{z}$ (RS 4:60, 61). The phrase $ha\bar{s}ari ha\bar{s}ulie\bar{s}$, so frequently met in the Boğazköy texts, has long since been associated with $h\bar{z}r h\bar{z}l\bar{z}$ (RS 4:1, 6, 10, *et passim*).⁶⁴ Also in the same sources is the deity $Wi\bar{s}ai\bar{s}aphi$, appearing in Ugarit as $P\bar{z}\bar{z}ph$ (RS 4:35, 37) and $P\bar{z}\bar{z}phnd$ (RS 50:5).⁶⁵ The voicing of the sibilant after r is to be observed in Ugarit $\bar{I}r\bar{z}p$ (RS 4:41) and $\bar{I}r\bar{z}pn$ (RS 4:42), which represent the deity known as Iršappi.⁶⁶ Beside m it is apparently voiced in accordance with the pattern, for the name $\bar{A}\bar{z}mny$ (RS 14:2), while it recalls to Harris the deity $E\bar{s}mun$,⁶⁷ is also strongly suggestive of the Nuzi name $\bar{A}\bar{s}mun-na\bar{j}a$.⁶⁸

While the voiced aspect of \bar{s} is approximated by z/\bar{s} by Akkadian scribes and \bar{z} by the Semitic scribes at Ugarit, the voiceless counterpart is rendered generally by \bar{s} by the former and θ by the latter. This is clearly illustrated by the occurrences of \bar{s} and θ in initial positions,

broadly speaking, one between the sibilants s and z . The Hurrians also had another sibilant which the writer expresses as s (see Part IV of the present article), since it appears as such at Nippur. Just what the phonetic distinction was between these two Hurrian phonemes is not clear. They must have been similar, for they interchange.

⁶² For $ar\bar{z}$ - etc. see Speiser, *Language*, XVI (1940), 322. The analysis of $t\bar{g}\bar{z}nnk$ is one on which Professor Speiser and the writer found themselves in agreement during a conversation a few months ago. Also to be considered is $hd\bar{z}nnk$ (RS 4:14, 21, 37, 46, 59), which must be Hurrian $*hut\bar{u}\bar{s}a$. The verbal element hut is very common in Hurrian names.

⁶³ Cf. $enna\bar{s}a$, written $DINGIR^{ME\bar{s}}na-a-\bar{s}a$ (KUB, XXVII, 39 obv. 3; 42 obv. 33; 43:8; KUB, XXIX, 8 iii 54, iv 26, 29). Cf. also $enna\bar{s}i$, written $DINGIR^{ME\bar{s}}na-a-\bar{s}i$ (KUB, XXVII, 42 obv. 14).

⁶⁴ By J. Friedrich in *AOF*, X (1935-36), 295. See also C.-G. von Brandenstein, *ZDMG*, XCI, 559. For a penetrating exposition of $-lie\bar{s}$ and its function see Gotze in *RHA*, V Fasc. 35 (1939), 103-8.

⁶⁵ Association made by B. Hrozný, *AOr.* IV (1932), 120 f., and Friedrich, *An. Or.*, XII (1935), 127 and n. 2, and 129 f.

⁶⁶ See Friedrich, *An. Or.*, XII, 129, 130 f. The Ugarit personal name $\bar{E}ur\bar{z}r$ (I. 1 of tablet published by E. Dhorme in *Syria*, XIV [1933] 235-37) is generally associated with $Ewari\bar{s}arri$, misunderstood as $Ew(i)ri\bar{s}arri$, on which see n. 19. Normally $\bar{E}ur\bar{z}r$ is expected. The writer feels that normal $Ewari\bar{s}arri$ had become $Ewar\bar{s}arri$ at Ugarit and that the \bar{s} had become voiced as it had in $En-zugri$ and $\bar{A}\bar{s}tu-zar$. It is probable that the Nuzi element $ewara$ - is identical with $ewari$ - in the name under discussion.

⁶⁷ See *JAOS*, LV, 98.

⁶⁸ Written $\bar{A}\bar{s}-mu-un-na-a-a$ (SMN 1251).

where the pattern calls for voiceless sounds. The evidence to be drawn from the sources utilized in this article is so abundant and so conclusive that investigation of the matter is hardly necessary here.

However, before passing on, it is quite necessary to remark that the voiceless aspect of the spirant in question, although mostly rendered by \bar{s}/θ by Akkadian and Ugaritic scribes, undergoes a phonetic deviation influencing them at times to render it by signs containing Semitic s . Hurrian $\bar{s}amp$ as portrayed by Šampiija⁶⁹ and Kel-šampa⁷⁰ at Nuzi is to be found in Sambī⁷¹ and Sa(m)biḫ-ari⁷² at Nippur. The name Šellu at Nuzi is Sellum or Selli⁷³ at Nippur. Unless si is to be read $\bar{s}i$ at Ch.B., the theophorous element $-šimika$ appears to be $-simiga$ in the Ch.B. name $\bar{H}a-zi-ib-si-mi-ga$.⁷⁴ Hurrian $*šinenni$, apparently formed on $\bar{s}inen$, as, e.g., in Ch.B. $\bar{S}inen-na\bar{j}a$ and $\bar{S}inen(?)\bar{s}alli$, results in the name Sinenni at Nippur.⁷⁵ Hurrian $\bar{s}umm$, as represented perhaps in $\bar{s}u-um-mu-un-ni-we$ (KUB, XXVII, No. 1 ii 10), $\bar{s}u-mu-ni-we$, "of the hand" (Ug. Voc. ii 3), and in the Nuzi names Šummiija⁷⁶ and Šummi-šenni,⁷⁷ appears to Akkadian ears as $Sul-um-še-en-ni$ (BE, XIV, No. 14:8),⁷⁸ at Nippur and $Sum-mi-te-šu-ub$ (KAV 30b:5)⁷⁹ at Aššur and perhaps lexically as $sú-mu-un$ (Mari 4:28). The same variation finds expression perhaps in θbl (RS 4:27, 33) and sbl (*ibid.* l. 8).⁸⁰

Doubling of the spirant, reflected in Hurrian writing as $\bar{s}\bar{s}$ is also frequently defectively written as a single \bar{s} by Akkadian scribes. This laxity has caused the name of the deity Teššup to be universally misunderstood as Tešup. The correct version is given in the Tušratta let-

⁶⁹ Written $\bar{S}a-am-pi-ia$ (JEN 243:21; 289:29).

⁷⁰ Written $Kc(ri)-el-ša-am-pa$ (AASOR, XVI, 95:14).

⁷¹ Written $Sa-am-bi$ (BE, XIV, 12:2).

⁷² Various written $Sa-bi-ḫa-ri$ (PBS, II, Part II, 90:6) and $Sa-am-bi-ḫa-ri$ (BE, XIV, 12:9).

⁷³ The Nuzi form is written $\bar{S}e-el-lu$ (JEN 151:24; CT, II, 21:23, 24). Cf. Nippur $Se-el-lum$ (BE, XV, 194:4) and variant $Se-el-li$ (*ibid.*, 198:93).

⁷⁴ Unless $\bar{H}a-zi-ib-šī-mi-ga$ is the correct reading.

⁷⁵ Written $Si-né-en-ni$ (CBS 4572), cited as $Si-ni-en-ni$ in Clay, *PNCP*, p. 125.

⁷⁶ See, e.g., $\bar{S}um-mi-ia$ (JEN 5:19, *passim*) and $\bar{S}u-um-mi-ia$ (JEN 211:15).

⁷⁷ See, e.g., $\bar{S}um-mi-še-en-ni$, AASOR, XVI, 29:29.

⁷⁸ $Lu-um(ap)-še-en-ni$ in Clay, *PNCP*, p. 103.

⁷⁹ The writer abandons the reading $\bar{S}um-mi-te-šu-up$ in "Early Scribes," p. 178.

⁸⁰ As noted also by Speiser, *JAOS*, LVIII, 177, but not necessarily an error as Speiser states. Perhaps underlying form is something like $\bar{s}ap-la$ (KUB, X, 63 ii 13).

ter, where it is written ^d*Te-e-šš-šu-pa-aš* (Tuš. i 76, ii 65, iv 118) and ^d*Te-e-šš-šu-u-up-pē* (Tuš. ii 77). When not expressed ideographically in the Hurrian texts from Boğazköy, it is written ^d*Te-e-šš-šu-up-pi* (KUB, XXVII, No. 38 ii 14, 20; iii 2, 4, 6). As the already cited Dilbat name ^d*Te-e-šš-šu-ub-a-ri* attests, the actual quantity of the double consonant was perceptible enough to be significant to the Akkadian scribe who wrote it. This is rather unusual, for, as in the Mari writing *Te-šu-ba-am* (Mari 1:34), the single *š* indicates that scribes did not understand that actual length was involved.

That writings in *šš* reflect quantity has already found exemplification in Part I with regard to Nuzi names in which the formative *-n* is assimilated to a following *š*. Similar evidence is yielded by Ch.B. *A-ra-an-zi-iḫ-a-ri*, also mentioned in Part I. The underlying Hurrian form of the initial element would seem to have been **aransih*, but quite possibly it may have been **aransih*.⁸¹ Whatever it was, the *-n* evidently assimilated to the following consonant with the resultant Nuzi form *aransih*.⁸²

In Ch.B., *ešše* (as in 'Amman-ešše) is written *-e-še*. The Nuzi elements *-šalli* and *-elli* appear in Ch.B. as *-ša-li* and *-e-li*.⁸³ The Mari texts show some evidence of this misconception. That *aššenel* may be the underlying form for *'a(?) -še-né-el* (Mari 4:25) is suggested by the writing *a-aš-še-iš* (KUB, VII, No. 56 ii 19) from Boğazköy. Likewise *iš-ši-na-a-an* (Tuš. iii 66) implies underlying *iššamma* for *i-ša-am-ma* (Mari 1:30 and 2:14, 15), since both seem to occupy the same syntactic position in the sentences in which they occur. More acceptable evidence is revealed when *pa-ši-ib* (Mari 1:3, 6, and 2:9) is confronted with *pa-aš-še* (Mari 6:20). Likewise *pi-ši-di-in* (Mari 5:16) appears as a defective writing of the phonetic form *piššidin* when compared with *pi-iš-ši-la*, two lines above in the same document.

⁸¹ Which recalls the river Aranzuh, so Dr. Gelb reminds the writer. Cf. equations *Arans/zuh* = *Idiqlat* (in *Subartum/Iamutbal* in synonym list published by W. von Soden, *ZA* (N.F.), IX (1936), 235, l. 44, and *Aransuh* = *Idiqlat* (in *Iamutbal* in synonym list published by *idem*, *Die lexikalischen Tafelserien der Babylonier und Assyrer in den Berliner Museen*, II (Berlin, 1933), No. 8126. See also H. G. Güterbock, *ZA* (N.F.), X (1938), 84, n. 1, on this and the Hittite form *A-ra-an-za-ḫu*. Hurrian **Aransih* with a *s/š > z* variation under pattern conditions seems very likely.

⁸² In *Arip-araših*, written *A-ri-ip-a-ra-ši-iḫ* (SMN 20), and *[Ḫa]šip(?) -araših*, written *[Ḫa(?) -š]-ip-a-ra-aš-ši-iḫ* (JEN 659:39). In the Nuzi rendition of *Arip-araših* note the failure to write double consonants when they are required.

⁸³ See Ch.B. *'A-ḡa-ḡab-e-li* (transliterated *A-ta(?) -ab(?) -e-li* in *Iraq*, VII, 36), *'E-de(?) -en-e-li*, *'Ki-ri-ib-e-li*, *'Nu-bur-e-li*, *'Ta-qa-al-e-li*, *'Ši(?) -né(?) -en-ša-li*, *'U-nu-ūš-ša-li*.

That quantity is involved becomes quite evident when *ú-šu-ul* (Mari 1:2) is compared with *uš-šu-li-e* (KUB, XXIX, No. 8 iii 51) and *u-uš-šu-li-e-eš* (KUB, XXVII, No. 42 rev. 24) from Boğazköy. The defective writing *ú-šu-um* (Mari 2:18) is clearly identical in form with *uš-šu-u-um* (KUB, XXVII, No. 38 iii 4, 6, 7, 10, 12) from Boğazköy Hurrian. The quantitative character of the sibilant in *wa-šu-me* [. . .] (Mari 3:26) becomes clear upon comparison with the Boğazköy Hurrian forms *wa-a-aš-šū* [. . .] (KUB, XXVII, No. 36 iv(?) 7) and *wa-a-aš-šū-u-ša-a* [. . .] (*ibid.*, 1. 6).

Now that it has been shown that the double sibilant was at times misunderstood as a single voiceless sound, the writing *te-šub* finds a ready explanation. Its use in Nippur names to represent the theophorous element *-teššup* results, no doubt, from imperfect understanding of the matter by the native scribes. At Nuzi, although *-teššup* is often correctly written in personal names, the writing *-te-šup* was preferred by the Hurrian scribes who should have known better. Thus, after having been a defective writing, it probably became a convention, recommending itself by its economy. To Nuzi scribes, who were very lax in expressing doubled sounds in writing, this short cut must have been very welcome, for *-teššup* is the most common element in Nuzi personal names.

An examination of the material adduced as evidence shows quite clearly that Akkadian scribes often overlooked the factor of quantity. It has already been observed that, when the Hurrians doubled a consonant, quantity alone was phonemic to them. However, the phonetic result was twofold: quantity and voicelessness. While the Hurrians were conscious of the former, they were quite indifferent to the latter. Conversely it may be said of Akkadian listeners that they were always aware of the voiceless effect, but only occasionally did they show themselves aware of quantity, which was the real issue at stake.

Up to now the phonetic behavior of the sibilant represented as *š* by the Hurrians has shown itself to be identical with that of the stops. However, this perfect agreement is now sharply interrupted. Postvocalic stops when final seem to be generally voiced, although there are some exceptions. The sibilant in question is consistently voiceless under these circumstances. The Ch.B. names *Muzum-ari* and *'Awešmuze* include the elements written *-mu-ze* and *mu-zu-um* based on the Hurrian root *muš*. When Hurrian *muš* occurs alone without the em-

bellishments of stem vowel or other formatives, the phonetic result is not *muz* or the like but *muš*, as in *mu-úš* (Mari 6:10, 11, 15, 19). What is more, final *š* is unanimously revealed in *al-lu-la-da-i[š]* (Mari 5:7), in *a-ni-iš* in the name *Aniš-ḫurbi* from a Mari king list,⁸⁴ in *aš-ti-ni-iš* (Mari 4:25), *a-wa-an-du-úš* (Mari 3:23, 26), and in *a-we-eš* in Ch.B. 'Aweš-muze discussed above. Other instances of final *š* are *e-ni-eš* (Mari 6:12, 21), *e-ni-iš* (Mari 1:32 and 2:12, 16), *e-ni-wu-úš* (Mari 6:10, 11, 18, 19), *ḫi-ia-ri-ia-aš* (Mari 4:28), *ḫi-in-zu-ru-úš* (Mari 6:7), *ke-ra-ri-ia-aš* (Mari 4:27), *i-su-di-iš* (Mari 5:6), *mu-ga-ri-iš* (Mari 6:15), *pa-ar-ḫi-wa-aš* (Mari 6:13), *ša-ma-ḫa-aš* (Mari 4:22), *ta-nu-úš* (Mari 6:17), *ú-né-eš* (in Ch.B. 'Uneš-na), and *ú-nu-úš* (in Ch.B. 'Unuš-kijaze, 'Unuš-šalli, and 'Unuš-umar).⁸⁵

Final *š* is phonetically so well entrenched that it has a strong retrogressive force. What is by now the well-known ending *-šuš*, a plural subject or agentive form,⁸⁶ should be something like *-zuš* at Mari. As such it is to be found in [. . .]'*e-ḫu-na¹-zu-úš* (Mari 6:14). But this instance seems exceptional in view of *pa-pa-na-šu-uš* (Mari 5:8) and *ši-we-na-šu-úš* (Mari 5:9), which apparently exemplify a retrogressive assimilative force of the final sound. Oddly enough this process happens to preserve a phonetic effect in keeping with the Hurrian phonemic system.

The sibilant, when final, results in *θ* in Ugarit writing, as e.g., in *Kmrbnθ* = *Kumarbiniš* (RS 4:8), *kldnθ* = *keldiniš* (RS 4:2), *enθ* = *eniš* (RS 4:39). However, difficulty is presented by *ḫzłz*, mentioned above, which has been compared with *ḫašulieš*. The form to be expected in Ugarit is *ḫzłθ*, like *pžłθ* (RS 4:36). According to the conclusions attained so far in this article, the actual Ugarit writing *ḫzłz* implies a form in which the final sibilant is followed by a vowel. While suggesting this hypothesis as a possibility, the writer does not insist upon it. First of all, all the evidence serving to demonstrate such grammatical variations of the *-lieš* formation is not available. Second, there may be involved phonetic processes which the writer, for one, is not at present competent to deal with. Hence any suggestions offered by him would be a matter of guesswork, which generally turns out to be more misleading than helpful.

⁸⁴ Occurring twice as *A-ni-iš-ḫu-ur-bi* (*Syria*, XX [1939], 109).

⁸⁵ Respectively written 'U-né-eš-na, 'U-nu-úš-ki-ia-ze, 'U-nu-úš-ša-li, and 'U-nu-úš-ú-mar.

⁸⁶ See Friedrich, *Kleine Beiträge zur churritischen Grammatik* (Leipzig, 1939), pp. 10-12.

III

Also subject to pattern was the phoneme variously expressed *p/w*,⁸⁷ for where correspondences are obvious the pattern makes itself felt when this sound is expressed by *p* or *b* by Semites. That true quantity is involved in the postposition *ww*, "my," finds confirmation in *pa-ḫi-ip-pi-ni-im* (Mari 1:32). That the accompanying effect of voicelessness ensued is attested by the writing *p* in Ugarit *atynpθ* (RS 4:3) and *atynpd* (RS 4:4), which must express underlying *attainippaš* and *attainippada*, "my father" and "to my father."⁸⁸ Elsewhere the pattern is followed by Hurrian *want*, which appears as *pand* in 'pa-an¹-di-en (Mari 6:19) and in Nippur *Pa-an-di-ia* (*BE*, XIV, No. 162:8, and XV, No. 199:6).⁸⁹ Similarly, Hurrian *wahr* is expressed as *paḫr* in Ch.B. 'Paḫri-šehirni and 'Paḫri-uzuwe.⁹⁰ Apil-Sin, an Akkadian scribe at Nuzi,⁹¹ writes *Pa-[ar-ḫi-š]e-ni* (SMN 3101) for the common Nuzi name *Waḫri-šenni*. Also concerned are the words *pa-ar-ḫ[a- . . . -d]a* (Mari 3:21) and *pa-ar-ḫi-wa-aš* (Mari 6:13). The deity *Wišaišaphi* is represented as *Pžžpḫ* (RS 4:35, 37, and 50 obv. 5). The deity *Kumarwe*⁹² appears as *Kmrw* (RS 4:6, 7, 8, and 7:1 f., 8 f.). If *b* in *ḫmrbn* (RS 4:60) actually represents the genitive *-we*,⁹³ it does so under pattern conditions, for it is preceded by *r* or by a vowel.

Similarly concerned is the velar spirant *ḫ*, for it is again one of those phonemes which had voiced and voiceless phases unperceived by Hurrians but discernible to the ear of Semites. Again this phonetic distinction is governed by pattern. Such a state of affairs is revealed in Ugarit, where single intervocalic *ḫ*, under its voiced aspect, turns out to have been something like a *ghayin*, hereafter to be symbolized by *ḡ*, for it is under this guise that it appears in the alphabetic writing.⁹⁴

⁸⁷ See F. Bork, *Die Mitannischsprache*, *MVAG*, XIV, Parts 1/2 (Berlin, 1909), 24, and Berkooz, *NDA*, p. 50, G.

⁸⁸ See von Brandenstein, *ZDMG*, XCI, 560, for translation "father."

⁸⁹ Also *Pa-an-di-ia* (SMN 3082, 3094, and 3101), written by Apil-Sin, an Akkadian scribe at Nuzi, on whom see "Early Scribes," p. 171.

⁹⁰ Written 'Pa-ḫ-ri-še-ḫi-ir-ni and 'Pa-ḫ-ri-ú-zu-we.

⁹¹ See "Early Scribes," p. 171.

⁹² So expressed by the writings ⁴*Ku-mar-w[e]* (*KBo*, V, 2 ll 60), ⁴*Ku-mu-ur-we* (*AASOR*, XVI, 47:1; 48:1), ⁴*Ku-mar-we-ne-eš* (*KUB*, XXVII, 38 iv 21), and *Ku-ma-ar-wi-ni-da-al* (Mari 5:4).

⁹³ As suggested by von Brandenstein, *ZDMG*, XCI, 568.

⁹⁴ Sign No. 21 in Gordon's list, *An. Or.*, XX (1940), 10, the reading of which is still in dispute. It was read *x* by Friedrich, *An. Or.*, XII, 126 f., in the form *tθb ḫlθg/x* = Hurrian

Thus *h̄lb̄g* (RS 4:10) seems to be identical with the Hurrian form *h̄alpahe*.⁹⁵ By the same token, *lbt̄g* (RS 4:36), as Speiser suggests,⁹⁶ should be *lubtuhe*. Ugarit *p̄gn* (RS 372 A 18) and *p̄gdm* (RS 4:3) are reminiscent of the stem *paḥi* occurring in Boğazköy texts.⁹⁷ With greater certainty *iḡznnk* (RS 4:49) can be interpreted as a form arising out of Hurrian *teḥuša*.⁹⁸ In the same line a stem based on *teḥ* is to be found in *iḡ*. The voicing of Hurrian *h̄* under the guise of *ḡ* beside *r* is demonstrated by the Ugarit treatment of Hurrian *niḥr*, as in *n̄grn* (RS 4:53 and 372 A 11) and *n̄rgp* (RS 4:58), which reveals the metathesis apparent in *ni-ih-ri-ia* (KUB, XXVII, No. 34 iv 10) and *ni-ir-ḥi-ia-aš-ši* (KUB, XXVII, No. 34 iii 13).⁹⁹ In addition, *agr* (RS 7:1, 8 and 34+35:1, 13) apparently expresses the root forming the basis of Hurrian *a-aḥ-ra-a-i* (KUB, XXVII, No. 23 iii 4) and *aḥruḥi*.¹⁰⁰ That *h̄* can be voiced when final is suggested by *eykz̄g* (RS 4:15), which suggests the phrase *eiḥa Kuḥuḥ* (the word-divider being omitted, as it often is in Ugarit texts).

Elsewhere, i.e., when initial and when doubled, Hurrian *h̄* appears as Ugarit *h̄* in examples too abundant to be cited here. Nevertheless, it is noteworthy that etymological evidence for quantitative *h̄h̄* may be sought in the Hurrian word for "female," occurring alternately as

Teššup Ḥalpahi, a comparison previously suggested by Hrozný in *AOr*, IV (1932), 128. Von Brandenstein treats it as sort of a sibilant. However, D. H. Baneth (*OLZ*, XXXV [1932], 705) and Ginsberg (*OLZ*, XXXVI [1933], 593 f.; Virolleaud, *Syria*, XIII [1932], 125, n. 1) read it as *ḡ*. Speiser (*JAOs*, LVIII, 197-201) read *ḡ* but later in *Language*, XVI, 334-36 became involved in uncertainties arising from method rather than from the evidence.

The Hurrian phoneme *h̄* in its voiceless aspect is practically the same as Semitic *h̄*. Under its voiced aspect it became something like *ḡ*, a change which was a matter of indifference to the Hurrians but which misled the Semitic listeners at Ugarit into thinking that an actual phonemic difference analogous to their own was involved. This misconception is not observable among Akkadian scribes who had to deal with Hurrian. In the first place, *ḡ* was not one of their phonemes. Second, the cuneiform syllabary provides no ready means which would have enabled them to express any phonetic difference they may have noticed between *h̄* and *ḡ*.

⁹⁵ See preceding note.

⁹⁶ In *Language*, XVI, 335.

⁹⁷ Cf. *pa-a-ḥi* (KUB, XXIX, 8 iv 11, 25), *pa-a'-ḥi-la'-a'-e-na* (KUB, XXVII, 38 i 14), *pa-a-ḥi-ip* (KUB, XXIX, 8 iii 21), *pa-a-ḥi-pa* (KUB, XXIX, 8 ii 36), and *pa-a-ḥi-ta* (KUB, XXIX, 8 iii 9, 12, 18).

⁹⁸ See n. 62.

⁹⁹ *Niḥr* occurs also in the Nuzi names Nihrija, Nihri-teššup. For an instance of the same metathesis as observable in Nihri-teššup and Nirhi-teššup see Berkooz, *NDA*, p. 64.

¹⁰⁰ Occurring frequently in Boğazköy texts. See A. Götze and H. Pedersen, *Muršili's Sprachlähmung* (København, 1934), p. 31 and n. 1.

aštuḥena and *aštuḥhena*.¹⁰¹ Outside of the fact that the former quite possibly is defectively written, the quantity in *h̄h̄* of *aštuḥhena* in one instance can perhaps be accounted for by the ignored form *aš-tù-uš-ḥe-e-na* (KUB, XXVII, No. 3 i 12), which indicates original *šh̄* > *h̄h̄* by assimilation. That *š* has assimilative tendencies becomes evident when the Nuzi name Kuš-ki-pa is compared with its variant Kuk-ki-pa.¹⁰²

IV

The phonetic system which was termed as stop-pattern at the beginning of this article turns out to be a consonantal pattern that involves the stops *k*, *p*, and *t* and the spirants *ḥ*, *š*, and *p/w*. Excluded from this scheme are the sonants *l*, *m*, *n*, and *r*. Whether another Hurrian sibilant, this time *s*, not *š*, is to be included cannot be affirmed or ruled out until a very serious study is undertaken. It is well known that in writing during the Old Babylonian period and even later, Akkadian *s* was expressed by *z*-containing signs. The same ambiguity characterizes the writing at Nuzi, as established by Akkadian scribes.¹⁰³ By them *s* was expressed by the signs *zi*, *za*, and *zu*. The sign *si* was used only with the value *ši*. Thus Sin-bani, another member of the small colony of Akkadian scribes, writes the Akkadian form *sasinni* as *sà-si-ni* (HSS, V, No. 65:16, wrongly copied, as discovered by Dr. Lacheman). Akkadian *kisu*, "pouch," is written *ki-si* (SMN 3094:13) by Apil-Sin.¹⁰⁴ Ṭāb-milk(i)-abi writes *si-ki-il-ta* (HSS, V, No. 71:18) and *si-ra-šū-ú* (JEN 404:36) for Akkadian *sikilla*, which designates a type of property, and *sirašū*, "vintner." The Akkadian forms *ipussu* and *mārassu* appear, respectively, as *i-pu-sú* (HSS, V, No. 65:3) and *ma-ra-sú* (*ibid.*, 1. 5) written by Sin-bani.

The sign *si* when encountered in texts written by the Akkadian scribes at Nuzi has the value *ši*, as in *ši-mi* (SMN 3082:10), written by Apil-Sin; *i-ši-mu* (HSS, V, No. 71:4), *ši-im-ta* (*ibid.*, 1. 2), *ši-im-ti*, (*ibid.*, 1. 1), written by Ṭāb-milk(i)-abi. Both Amurru-šar-ilāni and

¹⁰¹ For examples, see von Brandenstein in *ZA* (N.F.), XII (1940), 113.

¹⁰² Cf. *Ku-úš-ki-pa* (JEN 396:13) and *Ku-uk-ki-pa* (JEN 537:3) with same genealogy.

¹⁰³ See "Early Scribes," p. 171, for evidence indicating relationship of Nuzi tablets to those prevalent in the Old Babylonian period.

¹⁰⁴ What Ṭāb-milk(i)-abi meant by writing *gi-is* LUGAL.GAL (JEN 404:26 f.) is not clear.

Táb-milk(i)-abi write *mar-šit-ti* (JEN 414:8 and HSS, V, No. 71:26) for Akkadian *maršīti*.

Since these scribes were Akkadian, they must have known what they were doing, and thus the all-embracing use of *z*-containing signs at Nuzi turns out to be a matter of orthography rather than the result of phonetic misunderstanding of Akkadian sibilants by the Hurrian populace. These rules apply to Nuzi orthography in the main, but there can be found exceptional instances in which *s* seems to be expressed by *s*-containing signs.¹⁰⁵ That this intrusive trend took root in the early scribal period is evident in *É-su = bīssu*, "his house" (JEN 570:20).¹⁰⁶

Nevertheless, it becomes obvious now that *z*-containing signs rather than so-called *s*-containing signs at Nuzi provide the medium par excellence for expressing *s*, a phoneme in Hurrian as well as in Akkadian. Confirmation of this state of affairs is amply provided in the Nippur documents, where, for the most part, *s*-containing signs rather than *z*-containing signs are employed to portray *s*. The actual character of the sibilant in the Nuzi name written *Ka₄-ni-iz-za*¹⁰⁷ is revealed as *s* by the Nippur scribes, who are the most reliable as far as portrayal of actual phonetic results is concerned. They write *Ka-ni-is-si* (CBS 3473 A 3), which imposes the reading *Ka₄-ni-is-sà* for the Nuzi writing. There are many Nuzi names in which suffixal formations involving *ss* are expressed exclusively by *z*-containing signs.¹⁰⁸ In Nippur this suffix is generally written with *s*. The pertinent examples are *A-gi-is-si* (BE, XV, No. 190 ii 31; PBS, II, Part 2, No. 11:7), *En-na-as-si* (CBS A3), *Ka-ni-is-si* (already cited), *Ki-ir-ma/ba-as-si* (CBS 3474), *Pa-pa-as-si* (CBS 3474), *Ta-gu-(us)-si* (CBS 3474). Additional comparisons of this sort change, e.g., Nuzi *Ki-iz-zu*¹⁰⁹ to *Ki-is-sú*,

¹⁰⁵ E.g., in the different writings of the name Partasua (see Berkooz, *NDA*, p. 62). All the same, the question of sibilants in Nuzi is to be reappraised, probably along the lines set forth by Goetze, *Language*, XIV (1938), 136 f., with regard to Nuzi, and recently in *Language*, XVII (1941), 128 f. and n. 15 and pp. 168 f., with regard to conventions in writing in the Amarna letters.

¹⁰⁶ Written by the unknown Akkadian member of the early scribal group at Nuzi (see "Early Scribes," p. 171).

¹⁰⁷ JEN 360:2, 7, 10 ff.

¹⁰⁸ For examples, see Oppenheim, *WZKM*, XLIV, 206.

¹⁰⁹ *AASOR*, XVI, 20:20.

*Pa-az-zi-ia*¹¹⁰ to *Pa-as-si-ia*, *Zi-iz-zi-ia*¹¹¹ to *Si-is-si-ia*, and *Zu-un-na*¹¹² to *Sú-un-na*, for their respective Nippur occurrences are written *Ki-is-si* (CBS 11683), 'Pa-as-si', (BE, XIV, No. 112:15), *Si-is-si* (BE, XIV, No. 19:13, and XV, No. 198:5), and *Si-is-si-ia* (BE, XIV, No. 19:65) and *Su-un-na* (CBS 3480). As observed before,¹¹³ the Nuzi name ordinarily taken as *Zil-teššup* is rendered as *Si-il-te-šub* (PBS, II, Part 2, No. 84:41, and CBS 3480), indicating, of course, that the Hurrian root in question is *sil* throughout, but not *zil*. Likewise the Nuzi names previously understood by the writer and others to be *Zike*¹¹⁴ and *Zikipa*¹¹⁵ turn out to be actually *Sike* and *Sikipa*, for they appear in Nippur as *Si-ge* (PBS, II, Part 2, Nos. 13:45; 84:20, 39; 132:32; BE, XIV, No. 138:3, and XV, No. 198:25) and *Si-gi-ba* (CBS 11869). The name written 'A-zu-e (G 76:6) at Nuzi expresses underlying 'A*ssue*, a pronunciation revealed by 'A*ssu-me* (PBS, II, Part 2, No. 111:3, and CBS 3638).

Obviously *z* whenever encountered in the Mari material and in the Hurrian names from Ch.B. is to be read circumspectly, since readings with *s* now are not only possible but probable. As already observed in Part II, *s* perhaps properly written with *sr* makes its appearance in Ch.B. 'H*a-zi-ib-si-mi-ga*. In Mari properly written *s* occurs in *i-si* (Mari 5:1, 5), *i-su-di-iš* (Mari 5:6), and *ma-ru-sa* (Mari 5:15). That such usage is unusual is indicated by the fact that these writings occur in only one of the six texts concerned, and attest, therefore, the style of a particular scribe. Moreover, the system of writing which clearly distinguished between *s* and *z* or *š* did not become commonplace until the Kassite period. Being sparse and ambiguous, this evidence provides no reliable means for reaching final judgments on Hurrian *s* other than its phonemic existence in Hurrian. The proper time has not yet come to judge whether or not it became voiced as *z* under the appropriate phonetic conditions. What is known is the interchange of initial Hurrian *š* with it discussed in Part II of

¹¹⁰ JEN 5:28 *et passim*. Genealogy imposes the reading <Pa>-*as-si-ia* for JEN 418:28.

¹¹¹ JEN 482:19, 30. This applies, no doubt, to the city name written *Zi-iz-za*, or, better, *Si-is-sà*, in the Nuzi tablets and the identically written name element. Both must be *Sissa*.

¹¹² HSS, V, 16:22, 34.

¹¹³ "Early Scribes," p. 177, n. 66.

¹¹⁴ Generally written *Si-ke*(*ar*) at Nuzi, where it is very common (see, e.g., JEN 46:2, 10 ff.).

¹¹⁵ Various written *Si-ki-pa* and *Si-ki-pá* (JEN 636:5, 8, 9 ff.).

this article. The clue to this and other related problems no doubt lies in the Hurrian written in the alphabetic texts at Ugarit, where *s*, *z*, and perhaps *š* occur.¹¹⁶ However, comparisons of the words in which they occur with known Hurrian words are not at hand, and thus this very fruitful study must await a later date.

V

Despite lack of information about this interesting phoneme *s*, the stage must be set for a survey to determine the phonetic effect of Hurrian consonants when they are contiguous. When the pattern was first discovered, it was observed that stops were voiceless after Ugarit *θ*.¹¹⁷ But *θ* and its counterpart, *š*, in Hurrian written by Akkadian scribes, have been found in this article to represent the voiceless aspect of the sibilant represented by the Hurrians as *š*. The observation alluded to above concerning the voiceless stops after *θ* can now be stated in another way. That is, *when any two pattern consonants occur together, both are voiceless.*

Beginning with the Ugarit consonantal clusters involving *θ*, *θk* occurs in *θuθk* (RS 4:22, 49:2, 6, and 372 B 8) and in *θwθk* (RS 34+45:2, 12, 14) which have long since been identified with the name of the Hurrian goddess Šauška.¹¹⁸ The voiceless cluster *θt* is to be found in *ʔaθh[n]* (RS 4:55), *ʔaθhnm* (RS 4:56), and *ʔaθhnr* (RS 4:58), all based on Hurrian *ašt*,¹¹⁹ in *ʔAθtb* (RS 4:29, 31) and *ʔAθtbd* (RS 50 rev. 3), representing the Hurrian deity Aštapi,¹²⁰ and perhaps in *tnθt* (RS 7:11-12), possibly representing the Hurrian verbal form **tanašta*.¹²¹ To be discounted perhaps is *gθhp* (RS 4:61), which has been tentatively associated with Hurrian *kešhi*.¹²²

¹¹⁶ See Speiser, *JAOS*, LVIII, 177, and von Brandenstein, *ZDMG*, XCI, 575 f.

¹¹⁷ Friedrich, *An. Or.*, XII, 131; von Brandenstein, *ZDMG*, XCI, 574.

¹¹⁸ By Hrozný, *AOr*, IV, 127, n. 1; H. Bauer, *OLZ*, XXXVII (1934), 475.

¹¹⁹ See von Brandenstein, *ZDMG*, XCI, 567 f., and ZA (NF), XII, 113.

¹²⁰ See Hrozný, *AOr*, IV, 123.

¹²¹ Cf. *ta-a-na-aš-to(du)-en* (Tuš. iv 15).

¹²² By von Brandenstein, *ZDMG*, XCI, 569. The initial voiced stop violates the consonantal pattern, while in the Ch.B. name element *-ke-ēš-ḥe* in *Ša-du-um-ke-ēš-ḥe* the stop in question conforms to pattern. Under these circumstances it looks as if the *g* in the RS occurrence under study is actually a word-divider. Hence a word *šhp* is possibly at issue. Speiser (*Language*, XVI, 332) uses *gšhp* as evidence for aspiration of Hurrian stops. To this he couples von Brandenstein's assertion, unconfirmed by citation or reference, that *kešhi* occurs with initial *h* in an unpublished text from Boğazköy. Although the writer believes that aspirated stops are by no means impossible in Hurrian, he does not, for one, feel that the doubtful Ugarit *gšhp* and the purported but uncited form *hēšhi* constitute adequate evidence.

Similar evidence is yielded by Akkadian renditions of Hurrian, viz., Nippur *A-bu-uš-ki* (*BE*, XIV, Nos. 58:34, 60:17, 62:6), Ch.B. *A-re-ēš-ka-an*, *ʔAš-tu-a-la-na* (=ʔAštua-tanna), *ʔAš-tu-e*, and *ʔAš-tu-za-ar* (=ʔAštu-šar), and Nippur *Aš-tar-til-la* (CBS 3480) (=Aštar-tilla). Here the voicelessness of *šk* and *št* is beyond question. In regard to the latter the same is confirmed by *aš-ti-ni-iš* (Mari 4:25), *ḥa-za-aš-ta-ri* (Mari 3:18, 19), *mi-iš-ta-ḥu-úš* (Mari 6:4), *pt!-ši-iš-ti-di-en* (Mari 5:17).

Voiceless *š* occurs immediately adjacent to other consonants presumably voiceless also in [. . . -a]ḥ-šū-úš-ḥi-ni-el (Mari 4:23), *ak-šum* (Mari 3:26), *ku-šū-uk-še-en* (Mari 1:11), *aš-ḥu-u[n]* (Mari 6:14), *uš-ḥa-lu-ru-um* (Mari 4:26), [. . .]-ti-ip-ša-ri (Mari 6:7). In personal names the same occurs in Ch.B. *Na-wa-ar-ni-iš-ḥe* (=Nawar-nišḥe), *Ša-du-um-ke-ēš-ḥe*, and *ʔŠa-zu-um-ke-ēš-ḥe* (=Šatum-kešḥe and perhaps *ʔŠašum-kešḥe*, respectively).

Ch.B. *Ap-ša-am* may perhaps be included, but at present the writer is not competent to deal with it, for here the Hurrian sibilant *s*, not *š*, is concerned, the Hurrian root being *aps*.¹²³ Whether or not the change to *š* is a Hurrian matter, the root seems to be correctly understood in Ch.B. *ʔŠi-in-ap-ze/sē*. Incomplete knowledge of *s* postpones discussion of the problem.

The same effect is revealed in the case of Ugarit *ph*, a voiceless combination exemplified by *Pzšph* (RS 4:35, 37) and *Pzšphnd* (RS 50 obv. 5), which represent the deity Wišaišaphi.¹²⁴

Difference in voicing of consonants performing identical morphological functions under varying phonetic circumstances is well illustrated in RS 50. This document is concerned with offerings made to Hurrian deities and calls for expression of the dative concept. It is not surprising, therefore, to find that the Hurrian postposition *-ta*, "to," appears after the name of each deity. Since these god-names end in vowels, Hurrian *-ta* is expressed by *-d*, showing that the phonetic result was *-da*. In obverse, line 9, of this very text Hurrian *-ta* does

¹²³ As in *ap-[su]-u-ša* and *ap-su-ša-a-ul-la-ma-an* (Tuš. iv 63). But in Boğazköy *ap-ši-úr-ra* (*KUB*, XXVII, 42 obv. 30) and *ap-še-ne-we* (*KUB*, XXVII, 38 ii 22) are puzzling, implying perhaps a *s/š* variation or confusion on the part of Hittite scribes of the two Hurrian sibilants, respectively known as *s* and *š*.

¹²⁴ Friedrich (*An. Or.*, XII, 131) observed that *p* was voiceless beside *h* in the Ugarit occurrence of this divine name.

not occur after a vowel but after θ in $h\delta n\theta t h\delta l r\theta [t]$, which in Hurrian is $h\delta t e n a \delta t a h\delta t e l l u r r a \delta t a$, "to the $h\delta t e$ - $h\delta t e l u r e$ gods."¹²⁵ But here it is written t , not d , for the phonetic result was $-ta$. The suffixal formation $-tukku$ appears in δa - al - $h\delta$ - du - uk - ku (Mari 6:8), where the dental is voiced as a result of its intervocalic position. In the next line it appears as ak - tu - uk - ku , in which it becomes unvoiced presumably along with the immediately preceding k . In his earlier article¹²⁶ the writer demonstrated the same sort of phenomenon in the vagaries of $-te$, the shortened form of the theophorous element $-te\delta\delta ub$ at Nippur. With the resultant loss of the pause between elements it became $-de$ under pattern conditions. But in the name Il - hap / hip - te (CBS 4574), a shortened form of $*Ilhip$ - $te\delta\delta ub$, it resumed its normal voiceless character. Presumably the preceding formative p did the same. The writer alluded also to the behavior of the suffixal formation $-tien$ ¹²⁷ as illustrated by ha - tu - di - en (Mari 5:19) and ki - ip - ti - en in the following line. Voiceless k after p , which presumably is also voiceless, occurs, incidentally, in Ch.B. Na - wa - ar - tu - up - ke (=Nawar-tupke) and Tu - up - ki - ta - na (=Tupki-tanna).

With regard to the matter of contiguous voiceless consonants, certain deviations may be possible, as in $lbt\dot{g}$ (RS 4:36), which smacks very much of Hurrian $luptu\delta e$, "the one of Lupti." Nevertheless, in the majority of instances in which two consonants are encountered in the Ugarit texts, both consonants are represented as voiceless. The testimony from Akkadian scribes provides quite complementary evidence to support this view. Obviously this new law concerning the voicelessness of contiguous consonants is identical with the one concerning the voicelessness of double consonants which merely reflects instances in which contiguous consonants happen to be similar. Thus the consonantal pattern can be described very simply as one in which *consonants are voiceless when initial and contiguous, but voiced when occurring singly after vowels and adjacent to sonants*. With the exception of the sibilant known as δ , they have an inclination to become voiced when final. A much simpler way of describing the situation is to amend the observation Bork made on stops long ago¹²⁸ and apply it

¹²⁵ This point was noted by von Brandenstein, *ZDMG*, XCI, 574.

¹²⁶ "Early Scribes," p. 179.

¹²⁷ *Ibid.*, p. 185, n. 112.

¹²⁸ *Die Mitannisprache*, p. 9.

to Hurrian consonants in general, for voicing of consonants had nothing to do with the Hurrian phonemic system. To all intents and purposes they were essentially voiceless. As already observed,¹²⁹ the Hurrians seem to have pictured all Akkadian stops as voiceless. Accordingly, unless aspiration is to be considered, their system phonemically had only one set of consonants. Under this guise the Hurrian consonantal system is not unusual, for it finds parallels in other languages.¹³⁰

VI

By way of conclusion the writer departs from the field of fact to that of hypothesis in an attempt to explain the relationship between Hurrian and Hittite writing. Since the Hurrians were geographically situated between Hittite Asia Minor and Akkadian Mesopotamia, which was the source of culture, they presumably may have provided a channel through which Akkadian culture was transmitted to Hatti. Accordingly, it may very well have been through them that the Akkadian syllabary came to the Hittites.

If this is what happened, it can be assumed that distinction between voiced and voiceless consonants, customary in Indo-European languages, was felt also in Hittite. The Hurrian consonantal system being what it was, the syllabary would have been one in which this distinction was ignored in the script. For instance, the signs GA, KA, and QA would all three have been represented to the Hittites as having essen-

¹²⁹ As in the case of the Nuzi scribe Atal-teššup (see "Early Scribes," p. 185).

¹³⁰ Dr. Sachs recalled to the writer's attention Sapir's important article, "Sound Patterns in Language," *Language*, I (1925), 37-51, in which among the items studied there is revealed a situation quite comparable to the one demonstrated in the present article. This involves, among other matters, Upper Chinook, a language characterized by consonants which are primarily either voiceless or aspirated. Voicing of the unaspirated consonants occurred under practically the same pattern conditions as in Hurrian without being noticed by the speakers of the language concerned (see *ibid.*, pp. 42-44.). The same phenomenon becomes quite obvious to one who reads L. Bloomfield's article "On the Sound-System of Central Algonquin," *Language*, I (1925), 130-56. See also Bloomfield's *Language* (New York, 1933), pp. 82 f. That the consonantal pattern of the Hurrian type is no uncommon phenomenon is attested by its appearance in Dravidian languages (see G. W. Brown, *JAOS* L [1930], 279 f.), where it appears to be a modern development; see, e.g., Speiser in *Language*, XVI (1940), 339, n. 59, who refers to "Linguistic Survey of India IV," p. 288. The writer finds these parallels most welcome; they suggest that Hurrian too may have had aspirated as well as voiceless stops. As in the Dravidian languages, the stop-pattern may very well have been a transitory phase in the development of Hurrian and related languages. Naturally, the consonantal pattern must be considered as a sporadically occurring linguistic development rather than as evidence of genetic relationship of the languages in which it is observable.

tially the value *ka*. In addition, the Hittites, speaking an Indo-European language, may have readily perceived that these three signs had the value *ga* when following vowels. This would have permitted them to use these signs ambiguously in this regard. When *GA*, *KA*, and *QA* occurred after signs ending in *k*, resulting in intervocalic double *k*, the Indo-European Hittites, like the Semites, probably understood the effect as a single voiceless sound and not always as the intended lengthened sound. Accordingly, they would have been misled to believe that voiceless consonants had to be written double as digraphs. The Akkadian syllabary as utilized by the Hittites for expressing their language conforms to this hypothesis. As is well known, distinction in voice is an equivocal issue as far as the individual signs are concerned. However, as has been observed, many instances of double writings of stops and perhaps of other consonants in Hittite correspond quite regularly to single Indo-European voiceless sounds.¹³¹

Since this hypothesis is based only on inferential grounds, it stands in need of documentary proof, which may or may not come to light. Moreover, it is also quite possible that others may demonstrate it to be incorrect. Therefore the writer, who has an aversion toward so-called "predictions," presents it only as an idea which he strongly believes in for the present. More important than this theory is the factually demonstrable Hurrian consonantal pattern, which is the immediate purport of this paper and which the writer places at the service of those engaged in Hurrian studies.¹³²

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¹³¹ For a summary of the problem, see E. H. Sturtevant, *A Comparative Grammar of the Hittite Language* (Philadelphia, 1933), pp. 74-86, and his confirmatory evidence in *Language*, XVI (1940), 81-87. See also nn. 60 and 61 in the present article, where it is believed that *š* in Hurrian writing might even express a sibilant with *s* and *z* as the respective non-phonemic phases.

¹³² The conclusions reached in the foregoing article conflict with certain views expounded by Professor E. A. Speiser, in *Language*, XVI, 319-40, in an article dedicated to the thesis that double writings in Hurrian texts were digraphs expressing single voiceless consonants. The writer, on the other hand, holds that, since difference in voice was not phonemic in Hurrian, double consonants in Hurrian writing were simply a direct expression of double consonants in speech and hence voiceless like any other Hurrian consonantal cluster. The reader is advised to read Professor Speiser's article to see the other side of the question.

THE ORIENTAL INSTITUTE ARCHEOLOGICAL REPORT ON THE NEAR EAST, 1941

Compiled by

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EGYPT AND NUBIA

Abusir (in the Delta). Department of Antiquities

Excavations and restorations begun in 1937 (*AJSL*, LV, No. 4 [October, 1938], 426) on the ancient sites of Plinthiné and Taposiris, 40 kilometers west of Alexandria, were continued in 1940. The great tower, octagonal at the bottom and cylindrical at the top, has been completely restored from near ruin. The stairway in the octagon and the spiral staircase in the top are now rebuilt and show the means of access to the summit. Once thought to be a funerary monument, the tower seems clearly to have been a lighthouse. It is the only building of its kind extant in Egypt and may indicate what the great Pharos of Alexandria was like. The cemetery around it has proven on excavation to be of earlier date than the tower.

The excavation of the so-called "Temple of Osiris" was also continued. The building now appears to have been something like a convent with small cells about a central chapel. Pottery found under the floor of one cell points to a date at the earliest Hellenistic for the founding of the building.

From a Department of Antiquities release.

Alexandria. Alexandria Graeco-Roman Museum

In March, 1941, Alan Rowe discovered the richly ornamented body of a young woman in the Alexandria catacombs. The skull contained the gold tongue and eyes inserted at burial, and on the body were various rosettes, two necklaces, and fingernail coverings, all of gold. The catacombs, first discovered in 1900, date from the second century of our era.

New York Times, March 30, 1941.

Antinoë (Sheikh Abâdeh). Royal University of Florence

In May, 1940, the expedition continued soundings in the mound west of the northern cemetery, and further explored the temple of Ramses II. The soundings were in rubbish, hence no buildings were unearthed; objects of domestic use were, however, numerous. Many fragments and several small rolls of

papyrus were recovered. The late cemetery in the natural sand under the mound showed only very poor burials containing no coffins.

The excavation of the Ramses temple continued on the north and south sides and to the back, where the sanctuary must have been. The temple appears to have had a crypt added later. The entrance was in the angle of the south tower of the pylon. The crypt went down about 7 meters and consisted of a narrow passage, now below water level, which ran out in front of the temple under the court.

Some of the stone and mud-brick walls of a private house of Christian times now stand in the sanctuary area. Only here and there did the traces of walls and pavements of older buildings appear. However, these traces furnished more reused blocks bearing the cartouches of Ikhnaton (cf. *AJSL*, LVI, No. 4 [October, 1939], 423). The titles of the Aton are of the earlier type, showing that the original building was built early in Ikhnaton's reign. One door jamb has been reconstructed. The area also yielded several figures of deities and kings from Ramses II's time.

From a Department of Antiquities release.

(Edfu). *L'Institut Français d'Archéologie Orientale and University of Warsaw*

K. Michalowski et al., *Tell Edfou 1938* ("Fouilles Franco-Polonoises, Rapports," Tome II, Fasc. 2 [Le Caire, 1939]). This section is by J. Sainte Fare Garnot on the Pharaonic necropolis.

Emile Chassinat, *Le Mammisi d'Edfou* ("Mémoires ... de l'Institut Français ...," Tome XVI, Fasc. 2 [Le Caire, 1939]).

(Maasara). *Egyptian Museum in Stockholm*

In March, 1937, the Swedish expedition which had been working at Abū Ghalib opened six tombs at this site 20 kilometers south of Cairo. Five of these shaft graves had been plundered, two of them recently, but the broken pottery recovered dates them to the beginning of the Old Kingdom. The sixth, a pit grave mud-brick lined and stone covered, was intact. It furnished numerous fine examples of red and gray pottery and a few alabaster and slate bowls. These are definitively published by the excavator.

Hjalmar Larsen, "Tomb Six at Maasara: An Egyptian Second Dynasty Tomb," *Acta archaeologica*, XI, Fasc. 1-2 (1940), 103-24.

Nekheb (el-Kāb). *La Fondation Egyptologique Reine Elisabeth*

Jean Capart, "Un dépôt de fondation sous le sanctuaire," *Chronique d'Égypte*, No. 30 (July, 1940), pp. 205-10. The deposit is that of the last sanctuary of Nekhbet. The vases compare only with Achaemenid pottery found at Susa. The restorer of the sanctuary must have been a king of the Persian dynasty.

(Sakkarah). *Department of Antiquities*

Work was continued in 1939-40 in the area between the Step Pyramid and the pyramid of Unis. The clearing of the causeway of Unis resulted in the finding of blocks bearing interesting reliefs, including a famine scene. Masta-

bas have been uncovered to the south of the causeway and others near that of Idut south of the Zoser inclosure.

In the burial chamber of one mud-brick mastaba south of the causeway was found a very unusual false door of acacia measuring 2 meters high by 1.50 meters wide. The mastaba is older than the causeway, for the building of the latter resulted in its being covered up. The burial chamber was otherwise empty, and the other rooms appeared inferior in workmanship. The false door was excellently preserved and has been moved to the Cairo Museum. The deeply cut hieroglyphs and the figures in low relief present the deceased Ika, a high functionary, his wife, Imerit, and twelve other members of his family.

Close to the funerary temple of Unis the excavators came upon the mastaba of a Queen Nebet, whom they believe to have been the wife of Unis. The rooms are large and the stone is of the best quality from the Tura quarries. The whole is richly sculptured and the figures and inscriptions are on an unusually large scale.

Another complete mastaba proved to belong to a vizier Mehu who lived under the first three kings of the Sixth Dynasty. Unpublished evidence is said to prove that Mehu was the grandson of Unis. The inscriptions show that he was married to two royal ladies. He must have been a great landowner, for forty estates are represented as contributing to his offerings. The entire tomb is in perfect preservation, even to the colors on the walls. The scenes present some hitherto unfamiliar subject matter in addition to the more common. One of the false doors is a single piece of limestone 3.1 meters high by 2.05 meters wide. It is painted dark red, as are the limestone roofs of the chambers, in imitation of red granite. The incised figures and hieroglyphs on it are painted yellow to simulate gold.

ILN, September 28, 1940, pp. 412 f.; *Chronique d'Égypte*, No. 30 (July, 1940), pp. 211 f.

(Sakkarah)

Lucienne Epron, *Le Tombeau de Ti* ("Mémoires ... de l'Institut Français ...," Tome LXV, Fasc. 1 [Le Caire, 1939]).

(Semneh)

Hermann Grapow, "Die Inschrift der Königin Katimala am Tempel von Semne," *ZAS*, LXXXVI (1940), 24-41 and Pls. II, III.

Tanis (Şan el-Hagar). *La Mission Française de Tanis*

On the tombs of Kings Psusennes and Amenemope previously reported see also *JEA*, XXVI (February, 1941), 162; *Chronique d'Égypte*, No. 30 (July, 1940), pp. 212-14; *Scientific American*, January, 1941, p. 27.

Thebes (Deir el-Medīneh). *L'Institut Français d'Archéologie Orientale du Caire*

Clearance of the area north and east of the Ptolemaic inclosure wall added other small chapels dedicated by cemetery workmen to the forty or so previously found (cf. *AJSL*, LVI, No. 4 [October, 1939], 427). The most imposing to the north were one dedicated under Seti I to Amon and Hathor and another

dedicated to the deified Amenhotep I and Nefretiri. To the east was a large chapel of the reign of Ramses II. Among the chapels were found parts of the large statues which once adorned them as well as fragments of steles, offering tables, ostraca, papyri, and other objects.

JEA, XXVI (February, 1941), 162. Publications of previous work: Bernard Bruyère, *Rapport sur les fouilles de Deir el Médineh (1934-1935)*, Troisième partie: *Le Village, les décharges publiques, la station de repos du col de la vallée des rois* ("Fouilles de l'Institut Français ... du Caire," Tome XVI [Le Caire, 1939]); Jaroslav Černý, *Catalogue des ostraca hiératiques non littéraires de Deir el Médineh (Nos. 242 à 339)*, Tome IV ("Documents de fouilles de l'Institut Français ... du Caire," Tome VI [Le Caire, 1939]).

Thebes (Karnak). L'Institut Français d'Archéologie Orientale du Caire

Having relinquished the Madamūd concession, the French Institute began excavation of the temple of Montu at Karnak. Traces have been found of a temple dedicated by Amenhotep III to Amon. Under the pavement appeared blocks deriving from a square-pillared monument of Amenhotep II and others bearing geographical inscriptions from a temple of Amenhotep I. Important finds included a seated statue of Amon holding a kneeling figure of Amenhotep III in the costume of the *Sed* festival, and part of a stele of Haremhab dealing with the restoration of Theban temples. Montu does not appear until much later times, commonly on Ptolemaic blocks. Among these latter was one bearing a relief of the sacred bull of Montu.

JEA, XXVI (February, 1941), 161 f.

Thebes (Medinet Habu). Oriental Institute, University of Chicago

Uvo Hölscher, *The Excavation of Medinet Habu*, Vol. III: *The Mortuary Temple of Ramses III, Part I* ("OIP," Vol. LIV [Chicago, 1941]); Uvo Hölscher, "Gessodekorationen, Intarsien und Kachelbekleidungen in Medinet Habu," *ZAS*, LXXXVI (1940), 41-45.

Thebes (Sheikh Abd el-Qurna)

Nina M. and N. de G. Davies, "The Tomb of Amenmosé (No. 89) at Thebes," *JEA*, XXVI (February, 1941), 131-36 and Pls. XXII-XXV.

Thebes (Qurnat Murr'ai). Department of Antiquities

Under the direction of Baraize the tomb of Amenhotep III's well-known viceroy of Nubia, Mermose, was cleared. In it was discovered an anthropoid coffin on its sledge, both of granite.

JEA, XXVI (February, 1941), 162.

PALESTINE

General

Grace M. Crowfoot, "Some Censer Types from Palestine," *PEQ*, October, 1940, pp. 150-53.

Bethel (Beitin)

W. Ross questions whether Beitin is the Bethel of Jeroboam. He acknowledges that it is undoubtedly the Bethel of Jacob's dream but points out that another Bethel is mentioned in the Bible (Bethuel) and that there might be several Bethels. See *PEQ*, January, 1941, pp. 22-27.

Beth Shan (Beisan)

Alan Rowe, *The Four Canaanite Temples of Beth-shan, Part I: The Temples and Cult Objects* (Philadelphia, 1940).

The Palestine Archaeological Museum has recently purchased a small bone tablet, found on the east side of Beisan, representing a seven-branched candlestick with four other Jewish symbols. See *BJPES*, Vol. VIII, No. 1 (1940) (English summary).

Beth She'arim (Sheikh Abreiq). Jewish Palestine Exploration Society

For a résumé of the fourth season see Nelson Glueck in *AJA*, XLV, No. 1 (January-March, 1941), 116. According to Benjamin Maisler, it has now been possible to distinguish at Sheikh Abreiq four separate periods of occupation, extending from the first to the fourth centuries A.D. Also see *Palestine Review*, August 2, 1940, p. 71; *AJSL*, LVII (1940), 190, 323.

Jerusalem

R. W. Hamilton reports on three soundings in the area of the North Wall. Sounding A, at the west tower of the Damascus Gate, revealed two building periods, the second falling between the foundation of Aelia Capitolina and a probable repair of the curtain wall in or before the fourth century A.D. Sounding B, just to the east of Herod's Gate, confirmed the sequence of pottery types observed in A and also repeated the evidence for a construction in the third or early fourth century. The pottery also indicated a reconstruction of the wall in the sixth or seventh century A.D. Sounding C, just to the west of Herod's Gate, revealed two distinct lines of fortifications at this point, one represented by the present wall and its projecting tower, and the other by a rock scarp, which still carries the remains of masonry superstructures some 9 meters farther north. As at B, there is no evidence of a wall that could have belonged to any city earlier than Aelia Capitolina.

See R. W. Hamilton, "Excavations against the North Wall of Jerusalem, 1937-38," *QDAP*, X, No. 1 (1940), 1-54.

In August, 1940, a few large building stones were discovered during the construction of a new road just behind the property of the American School of Oriental Research in Jerusalem. Since these stones were located on a line directly east of the easternmost part of the Third Wall of Jerusalem which had been previously traced, the importance of the find was immediately evident, and the late C. S. Fisher and E. L. Sukenik immediately undertook a joint excavation in this area. A new stretch of city wall, 23 meters long and 4 meters wide, together with a tower about 12 meters long and 9 meters wide, was exposed. Some 140 meters farther east another tower, about 20 by 8 meters, has been discovered, thus extending the total explored length of the Agrippan Wall to nearly 800 meters.

W. F. Albright, "New Light on the Walls of Jerusalem in the New Testament Age," *BASOR*, No. 81, p. 10, and No. 83 (in press). Also Glueck in *AJA*, XLV, No. 1 (January-March, 1941), 116.

Kinnereth

Nelson Glueck, in *AJA*, XLV, No. 1 (January-March, 1941), 116, reports that in July, 1940, Benjamin Maisler, on behalf of the Jewish Palestine Exploration Society, undertook

a small excavation at Kinnereth, close to Khirbet Kerak on the Lake of Galilee. A grave was found containing seventy unbroken vessels, hundreds of beads, and ornaments of gold, bronze, and semiprecious stones. Maisler assigns the pottery to the middle of the Early Bronze Age, that is, to about 2500 B.C.

Lachish (Tell ed-Duweir). Wellcome-Marston Archaeological Research Expedition to the Near East

Lachish II (Tell ed-Duweir) The Fosse Temple, by Olga Tufnell, Charles H. Inge, and Lankester Harding (1940). D. Winton Thomas in *PEQ*, October, 1940, pp. 148-49, questions whether Ostrakon IV actually demonstrates that Tell ed-Duweir is Lachish. A series of philological notes on the epigraphic material discovered at Tell ed-Duweir and its bearing on the Bible is given by D. S. Mahbub in *Bible Documents' Notes I-IV* (London, 1939). David Diringer published some known and unknown inscriptions from Lachish in *PEQ*, April, 1941, pp. 38-56. Also see H. L. Ginsberg, "Lachish Ostraca New and Old," *BASOR*, No. 80, pp. 10-13; W. F. Albright, "The Lachish Letters after Five Years," *BASOR*, No. 82, pp. 18-24.

Lydda

BJPES, Vol. VIII, No. 1 (1940) (English summary) reports the finding, on the northwestern outskirts of the ancient site of Lydda, of a small tombstone bearing what is believed to be a Samaritan inscription.

Megiddo (Tell el-Mutesellim). Oriental Institute, University of Chicago

J. W. Crowfoot in *PEQ*, October, 1940, pp. 132-47, suggests that the stables of Stratum IV belong to Ahab and not to Solomon. He bases his argument on the similarity of the masonry to that of the Omri and Ahab palaces at Samaria and also on the chronological sequence of the strata, which seem to fit more smoothly into the historical background if the stables are put in the period of Ahab. He gives a revised chronology for Strata III, IV, and V.

Mizpah (Tell en-Nasbeh). Palestine Institute of Archaeology of the Pacific School of Religion

Joseph Carson Wampler, "Three Cistern Groups from Tell en-Nasbeh," *BASOR*, No. 82, pp. 25-43.

(Nablus)

W. R. Taylor, "A New Samaritan Inscription," *BASOR*, No. 81, pp. 1-6. This inscription, exposed after heavy rains in 1935 on the main road at Nablus, is assigned to the third-fourth century and furnishes assistance in determining the chronological relations of other early Samaritan inscriptions.

(Tell ej-Jerisheh). Hebrew University in Jerusalem

J. Ory, "A Late Bronze Age Tomb at Tell Jerisheh," *QDAP*, X, No. 1 (1940), 55-56. This is the first instance of a LB burial at this site.

TRANSJORDAN

Ezion-Geber (Tell el-Kheleifeh). American School of Oriental Research in Jerusalem

The excavation of March-May, 1940, showed further details of the smelting technique at this site. The smelter walls were probably 12 feet high, made of bricks laid in complex diagonal cross-patterns. The city itself was protected by two separate walls, with a glacis built against each of them and a dry moat between. The excavator assigns Period I to the tenth and ninth centuries and its most important phase to the time of King Solomon. Period II is assigned

to the ninth century, probably to the reconstruction by Jehoshaphat of Judah. Period III is assigned to the eighth century, when it may have been reconstructed by Uzziah. Period IV was Edomite, extending from the end of the eighth century B.C. to the end of the sixth. Not much is left of Period V, which lasted from the end of the sixth century to the fourth century B.C. Aramaic ostraca were found in this period. Glueck concludes that the cultural patterns of Ezion-Geber fit in with East Palestine rather than with West Palestine.

Nelson Glueck, "The Third Season at Tell el-Kheleifeh," *BASOR*, No. 79, pp. 2-18. Aug. Bea, in "Archäologische Beiträge zur israelitisch-jüdischen Geschichte," *Biblica*, XXI, Fasc. 4, 437-45, discusses the excavations at Ezion-geber and the question of its being the biblical site by that name. See also Glueck, "Ezion-Geber: Elah—City of Bricks with Straw," *Biblical Archaeologist*, III, No. 4 (1940), 51-55; "Ostraca from Elath," *BASOR*, No. 80, pp. 3-10, No. 82, pp. 3-11; and "Ezion-Geber: 'Singapore' of Solomon," *Asia*, December, 1940, pp. 663-69; W. F. Albright, "Ostrakon No. 6043 from Ezion-geber," *BASOR*, No. 82, pp. 11-15; Charles C. Torrey, "On the Ostraca from Elath," *BASOR*, No. 82, pp. 15-16.

Petra

M. A. Murray and J. C. Ellis, *A Street in Petra* (London, 1940): an account of the excavation of several caves.

(Teleilat Ghassul). Pontifical Biblical Institute

R. Koepfel, *Teleilat Ghassul II: Comptes rendus des fouilles de l'Institut Biblique Pontifical 1931-1936* (Rome, 1940).

SYRIA

General

Jean Sauvaget, "Caravansérails syriens du moyen-âge," *Ars Islamica*, VII, Part I (1940), 1-19: Arabic inscriptions from a number of Mameluke caravansaries throughout Palestine and Syria.

Antioch. Princeton University, Baltimore Museum of Art, Worcester Art Museum, and Musées Nationaux de France

W. A. Campbell, "The Sixth Season of Excavations at Antioch-on-the-Orontes: 1937," *AJA*, XLIV (1940), 417-27: an extract concerning the development of the main street of Antioch, followed by a description of several mosaics. Also see Christine Alexander, "A Mosaic from Antioch," *BMAA*, XXXV, No. 12 (1940), 244-47.

Dura-Europus (Sālihīyah). Yale University and Académie des Inscriptions et Belles-Lettres

Clark Hopkins, "The Architectural Background in the Paintings at Dura-Europus," *AJA*, XLV, No. 1 (January-March, 1941), 18-29, compares the background of the Dura-Europus paintings with Roman and Hellenistic patterns and concludes that the evidence makes it probable that the Hellenistic Greek motives were the dominant element.

Hamath (Hama). Ny Carlsberg Foundation of Denmark

H. Ingholt, *Rapport préliminaire sur sept campagnes de fouilles à Hama en Syrie (1932-1938)* (Copenhagen, 1940); see also *Asia*, April, 1941, pp. 199-204.

(Jebel Kosseir)

C. W. McEwan, "Modelled Pot-Fragments from Jebel Kosseir, Syria," *Man*, XL (November, 1940), 167-69 (No. 193). Also see M. E. L. Mallowan, "Note on Modelled Pot-Fragments from Jebel Kosseir," *Man*, XL (1940), 169 (No. 194); Mallowan believes the scene represents animals led to sacrifice, as on the royal standard at Ur. He dates the fragments about 2500-2000 B.C.

Lebanon

Nelson Glueck in *AJA*, XLV, No. 1 (January-March, 1941), 117, reports that an expedition headed by Rev. Joseph Doherty, S.J., has completed the excavation of a prehistoric site in the Valley of Antelias in Lebanon. A petrified skeleton and the well-preserved skull of a child seven or eight years old, together with Stone Age tools and weapons, were found.

Ras-el-Ain. Marriner Memorial Syrian Expedition: Oriental Institute and Boston Museum of Fine Arts

C. W. McEwan, field director, will briefly describe the 1940 season at Tell Fakhariyya in the forthcoming number of this *Journal*.

(Sakçe Gözü)

Elaine Tankard, "The Sculptures of Sakçegeuzi," *AAA*, XXVI, Nos. 3-4 (December, 1939 [published July, 1940]), 85-88. A reinterpretation of the sculptures published in *AAA*, XXI (1934), 37 ff. The writer points out that the artist probably did not carve an oblique view of the face of the king but carved in the usual manner, working in two planes which are at right angles to each other.

(Tell Atchana). British Museum

Sidney Smith, "Timber and Brick or Masonry Construction," *PEQ*, January, 1941, pp. 5-10.

Ugarit (Ras Shamra). Académie des Inscriptions et Belles-Lettres

C. F. A. Schaeffer, *The Cuneiform Texts of Ras-Shamra—Ugarit* ("Schweich Lectures" [1939]); also *Cylindres de Ras Shamra—Ugarit* ("Première série" (Paris, 1940)). Schaeffer gives a description of the defense works and war machines of ancient Ugarit, based on the excavations, in *ILN*, June 14, 1941, pp. 778-80. Also see W. F. Albright, "Two Letters from Ugarit (Ras Shamra)," *BASOR*, No. 82, pp. 43-49.

CYPRUS

G. Ernest Wright, "The Syro-Palestinian Jar from Vounous, Cyprus," *PEQ*, October 1940, pp. 154-57. Wright dates the jar between 2700 and 2500 B.C. For earlier report see *PEQ*, July, 1939, pp. 162-68, where J. R. Stewart originally published the vase.

James A. Stewart, "Three Jugs of the Cypriote Iron Age in the Biblical Museum, Melbourne," *Man*, XL (October, 1940), 145 and 169 and Pl. K, dates this type of Cypriote pottery to Cypro-Archaic I, in the eighth-seventh centuries B.C. Also see John L. Myres, in *Man*, XL (November, 1940), 169-70.

John Franklin Daniel, "Prolegomena to the Cypro-Minoan Script," *AJA*, XLV, No. 2 (1941), 249-82.

TURKEY

General

Kurt Bittel has brief reports on recent small finds of Hittite materials and some unpublished museum objects in *Arch. Anz.*, LV (1940), 575-81, and also a summary of recently found monuments from the Greek and Roman periods (included are a relief of

Jupiter Dolichenus and a damaged stele depicting a warrior in the "late Hittite" style), in *ibid.*, pp. 584-88. Georg Rohde reports on another Jupiter Dolichenus stele dated to the second or third century A.D. in *ibid.*, pp. 596-99. Also see Kurt Bittel, "Der Depotfund von Soloi-Pompeopolis," *ZA*, XLVI (1940), 183-205; Albrecht Goetze, *Kizzuwatna and the Problem of Hittite Geography* ("Yale Oriental Series," *Researches*, Vol. XXII [New Haven, 1940]); P. Jacobsthal and A. H. M. Jones, "A Silver Find from South-west Asia Minor," *Journal of Roman Studies*, XXX, Part I (1940), 16-31.

(Alaca Hüyük). Turkish Historical Society

A beautifully illustrated report on the work carried on at this site has been made by Hamit Zübeyr Koşay. During 1939 the expedition concerned itself with further clearing in the Hittite temple area and with work in the lower levels of the Copper Age. The first cuneiform tablet found at the site was recovered in 1939. Two new tombs are reported from the Copper Age levels. A fine long dagger with a golden hilt was found in this area. Two fine statuettes with inset eyes are reported: one of copper and one of silver and gold. The small objects include rich decorative materials which seem to be related to Mesopotamian types but also have parallels in the Kurgans of South Russia.

La Turquie Kemaliste, Nos. 32-40 (August, 1939-December, 1940), pp. 20-26; *Arch. Anz.*, LV, 556 f.

Ankara

A brief survey of recent archeological work in and about Ankara by Hamit Zübeyr Koşay, "The Strata of Civilizations in Ankara," *La Turquie Kemaliste*, No. 31 (June, 1939), pp. 13-16.

(Bozkır)

At this site in a cave in the midst of the Taurus region Gaffar Totaysalgar discovered most unusual rock reliefs. One scene portrays a horseman with a long spear over a prostrate warrior. Another relief shows six rows of animals, apparently ibexes, and below them a dog. The style is reminiscent of African rock reliefs. There is no comparative material in Asia Minor, and no date may be assigned.

Kurt Bittel, *Arch. Anz.*, LV, 559.

Byzantium (Istanbul). Byzantine Institute

The Byzantine Institute carried on its work as usual from May to the end of December, 1940, in Hagia Sophia in Istanbul. Two life-sized figures were found, uncovered, conserved, and partly cleaned on the walkway of the north Tympanum wall. One of these figures represents Ignatius of Antioch and the other St. John Chrysostom. In the meantime the figures of the Mother of God and the Child in the eastern apse were opened to the public.

Excerpt from letter by Thomas Whittemore, July 28, 1941. See also illustrated article by Sami Boyar, "Aya Sophia," *La Turquie Kemaliste*, No. 41 (February, 1941), pp. 13-21; and A. M. Schneider, *Arch. Anz.*, LV, 589-91.

(Cağdır)

At Cağdır near the Sacur River a relief has been reported. The representation is presumably that of a god with pointed cap and sword and spear. Four damaged Hittite hieroglyphic signs appear above the figure, which is dated to the eleventh or tenth century B.C.

Arch. Anz., LV, 566 f.

(Çakalsuyu)

In the Çakalsuyu Valley in Malatya, Şevket Aziz Kansu reports the discovery of a Mousterian type of stone implement.

Arch. Anz., LV, 556.

(Ergazi)

At this site, a few miles southwest of Ankara, E. Pittard and Miss Afet report the discovery of about twenty stone implements of a Levallois-Mousterian or a Levallois type.

Arch. Anz., LV, 555 f.

(Göllüdağ)

The excavation carried out at this site under the direction of Remzi Oğuz Arık in 1934 was briefly reported in *AJSL*, LII (1936), 136, and LV (1938), 438. Now, for a concise review and discussion with photographs, see Kurt Bittel, *Arch. Anz.*, LV, 567-75.

(Hanyeri)

About half a mile north of the village of Hanyeri and the border of the Adana and Kayseri provinces, a Hittite rock sculpture depicts a large male figure carrying a bow and a staff or spear with a sword in scabbard at the waist. Accompanying the large figure are Hittite hieroglyphs and smaller pictorial representations. The monument is dated to the fourteenth to thirteenth centuries B.C.

Arch. Anz., LV, 560-64. Now published by Ali Rıza Yalgin in *Türk Tarih, Arkeologiya ve Etnografya Dergisi*, Vol. IV (1940).

(Maraş)

This site of many late Hittite monuments has yielded another, a stele about forty inches high. The stele probably represents a god and goddess whose arms are about each other in an unusual position. It is to be dated probably not earlier than the eighth century B.C.

Arch. Anz., LV, 564-66.

(Mersin). Neilson Expedition of the University of Liverpool

See previous *AJSL* reports and John Garstang, "Excavations at Mersin, 1938-39," and Miles Burkitt, "The Earlier Cultures at Mersin," *AAA*, XXVI (1939/40), 38-72.

(Pancarlı)

A damaged orthostat was found here which depicts a man holding a lion in his left hand and a double ax in his right. Style and clothing date this monument to the beginning of the first millennium B.C.

Arch. Anz., LV, 566.

Tarsus (Gözlü Kule). Bryn Mawr College, Archaeological Institute of America, and Fogg Museum of Harvard University

Hetty Goldman, "The Sandon Monument of Tarsus," *JAOS*, LX (1940), 544-53; Florence E. Day, "The Islamic Finds at Tarsus," *Asia*, March, 1941, pp. 143-48.

Thrace

Report by Arif Müfid Mansel on excavations carried on by Turkish expeditions during 1936 and 1939 in Thrace, *Türk Tarih Kurumu Belleten*, IV, Part XIII (1940), 115-39.

(Topkapu Sarayı)

Report by Aziz Ogan on excavations carried out in 1937, *Türk Tarih Kurumu Belleten* IV, Part XVI (1940), 329-36.

IRAQ

General

Fr. W. von Bissing, "Ägyptische und ägyptisierende Alabastergefäße aus den deutschen Ausgrabungen in Assur," *ZA*, XLVI (1940), 149-82; Th. A. Busink, *Sumerische en Babylonische tempelbouw* (Batavia-Centrum, 1940); L. Legrain, "Nippur Again. Sumerian Heads: Archaic Engraved Stone Plaque," *UMB*, IX (1941), 9-14; Heinz Lenzen, "Die Zikurrat in Ur," *ZA*, XLVI (1940), 116-48; E. A. Speiser, "The Beginnings of Civilization in Mesopotamia," *Antiquity*, XV (1941), 162-75; R. F. S. Starr, "A Rare Example of Akkadian Sculpture," *AJA*, XLV (1941), 81-86.

Eshnunna (Tell Asmar)

Max Hilzheimer, *Animal Remains from Tell Asmar*, translated by Adolph Brux ("OIC," No. 20 [Chicago, 1941]).

Nuzi

G. R. Driver and Sir John C. Miles, "Ordeal by Oath at Nuzi," *Iraq*, VII (1940), 132-38; E. R. Lacheman, "Nuzi Geographical Names. II," *BASOR*, No. 81 (February, 1941), pp. 10-15; "Nuziana," *RA*, XXXVI (1939), 81-95; and *Miscellaneous Texts* (Joint Expedition with the Iraq Museum at Nuzi, American Schools of Oriental Research, "Publications of the Baghdad School," *Texts*, Vol. VI [New Haven, 1939]).

IRAN

General

Henry Field, *Contributions to the Anthropology of Iran* (Field Museum of Natural History, "Anthropological Series," Vol. XXIX, Nos. 1-2 [Chicago, 1939]); *International Congress of Persian Art and Archaeology*, 3d, Leningrad, 1935 (*III^e Congrès international d'art et d'archéologie iranienne, Mémoires, Leningrad, septembre 1935* [Moscow, 1939]); Arthur Upham Pope and Phyllis Ackerman, "New Discoveries in Iran Exemplify the Superb Art of Persian Goldsmiths over a Period of Seventeen Centuries," *ILN*, May 31, 1941, pp. 718 f.; E. F. Schmidt, *Flights over Ancient Cities of Iran* (special publication of the Oriental Institute of the University of Chicago [Chicago, 1940]); Sir Mark Aurel Stein, *Old Routes of Western Iran* (London, 1940).

Luristan

Arthur Upham Pope and Phyllis Ackerman, "Prehistoric Nature Worship in Western Iran: Bronzes from Kuh-i-dasht," *ILN*, March 1, 1941, pp. 292 f.

Persepolis (Takht-i-Jamshid)

A fragment of a limestone relief from Persepolis is published in *UMB*, IX (1941), 28.

Shāpūr. Loure and Ministère de l'Éducation Nationale

R. Ghirshman and Mme Ghirshman continued working at this site during the winter of 1939/40 and returned for another campaign during the winter of 1940/41. Early in 1941 it was reported that they had discovered several meters of mosaics with portrait heads on panels and dancing figures and girls with musical instruments on other panels. These are the first Sassanian mosaics discovered. No further details are available.

AJA, XLV (1941), 165, and private correspondence from R. Ghirshman.

(Yazd-i-Khwast)

Myron B. Smith, "Three Monuments at Yazd-i-Khwast," *Ars Islamica*, VII (1940), 104 f.

INDIA

General

Bedřich Hrozný, *Über die älteste Völkerwanderung und über das Problem der proto-indischen Zivilization* (Československý orientální ústav v Praze, "Monografie archivu orientálního," Vol. VII [Prag, 1939]); M. S. Vats, *Excavations at Harappa between the Years 1920-21 and 1933-34* (2 vols.; Calcutta, 1940).

U.S.S.R.

General

Considerable information on recent widespread archeological activity throughout the Soviet Union is contained in the following summaries and reports: Henry Field and Eugene Prostov, "Archaeology in the Soviet Union," *Antiquity*, XIV (1940), 404-26; "Archaeology in Uzbekistan," *Antiquity*, XV (1941), 194-96; "Excavations in Uzbekistan," *Asia*, May, 1941, pp. 242-44; *AJA*, XLIV (1940), 535 f.; *AJA*, XLV (1941), 112-15, 299-301.

BOOK REVIEWS

SMITH, J. M. POWIS. *The Prophets and Their Times*. Revised edition by WILLIAM A. IRWIN. Chicago: University of Chicago Press, 1941. Pp. xvii+342. \$2.50.

It is assuredly a tribute to the late J. M. Powis Smith that after sixteen years his work on the Hebrew prophets is still in such demand that the publishers have issued this revised edition. The choice of reviser naturally fell on his former student and successor, W. A. Irwin. In a book whose subject matter is more theoretical than factual it was a delicate task that Irwin had to perform, but one that he has done exceptionally well. It would seem to the reviewer, however, that he should have completely revised the biblical quotations himself, or else he should have incorporated Smith's own revisions as found in *The Bible: An American Translation* (1931 ed.), particularly in view of the fact that he has revised the notes to bring them up to date with modern scholarship. For example, in Zech. 13:6 we know now from the Ras Shamra texts that *bēn yādhakhā* means "on your back," not "on your hands," and the last clause in Amos 7:12 should be interpreted as adverbial: "O seer, go away; off with you to the land of Judah and there eat bread [i.e., earn your living] by prophesying there." One wonders, too, why the new quotation on page 80 (Hos. 11:1-9) should be set up as prose when it is clearly poetry. Also, the quotations on page 4 should have "you" in place of "thou" to make them consistent with the rest of the volume. This is a slip carried over from the first edition.

A few other random observations may be noted. On page 13, note 39, Irwin should have included the discussion of prophecy favorable to his point of view by Porteous in *Record and Revelation*, edited by H. W. Robinson (1938). One is a bit shocked to have Torezyner set down as the authority on the Lachish letters (p. 186, n. 37) and to have Jack, Dussaud, and Schaeffer cited as the authorities on the Ras Shamra texts (p. 234, n. 31), when their views need to be corrected by those of their critics (see, e.g., Albright, *BASOR*, No. 82, pp. 18 ff.; No. 71, pp. 35 ff.; Bea, *Biblica*, XIX, 435 ff.; XX, 436 ff.). It would seem useless to cite a book like Harper's *Assyrian and Babylonian Literature* (1901), because it is so obsolete and has long been out of print. To our discredit there are no really good translations of cuneiform literature in English. Luckenbill's is the best translation of the Taylor prism of Sennacherib that we have, as noted on page 109, note 12, but it is far from perfect. On page 138 one wonders why Irwin did not follow his own emendation of Jer. 1:13 as presented in *AJSL*, XLVII, 288 f., or, better still, an emendation

(*ûphânâ(y)w* for *wnôphhō*) based on a suggestion of Ehrlich (see the reviewer, *JQR*, XIV, 285, n. 23). The final *hē* which is queried on page 138, note 13, is simply the terminative *hē*, as explained by the reviewer in *JAOS*, LX, 230. On page 219, note 2, Olmstead should have been given the credit for being the first to attach Isaiah, chapter 35, to Second Isaiah (see his note, *AJSL*, LIII, 251). Only one typographical error was noted: "Shemiaiah" (p. 35, n. 7).

Irwin has set himself against Smith's view that the canonical prophets were in any way ecstatics, but on page 12 he retains Smith's statement that there was "no sharp break between the early prophets and the great prophets of later times." As a matter of fact, Irwin sees a great difference between the two, apparently something like the change in Mohammed after establishing himself in Medina. Any ecstasies supposedly experienced by the later prophets are to be interpreted as nothing more than a literary device. But Irwin labors under the delusion that the ecstatic experience of God (which incidentally need not express itself in trances) makes a man less rational. This was assuredly not the case with Mohammed, whose earlier utterances, based on actual ecstatic experiences, are much superior to those of the later period, when he followed a literary device which earlier experience had shown him to be effective. The professional prophets can be accused of following a literary device, and perhaps a prophet like Ezekiel, but scarcely the prophets of the eighth century. In his *Hebrew Origins* (1936) the reviewer has devoted a whole chapter to Hebrew prophecy, and he would have been glad if Irwin had given consideration to the thesis there propounded that the early prophets represent a protest against the professionalization of the priestly office, while the later prophets grew out of a similar protest against the professionalization of the prophetic office. On Irwin's theory it is difficult to explain the fact that the utterances of the prophets, like those of Mohammed in his earlier career, were short, direct, authoritative, and only slightly argumentative—not long-drawn-out, reasoned, and logically arranged sermons, but oracles, full of conviction as coming from Yahweh and surcharged with the white heat of emotion. As Amos put it (3:8), "The Lord Yahweh has spoken; who will not prophesy?"

The most original part of the book is the chapter on Ezekiel. Here nothing remains of Smith's treatment except one citation. This is revision with a vengeance, but it is symptomatic of the tremendous change in critical opinions since the days of Smith. As Irwin well says (p. ix), the criticism of Ezekiel stands at present in complete chaos, the only agreement being that the book is composite and that Ezekiel began his ministry in Palestine. Under these circumstances Irwin is perfectly justified in presenting his own interpretation of the book, but its complete exposition and vindication are reserved for a later, more appropriate, publication. In the present volume he could only present his conclusions in the baldest outline. The Book of Ezekiel, according to him, was not the father of Judaism, as it was with Smith, but the child of Judaism (p. 216). The dates in the book are of doubtful genuineness (p. 208), and he

rejects most of them. The genuine oracles of Ezekiel are in the main poetic (p. 212). Not only are whole chapters spurious (e.g., 40-48), but whole sections throughout the book, some of which (e.g., 11:17-20; 20:33-38, 40-42; 34:13-16; 36:24-38) come from the hand of a gentle and pious Jew of the Greek period or later (p. 204), while others (e.g., 4:13; 5:10, 12; 12:15-16; 20:34-38,¹ 41; 22:15) originate with a still later commentator whose mood was stern and denunciatory (p. 205). In fact, the book is interspersed with the comments of numerous writers, and Irwin professes to be able to separate the wheat from the chaff. Judgment on the thesis must await his more elaborate presentation, but in the meantime it is attractive enough to whet our appetite for its fuller elucidation.

To a less degree the interpretation of Hosea has been re-written. Irwin argues that the Gomer of chapter 1 was a religious prostitute belonging to the fertility cult and that the adulteress of chapter 3 was a different woman, the two chapters being records of two distinct incidents in the prophet's career (p. 74). There were two marriages because Hosea had two different object-lessons in mind. We heartily commend Irwin's treatment as representing a decided advance over that of Smith.

The rest of the volume follows more nearly the lines laid down by Smith. The discussion is brought up to date and all the more important later views are taken into account, to make a volume seventy-four pages longer than its predecessor. We predict for the book a reception as cordial as that accorded the first edition and many years of usefulness as an authoritative guide to the prophets and their times.

THEOPHILE J. MEEK

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VAN BUREN, DOUGLAS E. *The Cylinder Seals of the Pontifical Biblical Institute*. ("Analecta orientalia," No. 21.) Rome, 1940. Pp. xii+51+12 pls. Rm. 14.

The Pontifical Biblical Institute bought this collection of cylinder seals for the use of students. Its variety and comprehensiveness, consequently, were more important than the quality or special features of individual pieces; but the seals, though of the usual types, warrant publication. The author brings her wonted enthusiasm to the task, and the text of the catalogue shows a praiseworthy attempt to counteract, by lively prose, the dullness inherent in a string of unconnected descriptions. It is probably a matter of taste whether one prefers this method or its opposite, namely, the reduction of the descriptive text to the absolute minimum in which nothing is insisted upon which the picture shows clearly. After a discussion of eighty-seven genuine pieces, the author pictures and describes about twenty which she considers forgeries. It

¹ There must be some slip here because vss. 33-38 of this chapter have just been assigned to quite a different hand.

is disquieting to note how many can be traced back to prototypes in well-known collections—some of which may have been imitated when still in dealers' hands, while others seem inspired by some such scholarly work as that under review. At any rate, the treatment of these forgeries is highly instructive and embodies much painstaking and conscientious work.

The reviewer is not quite convinced of the genuineness of No. 7, though no suspicion can take definite shape on the basis of an illustration only. The seated figure on this seal excludes in any case the Early Dynastic period to which it is assigned; if genuine, the seal belongs to the First Babylonian Dynasty; the drillholes point in the same direction,¹ but the figure holding the gateposts combines the head of the "nude hero" with the legs of the bull-man—a confusion of motives hardly to be expected from an ancient seal-cutter.

The outstanding piece in the collection is No. 14. It shows the killing of the "Bull of Heaven," symbol of drought; and, furthermore, the dragons of the weather-god which stand for the storms with their thunder, lightning, and life-giving rain.² This interpretation is quite certain if one considers our seal in connection with other renderings of the subject, but the author unfortunately has not recognized its true meaning. She interprets the bull as a sow, perhaps because the hunting dog³ which assists the slaying god is considered by her as a piglet. This small figure certainly lacks clarity; but the victim of the god is not a boar or a sow, as comparison with indubitable renderings⁴ will show. Moreover, our author is compelled by her interpretation of the animal to see in the bull's horn "a curved implement" held by the god. But the only implements depicted on Akkadian seals are weapons or identifying attributes. Bullfighters on Akkadian seals often grasp the horn of the beast, and, in fact, exactly this feature occurs in the Hermitage rendering of our subject.⁵ It is true that the neck of the bull on the seal in Rome shows a curious "ruff" which may be due to a chipping of the seal or to careless tooling in the engraving of the neck. But the tail as well as the comparisons noted above exclude the interpretation as a sow.

Some details in the scene remain inexplicable, notably the role played by the two figures placed among the dragons. One of these men or gods seems to release a dragon for its headlong flight toward the earth; the oblique placing of the dragons on our seal and on the Hermitage example may actually be due to their symbolizing the lightning and slashing rains of the storm. In a seal from the Pierpont Morgan Library⁶ the dragon pulling the god's thunder chariot spits fire. But, in any case, whatever may be the exact interpretation

¹ Frankfort, *Cylinder Seals*, Pl. XXVI*d*, *f*, and p. 148.

² *Ibid.*, pp. 124-27.

³ Or possibly lion, depicted on the small scale of an attribute.

⁴ Frankfort, *op. cit.*, Pl. XXIII*h*, *i*.

⁵ Ward, *Cylinder Seals of Western Asia*, No. 129*a*.

⁶ Frankfort, *op. cit.*, Pl. XXII*a*.

of certain details, the meaning of the scene as a whole is well established not only because this type of dragon is, in Akkadian times, the exclusive attribute of the weathergod and his spouse but also because the subject is very explicitly depicted on a seal in the British Museum where the death of the bull is shown to coincide with the breaking of the storm.⁷ There is no reason to claim that the scene takes place in "wild mountainous country"; its significance lies precisely in the value of rain for the peasants in the Plain of the Two Rivers, and the presumed "rocks" seem due to accidental chipping of the edge of the cylinder. Mountains, in any case, are rendered in Akkadian times by a regular scale pattern.

The errors into which so experienced a student as Mrs. Van Buren has fallen in this case emphasize once more the impossibility of interpreting any ancient seal in isolation. The first question must always be to which group of representations a given design belongs; next it must be determined which meaning the combined testimony of all versions suggests for the underlying myths or stories; and only after that may it be attempted to interpret any given variant of the subject.

The other seals in the collection do not call for comment. It remains to state that the author and the authorities of the Pontifical Biblical Institute have placed us under a real obligation by the publication of this catalogue.

H. FRANKFORT

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STARR, RICHARD F. S. *Indus Valley Painted Pottery*. ("Princeton Oriental Texts," Vol. VIII.) Princeton: Princeton University Press, 1941. Pp. xiii+106+1 map. \$3.50.

In this study Dr. Starr has undertaken the worth-while task of determining the status of the painted pottery of the Harappa culture in relation to the "great painted pottery family of Western Asia" (p. 6)¹ and thereby elucidating the relationship of the Indus Valley to other portions of the ancient world.

The reviewer is in considerable disagreement with the method used by Starr and with his conclusions. His method of comparing designs does not seem to lead to the most certain results possible; the assumption of a fairly close relationship between the Harappa, Amri, and Baluchistan cultures, which is the only justification for many of his East-West comparisons, does not seem justified; and particularly the absence of a chronological treatment of the Baluchistan and Indus Valley remains appears inevitably to vitiate Starr's conclusions. In fairness to him, however, his method and conclusions

¹ *Ibid.*, Pl. XXII*e*.

² The page references are to the book under review. The abbreviations used for other references are those employed by Starr.

are presented in the following three paragraphs before they are critically examined.

In Part I the various types of pottery to be utilized are described and discussed. These include, in the West, the early Mesopotamian and Iranian painted pottery; in the East, painted pottery from Baluchistan and the Indus Valley. Some indications of a Baluchistan sequence of cultures are given, but the relative order of the Baluchistan remains is not considered certain. Nevertheless, owing to an "obvious interrelationship" of Baluchistan and Harappan pottery, "various Baluchi specimens of painted design" of the types at Kulli, Mehi and Periano-ghundai are used in comparison with the West "not as substitutes for Harappan examples, or as their equivalent, but as representatives of the broad eastern family of which Harappa is a member" (p. 20). The Amri culture in the Indus Valley is recognized to be the direct predecessor of the Harappa culture. Because of the similarity in design between the pottery of the Amri and Harappa cultures, the former is used in the "comparison between Harappa and the west more frequently than any other of the Indus and Baluchistan fabrics" (p. 23). Starr recognizes the great time interval between the pottery of the West and Harappa. He infers, however, that, because of the static character of Harappan pottery, we may consider that Harappan "pottery decoration was a tradition long-fixed and faithfully retained, thus carrying our Harappan products back close in time to the prehistoric Iranian and Mesopotamian wares" (p. 11).

In Part II Starr proceeds to compare pottery designs of the Indus Valley and Baluchistan with the early painted pottery of Iran and Mesopotamia. Consideration of the pottery fabric and forms and a chronological treatment are excluded, and the comparisons are mainly of design elements and not design composition or style. In general, the method followed is to discover the possibly natural, i.e., representational, prototype of a design, or its basic geometric form if a representational origin cannot be found. Once the basic form of a design is recognized, variant types are distinguished which are then used in the eastern and western comparisons. The variant forms of the same basic motive may appear quite dissimilar but are compared because they are derived from the same original geometric or representational element.

From a study of twelve basic elements or patterns and their variants, as well as plant and animal designs, Starr concludes that "except for the plant motives, a few of the animals, and a queer" element, "there is not a single decorative element, not one pattern or motif, that does not have a correspondent among the earlier cultures of the west" (p. 87). Despite the western elements used, the style, or the impression which the design as a whole gives, is recognized to be individual and Harappan. Starr feels certain that the elements shared by Harappa with the West are an inheritance from the older cultures of Iran, Elam, and Mesopotamia and doubts if this similarity is due to borrowing alone. He reaches this conclusion because there is "no closely comparable ware from India or Baluchistan that is clearly and demonstrably

older than Harappan" (p. 87);² because the heaviness of the Harappan design and its standardization point to a conscientious retention of a much earlier decorative style; and because the marked difference in the content of the Harappan pottery design (which is in a tradition essentially foreign to the Indus) and that of other Harappan art indicates different traditions for the ceramic and nonceramic art. These facts are explained by the presence of two racial elements in the Harappan civilization. One derived from the West and insured the survival of the peculiar technique of pottery decoration; the other, responsible for the nonceramic art, was "native to the Indus," though not necessarily "autochthonous in India." By the time these people are known from the earliest discovered Harappan remains, they were amalgamated into a homogeneous stock. The presence of the western tradition is the result of inheritance, and the Harappa civilization is not the direct descendant of any western culture, for Starr would postulate a mixed and remote cultural and racial connection. The western element in the Harappan pottery design is not equated exclusively with any one western culture, though it is related to them collectively as a single cultural family. Starr finds it likely, however, that "an appreciable number of people brought up in the artistic tradition of the Halaf culture went into the make-up of the mixed race which was to evolve as Harappa" (p. 99).

Within the limitations with which his study is circumscribed, Starr has used all the material available. He shows a truly inquiring attitude and is never dogmatic in regard to the problems involved. The reviewer is in agreement with his view that there is a considerable Iranian tradition in the ceramic design of the Baluchistan and Amri cultures and, to a lesser extent, in the Harappa culture.

Starr's study would be more convincing if he had not limited himself to pottery design. Ceramic design is very valuable for comparative purposes, but conclusions as to the relationship of cultures are always more convincing the larger the number of traits shared by the civilizations which are to be compared. On the basis of a nonchronological comparison of ceramic design it seems to me that Dr. Starr has exceeded his evidence when he assumes that the western element became a part of Harappan culture before the latter was settled in the Indus Valley. In doing this, he is drawing conclusions of chronological significance from a nonchronological study of his material, while supporting them with an interpretation of a purely stylistic fact, that is, that Harappan design is static. An exhaustive study is needed of all the traits of the Harappan culture in relation to the cultures of Baluchistan, Amri, and the earlier Iranian cultures, before such far-reaching conclusions can be accepted.

Evidence for distinct elements in the Baluchistan and Indus Valley cultures might have been found if the author had been less skeptical of the value

² I am not quite certain what this means, for the Amri culture is admitted to be the predecessor of the Harappan. Apparently, this refers to an earlier stage of the Harappan culture when it had not yet settled in the Indus Valley.

of pottery fabric as a good criterion for comparison (pp. 7, 22, 90-91). A few words need to be devoted to this question, for it is unlikely that the use of a red slip "merely denotes a regional or group preference shared at random by east and west alike." Painted red ware, slipped or plain-surfaced, does not appear in Mesopotamia before the Uruk period. In Iran the situation is somewhat more complex. Painted red and buff wares do occur together in Siyalk I, but there is reason to suspect that they represent distinct cultures. Painted red ware alone occurs in Siyalk II. In Siyalk III and Hissar I, red ware is gradually replaced by buff ware as a result of influence from the buff-ware culture (Giyān V, Susa I, Bakun A).³ In all these cases the types of design used correspond to the ware on which they were applied.⁴ I believe that a certain degree of relationship between design and ware can be shown for the Baluchistan and Indus Valley pottery. The Amri and Nal cultures, which have striking design associations with Iran, seem to attempt to achieve in the main a light-surfaced ware; the culture of Mehi and Kulli and that of Harappa prefer red burnished surfaces and seem basically non-Iranian. At any rate, in every case in which a culture or cultures use pottery of different wares, a detailed study should be made to see if there is any possibility of correspondence between designs and wares.

In Part II Starr's comparisons of western and eastern designs seem to be confused in part by the attempt made to discover the original geometrical or representational prototypes. Subjective considerations certainly enter into the delicate matter of comparing variant forms of a design, with the result that such evidence is of secondary value. Many of the comparisons made in this book are of this sort and are therefore of doubtful validity. As long as a geometrical motif can be traced back to a representational original, it is the more probable that the same geometric form could arise independently in various areas. The more peculiar and complex the elements compared, whatever their origin, the surer we can be of a connection and the less likelihood there is of separate invention.

A number of designs compared with the West are not found in the Harappa culture but only in the cultures of Baluchistan and Amri. These are of dubious value, therefore, as regards the direct relationship of Western and Harappan design. Starr uses such comparisons because he believes that the painted pottery of the cultures of Baluchistan and the Indus Valley all belong to an eastern painted-pottery culture. This seems an unjustifiable assumption. Before we can consider the ceramic design of the Baluchistan and Amri cultures as basically Harappan (pp. 20, 21), their chronological relationships must be es-

³ The conclusions which are expressed here as to internal Iranian and Iranian-Mesopotamian relationships are based on a study of the relative stratigraphy of early Iran which will soon appear as *SAOC*, No. 23.

⁴ In Siyalk III and Hissar I the same designs are used on red and buff wares during the transitional phase only.

tablished and a detailed study of likenesses and differences be made. Had this been done, certain valuable material could have been utilized which would have inevitably modified Starr's conclusions. The position of the Nal culture is of real importance, for in it are found certain designs which are very close to the prehistoric designs of Fars in Iran. Fortunately, from two of Majumdar's excavations in Sind—at Pandi Wahi and Ghazi Shah—there is clear stratigraphic evidence that the Nal culture is older than the Harappan.⁵ Once we appreciate this fact, the explanation of Iranian elements in Harappan design becomes much clearer. There is no need to postulate contact between originally Iranian cultures and the Harappan civilization much before the beginning of the Early Dynastic period, when the latter was established in the Indus Valley. The Nal and Amri cultures contained an Iranian element from which this element in Harappan derived. This in no sense implies that the Nal or Amri cultures were ancestral to the Harappan. It merely indicates that during the period of transition between these cultures certain design elements and patterns were taken over by the Harappan ceramic industry to be rendered in its own peculiar style.

We thus see that—in contrast to Starr's view—there are earlier cultures than the Harappan in Baluchistan and the Indus Valley. Furthermore, there is no necessity to assume that the static character of Harappan design means it derives from a very old tradition. It seems quite as probable, or more so, to postulate an earlier stage of the Harappa culture than is known in the Indus Valley, in which red and gray pottery was unpainted. When the Harappa culture settled in the Indus Valley, it was in contact with the Amri and probably Nal cultures, from which was borrowed the idea of painting pottery as well as some elements of design. The native plants and animals were also drawn, and the whole was given a distinct style. This also explains the rarity of painted pottery in the Harappa culture.

A few particular points may be mentioned. The sherd shown in Figure 89 was found with Harappan pottery, but—as pointed out in our discussion of the Ghazi Shah stratigraphy—this same design is found at -39' with Amri pottery only. At Ghazi Shah, therefore, it continued in use at the beginning of the Harappa period. Originally this particular design derived from the Nal culture. Figures 163 and 165 are considered to be Harappan; actually they

⁵ At Pandi Wahi (*Sind*, pp. 109-14) in Trench I below +1', unmixed Amri pottery was found. At +2' a typical Nal design occurred (*ibid.*, Pl. XXVIII:8), and at +3.2' more Nal sherds appeared (*ibid.*, Pl. XXVIII:18, 22). The first Harappan pottery is shown from +8-10'. At Ghazi Shah, Amri pottery was found below -35', while the first Harappan design is shown from -32.3'. At -39.2' a Nal design was found (*ibid.*, p. 99, G.S. 253-54; cf. *Gedrosia*, Pl. XXIII:Nal. 9, 10). At Tando Rahim Khan (*Sind*, pp. 86, 103-5) two Nal sherds (*ibid.*, Pl. XXX:26, 40, 42) are shown with Amri pottery, and no Harappan pottery was found. More space cannot be devoted to the study of the stratigraphy of Pandi Wahi and Ghazi Shah, but both sites show evidence of the appearance of typical pottery design of the type found at Kulli and Mehi with the earliest Harappan pottery, and of a transition between the Amri and Harappa cultures.

were found at Ghazi Shah at -37' in a level at which only Amri pottery was found.

Figure 24 is not related to the fringed loop pattern, for it shows an "ibex" with wavy horns. Compare the long-legged bulls from Moghul Kala and the crowded form of this design at Sur Jangal.⁶

There is not space to discuss Starr's table (p. 8) of the relative position of the Mesopotamian periods and the levels of Iranian sites. It differs from Schmidt's views⁷ and from those of Ghirshman⁸ as well as from my own and, therefore, until publication of the evidence on which it is based, must be considered as highly tentative, as Starr himself does.

Starr's use of the term "great painted pottery family of Western Asia" requires some comment. Enough is known of Mesopotamia and Iran so that it is clear that the cultures with painted pottery cannot be treated as belonging to one original family. Two large families of light-faced wares are well known: that of Halaf and the buff-ware culture of Iran, to which the Samarra and Ubaid cultures were basically related. A third distinct group is represented by the culture of Siyalk II. The painted red pottery of Siyalk I is basically that of Siyalk II. The buff pottery of Siyalk I may represent a separate family or be related to the buff-ware culture. The pottery of Siyalk III and Hissar I is not a distinct fabric but represents a fusion of a Siyalk II type of tradition with the buff-ware culture. Thus we may distinguish at least three distinct painted pottery families in Western Asia, not just one.

Starr's explanation of the Halafian element in Harappan design also seems a little extreme. There is sufficient evidence to indicate that at a time corresponding to the beginning of the Ubaid period there was considerable Halafian influence throughout Iran. This was probably a result of the interaction between the Halaf and the incoming Ubaid culture while the latter was still in communication with the regions in Iran from which it derived. Certain Halafian traits, including some pottery design, were taken over in Iran and used in typically Iranian fashion. At a time roughly contemporary with the beginning of the Uruk period the cultures of northeastern and of western and southwestern Iran were displaced by cultures with plain, undecorated gray or red pottery. Some peoples of the cultures with painted pottery, as a result, apparently migrated to the east where we first find them in place after a considerable lapse of time in the Amri and Nal cultures. It was through this movement that Mesopotamian elements came indirectly and purely Iranian elements came more directly to the Indus Valley.

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⁶ *North Baluchistan*, Pl. XIII M.K.2; Pl. XX S.J.32, S.J.ii.1.

⁷ *Hissar*, p. 320.

⁸ *Sialk*, pp. 90-117.

BOOKS RECEIVED

"ISHMERAI." *Ezekiel Speaks Today: A Critical Analysis of the Prophecy of Ezekiel in the Light of Modern Events*. Boston: The Writer, Inc., 1941. Pp. xiii + 647. \$2.00.

Following the suggestion of "Swedenborg and others" that "beneath the ordinary words in the Bible are often spiritual values whose discovery makes the meaning clearer," the pseudonymous author of this huge work undertakes an interpretation, chapter by chapter, of the entire Book of Ezekiel.

"Such simple words as 'water,' or 'to eat,'" he tells us, are sometimes to be interpreted by the "spiritual concept they stand for"; otherwise the passages containing them will be incomprehensible. The method of discovery of this "spiritual" interpretation the writer happily makes clear to us. In Ezek. 34:23 "we read, 'I will set up one shepherd over them, and he shall feed them, even my servant David.' We naturally understand that 'David' here symbolizes the Messianic Christ, for the old 'David' was long since dead and to none but the Christ would such leadership belong. Again it is clear that the 'feeding' is the spiritual teaching we are to have through him. So we have gained two words to be tried whenever the literal significance appears in fault."

The application of this method to Ezekiel we may sample by the treatment of chapter 29. After noting the views of certain expositors (which satisfy him that something mysterious lies beneath the innocent words of the chapter), the author invokes a "spiritual 'Egypt'" which "may be quite as distinct an object of study as a geographic one," a view that is then bolstered by reference to Isa. 30:9 and 51:9. A little further study reveals the conclusion that the "dragon" of the chapter is likewise "spiritual," as is also the river in which it lies: it is some "group at variance with the Lord Jehovah, though not cut off from the stores of knowledge." Then the scales of the "dragon," the fish that stick in them, and the hooks for its jaws are all similarly illumined by this profound method. And so on through the entire chapter. And similarly for more than six hundred pages!

And what does it signify in the end? All the "spiritual" values so laboriously brought to light—in so far as they are real—are more readily revealed by the simple method of understanding the passages in their obvious interpretation related to the thought and circumstances of their writer's age: in brief, by the critical method which "Ishmerai" purports to survey but never understands. Six hundred and forty-seven pages of unmitigated drivel! Alas for the labor, which might have been turned to some good purpose!—W. A. IRWIN.

MARRERO, LEVI. *Perfil del imperio hitita*. ("Publicaciones de la 'Revista de los estudiantes de filosofía.'") La Habana, 1939. Pp. 19.

A short sketch of the political history of the Hittite Empire based on secondary, primarily French, sources. For readers who know nothing about the Hittites, sketches of this type may perhaps be useful, but a direct translation of a good original book would have served the purpose better.—I. J. GELB.



LUTZ, HENRY F. *A Neo-Babylonian Debenture and A Recorded Deposition concerning Presentment for Tax Payment*. ("University of California Publications in Semitic Philology," Vol. X, Nos. 9 and 10.) Berkeley, Calif.: University of California Press, 1940. Pp. 251-56 and 257-64. \$0.25 each.

Copies, photographs, transliterations, and translations (but no philological notes) of two Neo-Babylonian legal documents, one dated in the time of Neb-

uchadnezzar II, the other in the time of Nabonidus. Contrary to better practice, diacritic marks distinguishing various homophonous signs are omitted in the transliterations. In line 10 of the first text the copy and transliteration as *amēlāti ušabši* instead of the required *amēlmukinnā* are hardly possible, since *šubšū* does not mean "to cause to appear" but "to cause to be" or "to create." Instead of *amēlšanqū*, "the priest," translate "the scribe," as so often in Neo-Babylonian documents. The transliteration and translation of lines 12 and 13 in the second text do not seem convincing.—I. J. GELB.

LAUTNER, JULIUS GEORG. *Altbabylonische Personenmiete und Erntearbeiter verträge*. ("Studia et documenta ad jura Orientis antiqui pertinentia," Vol. I.) Leiden: E. J. Brill, 1936. Pp. xx+262. 10 guilders.

This study is a careful and exhaustive research in the legal terminology of economic texts from the First Dynasty of Babylon concerning the hire of persons for labor. Professor Lautner has consulted all pertinent texts within the limits he has set for himself and at times refers to similar materials from other periods. He has ordered his texts into three major groups, but he correctly recognizes that not all documents fall within these categories and that there are combinations or mixed types. His detailed investigation clarifies problems connected with the payment of wages and the position occupied by employer, employee, and contractor. In this connection the apprentice documents from the Chaldean and Persian periods might have been utilized for comparative purposes. In the tradition of Koschaker, the emphasis in Professor Lautner's volume is on an understanding of the legal side of the documents rather than on their interpretation from economic and social angles.—W. H. D.

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