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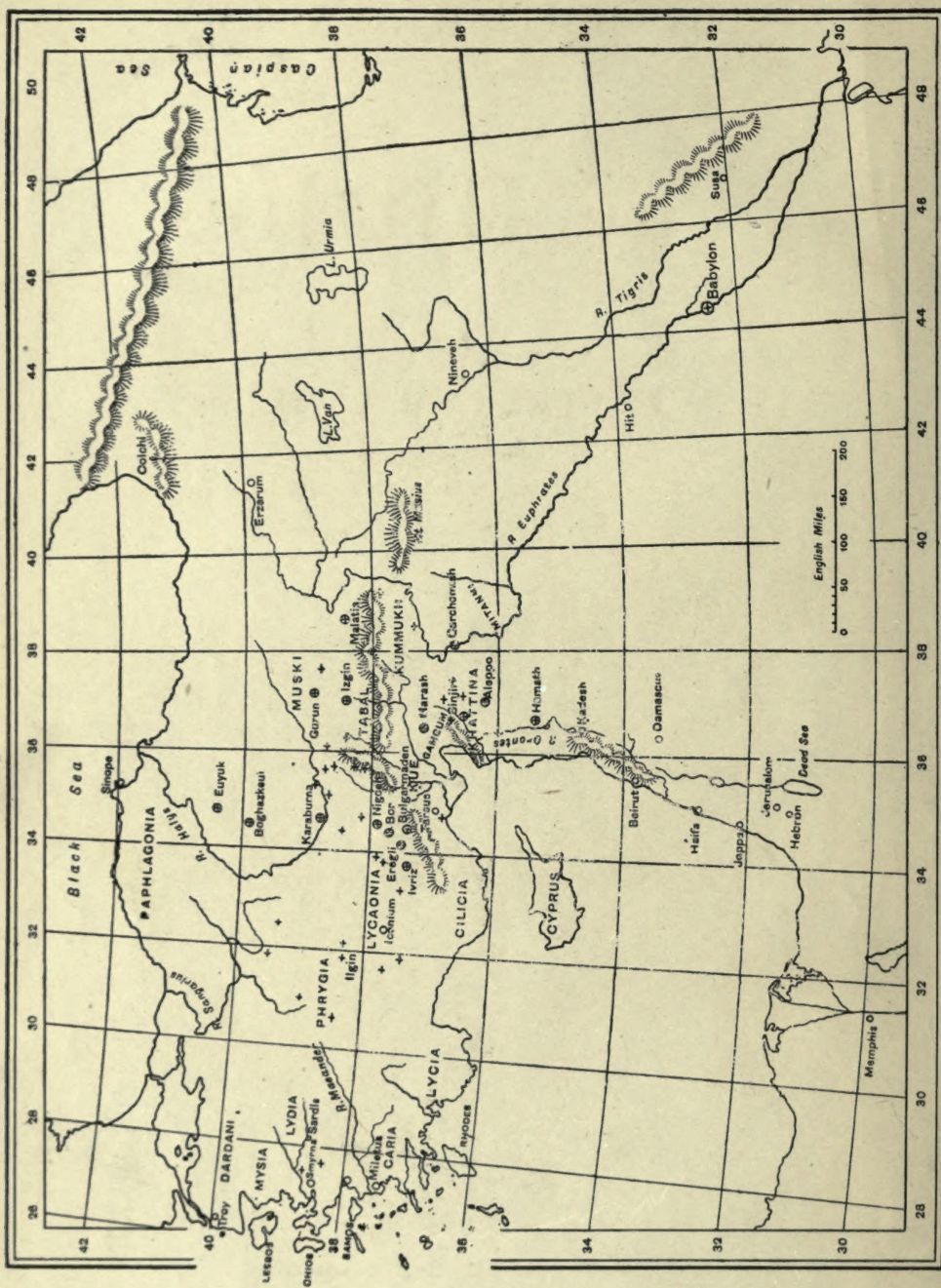




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# MAP OF HITTITE SITES



CRETE

+ Isolated monuments.

⊕ Sites with Hittite remains.

THE BRITISH ACADEMY

# The Hittites

By

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*The Schweich Lectures for 1918*

London

Published for the British Academy

By Humphrey Milford, Oxford University Press

Amen Corner, E.C.

1920

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PRINTED AT OXFORD, ENGLAND  
BY FREDERICK HALL  
PRINTER TO THE UNIVERSITY

The British Academy, London, 1918

London

Printed for the British Academy

by Frederick Hall, Printer to the University, 1, Broad Street, London, W.

Printed in Great Britain

1918



## PREFACE

THESE lectures were delivered in December 1918, but their publication has been delayed partly owing to difficulties about the strange characters, and partly owing to my other occupations. Nos. I and II now appear substantially as delivered; No. III has been somewhat expanded, and gives the main results of my own attempts at the decipherment of the hieroglyphics. I am anxious that they should be regarded only as lectures, intended rather to arouse interest in the subject than to satisfy it. In three hours it was not possible to do more than touch on some of the problems involved, and in several cases statements have been made which in a larger work would require modification or justification. It would have been more satisfactory to write a book on the subject, but even if I were competent to do so, the time has hardly come for fuller treatment. The material is only beginning to be accessible, and while these lectures were being printed, work has been done<sup>1</sup>, which may modify some of my statements or arguments. This I have been obliged to leave out of consideration.

Prof. Hrozný published *Die Sprache der Hethiter* (a full statement of his Indo-European theory) in 1917, and *Hethitische Keilschrifttexte . . . mit Übersetzung* in 1919. He very kindly sent me these a short time ago, but it was then too late to incorporate any discussion of them<sup>2</sup>. This matters the less, because an article was published in the *JRAS* (1920, p. 49) by Sayce, who, while giving full credit to Hrozný for his great acuteness and undeniable success in elucidating parts of the texts, at the same time rejects altogether the Indo-European theory.

<sup>1</sup> Especially the *Keilschrifttexte aus Boghazköi*, i-iii, published by Figulla, Weidner, and Weber in 1916, 1919, but only recently received here.

<sup>2</sup> I must, however, withdraw my statement (on p. 44) that he has not translated more than isolated sentences, though still holding that he is not helped by his Indo-European theory.

I have to thank the Trustees of the British Museum for permission to reproduce plates from their publications, and the Council of the Palestine Exploration Fund for similar permission. Some illustrations of well-known objects have been taken from photographs which I have had by me for a long time, and of which I do not remember the precise origin.

I also acknowledge most gratefully the help I have received from my learned friend Mr. T. W. Allen, with whom I have had the privilege of talking over problems connected with Asia Minor on many a genial evening; from Prof. Sayce by his constant encouragement and stimulus even when we differed in our conclusions; from Mr. Griffith in Egyptian matters; from Prof. Langdon in Assyrian; from the Controller of the Clarendon Press in the trouble he has taken over the printing.

A. COWLEY.

*May, 1920.*

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 ABBREVIATIONS

The following abbreviations have been used :

- A = Copies of inscriptions in Hogarth's *Carchemish* (plates A 1—).
- AJSL = American Journal of Semitic Languages.
- JAOS = Journal of the American Oriental Society.
- JHS = Journal of the Hellenic Society.
- JRAS = Journal of the Royal Asiatic Society.
- M = Copies of inscriptions in Messerschmidt's *Corpus*.
- MDOG = Mitteilungen der Deutschen Orient-Gesellschaft.
- OLZ = Orientalistische Litteratur-Zeitung.
- PEF = Palestine Exploration Fund.
- PSBA = Proceedings of the Society of Biblical Archaeology.
- QS = Quarterly Statement (of the PEF).
- TA = Tell-el-Amarna letters.
- TSBA = Transactions of the Society of Biblical Archaeology.
- ZATW = Zeitschrift für Alttestamentliche Wissenschaft.
- ZDMG = Zeitschrift der Deutschen Morgenländischen Gesellschaft.

The copies of hieroglyphic inscriptions are taken from Hogarth's *Carchemish* (1914) and from Messerschmidt's *Corpus Inscriptionum Hittiticarum* (Mitt. d. Vorderasiatischen Gesellschaft, 1900, 1902, 1906).

# THE HITTITES

## LECTURE I

UNTIL forty years ago, or less, the Hittites were still grouped with Hivites and Jebusites as an insignificant Syrian tribe unknown outside the Bible. It was only beginning to be suspected that they might be identified with the people called Kheta in the Egyptian records, and Khatti in the cuneiform texts of Assyria. The discovery of them began when attention was drawn to some curious engraved stones found at Hamath.<sup>1</sup> The first mention of these 'Hamath stones' apparently was by the French traveller La Roque<sup>2</sup> in 1722: 'Vis-à-vis du Château il y a une belle Mosquée, accompagnée d'un jardin, presque sur le bord de la rivière, au-devant de laquelle est une haute colonne de marbre ornée de bas-reliefs d'une excellente sculpture, qui représentent des figures humaines, plusieurs espèces d'animaux, des oyseaux et des fleurs.' A century later (1822) Burckhardt<sup>3</sup> says: 'I inquired in vain [at Hamah] for a piece of marble with figures in relief, which La Roque saw; but in the corner of a house in the Bazar is a stone with a number of small figures and signs, which appears to be a kind of hieroglyphical writing, though it does not resemble that of Egypt.' In fact no pillar of marble with a Hittite inscription ever has been found at Hamath. All the inscriptions there are on basalt, so that either La Roque saw some monument which was not Hittite, or the marble pillar had disappeared in the interval. Every one read Burckhardt, but another half century elapsed before any serious attention was paid to the matter. In 1870 two Americans, Johnson and Jessup, succeeded in finding inscribed stones at Hamath, but were pre-

<sup>1</sup> A good account of their discovery is given by W. H. Rylands in *TSBA*, vii (1882), p. 429, with plates.

<sup>2</sup> *Voyage de Syrie*, i, p. 243.

<sup>3</sup> *Travels in Syria*, p. 146.

vented from copying them by the usual fanaticism of the natives. They did, however, obtain a very imperfect drawing, by a local artist, of the one known as Hamath V (= M vi). This was published in the first *Quarterly Statement of the American Palestine Exploration Society* (1871), which I have not been able to see. Their account was reproduced (without the drawing) in the *QS of the PEF* 1871, p. 173. The Fund then commissioned Tyrwhitt Drake to get copies of the texts, since they now were known to exist and could be localized. Thanks to his great experience in



FIG. 1.

dealing with the natives, he contrived to take photographs and squeezes (fig. 1), which were published in the *QS* 1872, pp. 74, 199, and his account, *ibid.*, p. 11.

By this time interest was thoroughly aroused. It was a time of archaeological discovery. The decipherment of the cuneiform texts was beginning to be accepted, and was producing wonders, the Moabite stone had been brought to light, the Cypriote syllabary was being discussed. The learned world was therefore ready to be interested in yet another strange system of writing. The imperfection of the copies (cf. fig. 2), however, made the study of them difficult, if not impossible. Similar signs could not be distinguished, and a list of them was out of the question. The well-known traveller Burton, who was then H.M. Consul at

Damascus, saw the stones, and published revised plates (as in the *Journal of the Anthropol. Inst.* ii (1873), p. 41) of them in his



FIG. 2 (PEF, QS 1872, p. 200).

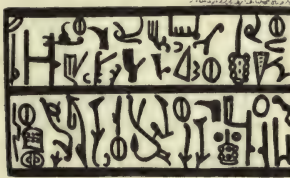


FIG. 3.

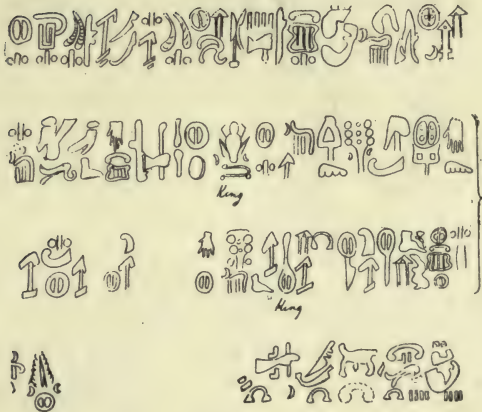


FIG. 4.

*Unexplored Syria* (1872, vol. i, p. 335) (figs. 3, 4), but his account, though full, added little to what was already known, except as to

the positions of the stones. It was William Wright who really began the serious study of the subject. In 1872, being then a missionary at Damascus, he took advantage of an opportunity to visit Hamath in company with the newly appointed Turkish Governor. It was an opportunity not to be lost, for now, if ever, it would be possible to exert authority to overcome fanatical opposition. He gives an excellent account of the expedition in his 'Empire of the Hittites' (1884). The result of it was that he obtained casts of the inscriptions, one set of which was sent to the British Museum, and another set to the PEF.<sup>1</sup> He also persuaded the Pasha to send the stones themselves to the museum at Constantinople, where squeezes were afterwards made for Berlin. Wright did far more than this, however, for in his book he dealt with the whole question of the authors of the inscriptions, and with the help of Sayce<sup>2</sup> supplied much of the preliminary research necessary for the study of them. It is largely due to his agreeable presentation of the material that general interest was aroused. A second edition of the book appeared in 1886, and the study of 'Hittitology', as some people have called it, was fairly started.

I have dwelt at some length on these 'Hamath stones', not because they are intrinsically of greater interest than other Hittite remains, but because they were the starting-point of the whole inquiry. So unmistakable were they in character, that, when once attention was drawn to them, no one could fail to recognize a Hittite inscription. Travellers began to look out for more of them, and as further specimens of the writing, and also of the art connected with it, began to accumulate in various parts of Asia Minor, it became more and more evident that the question of their origin was a very important one. It is unnecessary to enumerate all the travellers who have brought home copies. The chief are: George Smith, who excavated at Jerabis (which is Carchemish) in 1878; Hogarth and Headlam<sup>3</sup> in 1894; Humann

<sup>1</sup> See *QS* 1873, pp. 61, 74.

<sup>2</sup> Whose first article appeared in *TSBA*, v, p. 22 (read in 1876), using the name 'Hittite'. Other early articles need only a bibliographical mention: Hayes Ward in the second statement of the American Pal. Expl. Soc. 1873, and in *JAOS*, x (1880), p. 139; Heath in *PEF, QS* 1880, 1881, and in the *Journ. of the Anthropol. Inst.* 1880. The Aleppo inscription was published in Burton's *Unexplored Syria*, ii, p. 186, and by Clermont-Ganneau in *PEF, QS* 1883, p. 172. Cf. also *Journ. Asiatique*, 1873, p. 373.

<sup>3</sup> *Recueil de Travaux*, xvii (1895), p. 25.



and Puchstein<sup>1</sup> in 1882-3; Ramsay and Hogarth<sup>2</sup> in 1890; Anderson<sup>3</sup> in 1900; Olmstead and others in 1911. These were all (except the last) collected by Messerschmidt in his *Corpus*. The most recent and most important discoveries are those of Hogarth with Woolley and Lawrence in the excavations at Carchemish in 1911 and after. But above all, the study is indebted to Sayce, who has never ceased from the beginning to forward it with all the resources of his wide learning and brilliant genius.

A glance at the map will show that remains of this peculiar type are found sporadically from the north of Asia Minor (Eyuk) to Hamath in the south, and from the Euphrates in the east to the coast of Ionia in the west. You do not set up bulky monuments for fun. Evidently the people who did so were a widespread power. They must have occupied a large place in history. Who were they then? and how did they so completely disappear that scarcely a trace of them is to be found in all Greek literature? We now call them Hittites, but it must not be supposed that the identification was self-evident, or that it is entirely satisfactory, or that we know much more when we have agreed to it.

Wright claims (ed. i, p. 124) to have been the first<sup>4</sup> to apply the name, but it was Sayce who first<sup>5</sup> gave it currency. It did not meet with immediate acceptance, and even to-day one uses it with a half-apology. It is not the existence of a Hittite power which is in doubt. That is amply proved by the inscriptions of Egypt and Assyria. The question is whether the peculiar hieroglyphic writing discovered in the last fifty years, and the art which accompanies it, are the product of that Hittite power. Wright's arguments are certainly not very convincing, though his conclusion is nearly correct. He says in effect: here was a people powerful enough to leave records of itself throughout Cappadocia, even in Ionia, and down to Syria and Carchemish. They were not Egyptian nor Babylonian. The only power we know which could have done this, and disappeared before Greek history begins, was that called Kheta in Egypt, Khatti in Assyria, and the sons of Heth in the Old Testament. I need not point

<sup>1</sup> *Reisen in Kleinasien* . . . (1890).

<sup>2</sup> *Recueil*, xiv (1892), p. 74, and xv (1893), p. 89.

<sup>3</sup> *JHS* 1901, p. 322.

<sup>4</sup> In the *Brit. and Foreign Evang. Review*, 1874.

<sup>5</sup> In *TSBA* 1876, as above.

out the flaws in this argument, nor the large assumption on which it rests. Yet it has been justified.

Since the publication of Wright's book, monuments have been discovered at Malatia, Marash, Tyana, Ivriz, Babylon, Carchemish, and many less-known sites. But the next really important stage in the resurrection of this forgotten empire was when Hugo Winckler, in 1906 and after, excavated the mounds of Boghaz-  
*Köi* keui. It had long been recognized that these must conceal the remains of an important city, sometimes thought to be the Pteria, beyond the Halys, which was taken by Croesus.<sup>1</sup> Here, as well



FIG. 5.

as at Eyuk, some miles to the north, strange monuments had been discovered and drawings of them were published by Texier<sup>2</sup> in 1839 (figs. 5, 6). Great things might therefore be expected from the excavation of the site. The results were beyond all hope. Winckler found what could be nothing less than the state archives, containing about 20,000 documents or fragments, written, after the Babylonian manner, in cuneiform on clay tablets. His deeply interesting and brilliant account of them was published in *MDOG*, no. 35, in Dec. 1907. Some of the

<sup>1</sup> Hdt. i. 76. But that was probably further north, κατὰ Σινώπην πόλιν . . . μάλιστά κη κεϊμένη.

<sup>2</sup> *Description de l'Asie Mineure*, vol. i, p. 209, and plates.

tablets were written in Semitic cuneiform—the diplomatic and international language of the East at that time, as Aramaic was at a later date. These, of course, could be read with comparative ease. Many others, though written in cuneiform, were in what must have been the native language of the country, certainly not Semitic. This is not yet fully interpreted (see further in Lecture II). For the present the important point is that Winckler was able to establish beyond question the fact that the language was that of the Hatti, and the site of Boghaz-keui their capital. He also established the names and succession of the kings to



FIG. 6.

whom the archives belonged. Among them, by good fortune, was Hattusil, whose name had been read in Egyptian as Khetasira. This king made (about 1280 B.C.) a treaty with Rameses II of which the Egyptian text was already known. Fragments of a copy of it, in Babylonian cuneiform, were found in the Boghaz-keui archives. We thus arrive at the certainty that the Hatti were the Kheta of the Egyptian monuments, and also at a fixed date for the remains at Boghaz-keui. But further, the peculiar style of sculpture found there could only have been produced by the people whose city it was. Wright's or Sayce's conjecture was thus amply confirmed. The 'Hamath stones' have the same

origin as the Boghaz-keui sculptures—as we see from the hieroglyphics<sup>1</sup> which are common to both (fig. 7). They are, therefore, the work of the Ḫatti, who are the Kheta of the Egyptian monuments, who are the Ḫatti of Assyrian history, who are no doubt the Hittites of the Bible.

The earliest tradition of them is preserved in the Book of Genesis. In 10<sup>15</sup> we are told that Canaan begat Zidon his first-born and Heth, which is only a way of saying that in the records on which this chapter is based Hittites were described as settled in north Syria. They next appear at Hebron in



FIG. 7. (From Messerschmidt, *Corpus*, pl. xxvii.)

south Palestine, when Abraham bought from them the cave of Machpelah as a burial-place for Sarah (cap. 23). If the Amraphel of Gen. 14<sup>1</sup> was really the great Ḫammurapi, king of Babylon, whose date is approximately known, this transaction must have taken place somewhere about 2100 B. C. The account is, however, much later than the events, and is full of difficulties, which cannot be discussed here. The most we can say is that it seems to indicate that there was a Hittite settlement in south Palestine before the Tell-el-Amarna period and the Egyptian domination

<sup>1</sup> There are practically no *inscriptions* in hieroglyphics at Boghaz-keui, but isolated signs occur on the sculptures (see below), and these belong to the same system as those at Hamath.

of Syria.<sup>1</sup> They had perhaps diverged there from the main body in the course of a migration from north to south. That they were there for trade seems to be indicated by the phrase כסף עובר לסחר 'current money with merchants' <sup>2</sup> (Gen. 23<sup>16</sup>). It was therefore a case of peaceful penetration. Their first appearance in a military enterprise is when, in the reign of Samsuditana (1956–26 B.C.) they ventured to attack Babylon itself—Babylon the great—which had been made powerful by Hammurapi and developed by his successors. The Chronicle <sup>3</sup> merely says that 'the men of the land of Hatti marched against the land of Accad'. There is nothing to show what they did at Babylon, nor how long they remained there. They must, at any rate, have captured the city and plundered it, for apparently they carried off the statue of Marduk. It is generally supposed that this invasion or raid weakened Babylon so much that it ended the dynasty and prepared the way for the Kassite occupation. It is hardly probable that the Hittites conducted their expedition against Babylon from so distant a base as Boghaz-keui ('the land of Hatti'). It is more likely that they had already begun to spread southwards, attracted by the wealth and trading possibilities of Mesopotamia. Their presence in south Palestine may then have been due to the same movement. But the chronology of these centuries is so obscure, and our information so scanty, that it is better to record only what is stated by the documents, and for the present to beware of drawing conclusions.

The Kassite dynasty had established itself in Babylon by 1760 B. C. Who they were is another of the many problems of this dark period. They appear to have been a non-literary people, and even of their language the only specimen we have is one short vocabulary.<sup>4</sup> The history of their rule in Babylon is very obscure. It is said (in the lists of kings) to have lasted 576 years, i. e. till 1185 B. C. One of the kings, Agum II (about 1650 B. C.), in

<sup>1</sup> The sources are not quite consistent. In 14<sup>19</sup> Mamre is Amorite; in 23<sup>19</sup> it is Hittite.

<sup>2</sup> In II Kings 12<sup>5</sup> כסף עובר alone is used. סחר is properly a travelling trader. In Gen. 37<sup>28</sup> the Midianites who bought Joseph are called so. In 37<sup>36</sup> their name is המדינים. Is it possible that in one of the original sources they were so named and that they are Mitanni? The name would be altered to the more familiar מדינים but preserved here by an oversight of the Masorettes.

<sup>3</sup> King, *Chronicles*, ii, p. 22.

<sup>4</sup> See Delitzsch, *Die Sprache der Kassiter* (1884), p. 25, and Pinches in *JRAS* 1917, p. 101.

an important inscription,<sup>1</sup> says that he sent an *embassy* to the land of Khani to bring back the statue of Marduk, which had been carried off by the men of Khani. This is taken to refer to the Hittite raid mentioned before, so that the men of Khani would be Hittites, or, at any rate, members of a Hittite confederacy. It is to be noted that he sent an embassy, a friendly mission. He did not attempt to take the statue by force, the more usual method in those days. The men of Khani were therefore powerful, and it was prudent to be on good terms with them. Khani is usually taken as meaning Khana on the middle Euphrates, but it may mean Khani-rabbat,<sup>2</sup> which is Mitanni. If so, Hittites, Mitanni, and Kassites are here in close relation. This is merely a suggestion, but where all is so obscure the slightest clue is worth noting.

We do not know for certain on what terms the Hittites were with the early Kassite kings. It is evident, however, that their power, which was first shown in the invasion of Babylon, had not diminished in the next four centuries. Whether they gained by the goodwill of the Kassites, owing to alliance or racial connexion, or whether the temporary eclipse of Babylon gave them their opportunity, we cannot say. By about 1500 B.C. Egypt had become the dominant power in Asia. Thothmes I had conquered Palestine and marched as far as the frontiers of Mitanni, then a powerful state at the north of Mesopotamia. His grandson Thothmes III, early in the fifteenth century, completed the conquest of Syria, defeated the Hittites there, and exacted tribute from them. Carchemish was taken, as well as Kadesh on the Orontes. There is no evidence to show whether either of these cities was at that time in Hittite possession, as they both were later. In a subsequent campaign Thothmes III developed his success. He broke up the confederacy of which Mitanni was the head, and thus the whole of western Asia from Mesopotamia to the sea became subject to Egypt—including, of course, the Hittite states of Syria. This is the condition of things we find still existing when the Tell-el-Amarna letters begin. These are largely concerned with the intrigues of provincial governors in Asia and their difficulties in meeting the attacks of the Hittites. The main general fact which emerges with regard to the Hittites is that when the letters begin they are still settled in the north

<sup>1</sup> Jensen in *Keilinsch. Bibl.* iii. 1, 139.

<sup>2</sup> As in an inscription of Shalmaneser I. See *AJSL* 28, p. 188.

of Syria, and gradually extend southwards towards the end of the period. It was probably about the middle of the fourteenth century that they took possession of Kadesh on the Orontes. In the later letters, of the time of Amenophis IV (fig. 8), it is evident that the strength of Egypt is declining. Whether owing to troubles caused by that king's heresy, or for any other reason, troops were not sent when required to keep the unruly Syrian states in order. Partly in consequence of the disorganization of the country, the Hittite power began to grow as that of Egypt waned. The king of the Hatti (the dominant element) became the great king of



FIG. 8. (From *MDOG* no. 50).

a Hittite confederacy, with his capital at Boghaz-keui in the north, uniting the minor states represented by Hamath, Aleppo, Marash, Carchemish, Malatia, &c., and probably with more or less control over the rest of Asia Minor. It was a very formidable combination, with the best of reasons for holding together, since they were all threatened by Egypt on the one side and Babylonia on the other.

It is just at this point that the archives of Boghaz-keui<sup>1</sup> take up the story. The city must have been hitherto the head-quarters of one tribe or section of the confederacy. When the king of it became the 'Great King' of all the Hittites, his city became the capital of an empire and the repository of records of dealings

<sup>1</sup> See above, p. 6.

with his widespread dependencies. So we find the earliest of the kings whose archives are preserved there is the first of the 'Great Kings', named Subbiluliuma. His father Ḫattusil is called only 'King of the city of Kussar', a name otherwise unknown. It is evident, therefore, that he was a king in a small way, one of the kings of the confederacy. His son Subbiluliuma must have been a man of great force of character, since he succeeded in uniting the Hittite tribes into a really powerful state, and founded a dynasty. His reign was long, and though we cannot yet date the beginning and end of it precisely, we know from Tell-el-Amarna that he lived in the reign of Amenophis III and overlapped into that of Amenophis IV. He belongs, therefore, to the early part of the fourteenth century B.C. A TA letter<sup>1</sup> shows that the Ḫatti had been at war with Mitanni under Tushratta and had been defeated—for Mitanni was then a powerful state. But friendly relations must have been established since the Boghaz-keui records show that Subbiluliuma, as a sort of suzerain, supported Tushratta's son Mattiuaza on his accession after his father's death.<sup>2</sup> Having thus secured himself on the east, Subbiluliuma was strong enough to encroach on the Egyptian sphere of influence, and was acknowledged as overlord by the Amorites of Syria under Azir.<sup>3</sup> He contrived at the same time to remain on good terms with Egypt, but writes to Amenophis IV as an equal. The TA letters present a pathetic picture of the misery of the Egyptian provinces in Syria at this time, constantly subject to intrigue and war in which Hittites took a large part, much to their own advantage. It is not improbable that Carchemish became Hittite about this time.

Subbiluliuma was succeeded by his son Arandas, of whom there are no records, and then by another son, Mursil, read as Maurasira in Egyptian. In an interesting chronicle he mentions his father's conquest of Mitanni, and speaks of his own relations with various allied or subject states, mostly not yet identified.<sup>4</sup> He also maintained control over the Amorites of Syria. But Egypt had now recovered from its weakness and its new king, Seti I, regained possession of south Syria. This serious blow seems to have roused Mutallu (or Mutallis), who succeeded his

<sup>1</sup> To Amenophis III. Winckler no. 16. Cf. *PSBA* 15, p. 120.

<sup>2</sup> Tushratta himself corresponded in TA with Amenophis IV.

<sup>3</sup> This would be the first time, one would think, that they could have occupied south Syria, but it is too late for the event related in Gen. 23.

<sup>4</sup> Arzawa (known from TA), Gasga, Tibia, Zihria, &c.



father Mursil, to make a great effort to re-establish the Hittite power over unhappy Syria. He resumed the war with Egypt, and fought<sup>1</sup> a great battle against Rameses II (the successor of Seti) near Kadesh on the Orontes, which was still a Hittite stronghold. The Egyptian account of this battle is a well-known piece of literature. Things were going badly for Pharaoh: 'My warriors and my chariots had deserted me; not one of them stood by me. Then I prayed, Where art thou, my father Amon? . . . and Amon heard me and came at my prayer. He stretched out his hand to me and I shouted for joy . . . I was changed. I became like a god . . . like a god in his strength, I slew the hosts of the enemy: not one escaped me. Alone I did it!' But apart from its Homeric setting, the account is historically important because it indicates the extent of the Hittite confederacy. To oppose Rameses they had summoned contingents from Syria and Phoenicia, from Aleppo and Carchemish, Dardani, and Masu,<sup>2</sup> and others whose identity is uncertain. It was probably the greatest effort they ever made, and it nearly succeeded. Evidently both sides suffered severely, for Mutallu found it safer to shut himself up in Kadesh and Rameses did not follow up the victory he claimed. In consequence of his failure Mutallu appears to have been deposed, and no doubt murdered, by a military conspiracy after a short reign.

His brother Hattusil, who followed him, had a long and eventful reign, largely occupied by his dealings with Egypt. As most of the Boghaz-keui documents belong to him, we may hope for a good deal of information when the language of them is better understood. He was a powerful and resourceful king, a worthy grandson of Subbiluliuma. His policy was the same as that of his grandfather, and was in fact the only possible policy for a state situated as the Hittites were, with an equally powerful rival on either side. He maintained his friendship with Babylon (still Kassite) and his alliance with Mitanni, so protecting himself against the growing power of Assyria on the east, and at the same time kept a hold on the Amorites in the west. He was thus in a strong position to deal with Egypt.

<sup>1</sup> The name of the 'miserable king of the Kheta' is not mentioned in the Egyptian account, but the reference to Mutallu as having made war on Egypt, in the preamble of the treaty, is generally taken to mean that he was in command at Kadesh.

<sup>2</sup> Perhaps the Hebrew מִשׁ (Gen. 10<sup>23</sup>). Hardly Mysians, as generally explained.

Rameses, in spite of his boastful record of the battle of Kadesh, was content in his twenty-first year (c. 1280 B.C.) to make a treaty with the Hatti, leaving to them Syria and all western Asia from the Euphrates to the sea. The treaty was a great event. The fragments found at Boghaz-keui evidently belong to a draft of it, and the terms were much discussed by letter before it was finally presented to Rameses for ratification.

But in spite of Hattusil's diplomacy, the Hittite power from this time began steadily to decline. His reason for making the treaty with Egypt may have been that he foresaw danger from the increasing power of Assyria. At any rate it must have been soon after 1280 (the chronology is not quite certain) that Shalmaneser I in his great stone inscription<sup>1</sup> records with pride how he conquered the land of Khani(rabbat), or Mitanni, and 'slaughtered the army of the Hittite and the Aramaeans, his allies, like sheep'. This was the end of Mitanni power, and of any help it might give to the Hittites in their struggle.

The kings after Hattusil were his son Dudhalia, who mentions Carchemish as a vassal state under Eni-Tešup (a Hittite name), and his grandson Arnuanta—neither of them apparently of much importance. The Boghaz-keui records then cease—about 1200 B.C. It is probable that the city was losing its dominant position by this time (owing to pressure from the west?) and that the Hittite centre was being gradually transferred to Carchemish in the south. Assyria was temporarily eclipsed after the death of Tukulti-ninib, and as Egypt was also weak, it was a time of unusual peace, with no power able to restrict the southward expansion of the Hittites and their trade. Unfortunately we have in consequence very little external information for the years just after Boghaz-keui stops. From Egyptian sources we learn that the Hittites took part in an invasion of Egypt from the sea in the reign of Rameses III (twelfth century). They were no longer, however, the leading power among the allies. They merely joined in an attack which was organized from the west. It failed, and this is the last time they came in contact with Egypt.

It is from Tiglath-Pileser I, under whom Assyria again became powerful, that we next hear of changes in the Hatti state. He broke up their federation, about 1120 B.C., and was recognized by Egypt as the conqueror of Syria and north Palestine, which the Assyrians called Hatti-land. He did not, however, take Carche-

<sup>1</sup> Translated by Luckenbill in *AJSL* 28, p. 188.

ish, and this continued to be their chief centre, though we get no more news of it for more than two centuries. In his time we begin to hear of the Muški (Μόσχοι, Hebrew מושק), a powerful tribe who seem to take the place of the Hittites as head of the confederacy.

It has been suggested that the Kassite conquest of Babylon may have been facilitated by the Hittite invasion which preceded it. Whether or not the Hittites were racially connected with the Kassites, or had a particular interest in their fortunes, it is at least striking that we hear of them again at the end of the Kassite dynasty. That came to an end in 1181 B.C., and was succeeded by the Semitic dynasty of Isin, and some thirty years later the Hittites ventured to invade Babylon again. But this time they encountered Nebuchadrezzar I, a very different person from Samsuditana. They succeeded in taking the city, but not in holding it. In thirteen days Nebuchadrezzar drove them out and pursued them westward as far as Syria. It was merely a raid, which cannot have had any serious political effect, and never again did Hittites attack Babylon. In fact their glory was departed.

In all this long story, largely concerning Syria since the time of Hammurapi, there has been no mention of the people with whom we naturally associate it—the Israelites. Indeed, their entry into the promised land can have happened only a short time before the events just narrated. The Hittite control of Syria had been broken, and the Amorites, who had shared their ascendancy, shared also their downfall. This does not mean that no Hittites or Amorites were left in the country. On the contrary the books of Joshua and Judges mention both specially. The population remained, but the land was without a government, and therefore an easier object of attack to the Israelites under Joshua. That the invaders amalgamated with the native population is stated in Judges 3<sup>5,6</sup>, and Ezekiel's taunt (16<sup>3,45</sup>) of Jerusalem (some centuries later) is no doubt founded on historical fact: 'The Amorite was thy father, and thy mother was an Hittite.' The basis of the population must have remained largely Hittite, and when we can read the language we may find that their influence was fundamental. Indeed the Hittites were so closely associated with Syria that it continued to be called Hatti-land long after they had lost their hold on it. Similarly the name was applied vaguely to members of the confederacy, irrespective of race. It was a great name, and the

Assyrians did not forget it. After Tiglath-Pileser I there is a blank in our sources of information for about two centuries, during which Assyrian records give very little information at all. This interval must have witnessed the rise of Carchemish, and also the growth of Aramaean power.

The rest of the story of the Hittites now centres round Carchemish, and is a record of continual struggle against Assyria, with varying success, but always tending to the inevitable end. The Assyrian accounts are very full, and I can only indicate here the main features of the history. Assurnazirpal<sup>1</sup> (884-858), in his campaigns to the north and north-west, to strengthen his hold on the provinces there, after savagely crushing many small states, received tribute from Milid and Kurḫi, members of the Hittite confederacy. He had already subdued Kummuh. His constant attacks on the Aramaean states along the Euphrates show the importance which these had attained, probably at the expense of the Hittites. In 877 he took Carchemish. Owing to its position the city had become a rich commercial centre, under its king Sangara. It was for this reason that Assurnazirpal attacked it, and a large tribute was exacted. He then went on to the neighbouring and allied state of Hattin (capital Kunulua, under Labarna), through which the trade passed to the Orontes, and so on to the Lebanon and the sea. Here also a large tribute was exacted.

His son Shalmaneser III (858-824) carried out the same plan still further. He again had to deal with the Aramaeans, but his main object was to crush the Hittite confederacy. There could in fact be no peace for Assyria until these troublesome states were reduced to Assyrian provinces. They must have rebelled again, for he took tribute (to name only places of interest here) from Carchemish (King Sangara), Kummuh (King Kundashpi), Milid (King Lulli), Hattin (King Kalparuda), Pitru and Aleppo (whose god was Adad). He also fought a great battle at Karḫar, near the Orontes, against an army of allies from Hamath (Irḫuleni), Damascus (Bir-idri), and Israel (Ahab), with others. Though he claims a great victory, he was unable to follow it up. The alliance was powerful, and if it could have held together it might have maintained its independence, but it had too many incompatible interests to last. Adadnirari IV<sup>1</sup> (810-781) took

<sup>1</sup> See his inscriptions in Budge and King, *Annals of the Kings of Assyria* (1902), p. 155.

tribute from Damascus and Syria, which was now only traditionally called Hatti-land. While the Hittite power was thus being gradually broken by Assyria, it also had to contend with the new kingdom of Van, as we learn from the Vannic inscriptions. This kingdom had risen to importance soon after the death of Shalmaneser III. One of its kings, Sarduris III, about 750, overran north Syria and compelled the Hittite states of Milid (King Sulumal), Gurgum (Tarkhulara), Kummukh (Kushtashpi), and probably Carchemish (Pisiris), to form an alliance with him



FIG. 9. (From Hogarth, *Carchemish*.)

against Assyria. This bold adventure was crushed by Tiglath-Pileser IV (746-727), who took tribute from all the allies, as well as (or including) Damascus (Rezin), Kue (Urikki), Hamath (Enilu), Sam'al (Panammu), Tabal (Uassurme), Tyana, and many others.

The end of this 'strange eventful history' came with Sargon II (722-705). Hamath had again become a centre of opposition to Assyria, under its king Yaubidi or Ilubidi (successor of Enilu), who is called a Hittite. He was killed and the city was taken. Carchemish had managed to remain independent, and its king, Pisiris, was called *šar mat Hatti*, as though his city was now the capital of Hatti-land. He now joined with Mita of Muški in an

attempt to withstand Assyria. But the unity of the Hittite states had been broken and they were powerless except in a large combination. Pisisis was defeated and taken prisoner, together with large booty from the prosperous city. In order to guard against any trouble from it in the future, Sargon reduced Carchemish to the status of a province of the empire under an Assyrian governor in 717 B.C. Revolts of some minor states, such as Milid (Tarḫunazi) and Gurgum (Mutallu), had to be suppressed in the next few years, but this may be said to be the end of the Hittite power. Owing to its position Carchemish remained an important place for some centuries. It is now a mound whose identity has only recently been established by archaeological evidence (fig. 9).

## LECTURE II

THE fall of Carchemish in 717 B.C. marks the end of the Hittite empire as such, though, after the central power was gone, the population in various Hittite centres must have remained much as it had been—only paying tribute to Assyria instead of allegiance to its own Great King. Now, with the help of the map, let us review the history in the light of geography. The two Hittite capitals were Boghaz-keui in the north, and Carchemish in the south. The latter must, from its position, always have been a great trading centre (just as Aleppo was in the seventeenth century), and its importance is shown by the fact that a special standard of weight used by the Assyrians was called after it, the mina of Carchemish. It was a natural point by which trade from Mesopotamia should pass the Euphrates on its way to the west and north, and it is at Carchemish (Jerabis,<sup>1</sup> Jerablûs) that the great Baghdad railway at this moment crosses the river. The city, no doubt, originated through the exigencies of trade. It is mentioned in Egyptian records as early as c. 1480 B.C. Whether it was originally founded by Hittites we do not know, but it was certainly allied with them 200 years later. Like every other prosperous empire, that of the Hittites depended on trade, and it was more profitable to have a great centre at Carchemish, in touch with the resources of Mesopotamia, than to be shut off in Cappadocia behind the Halys, without ready access to important markets. The wealth of Mesopotamia was very great. Herodotus (i. 193) says that in corn alone it yielded two or even three hundred-fold, thanks to the elaborate system of irrigation maintained by the Babylonians. This has been entirely destroyed by neglect under the devastating rule of the Turk, but we may hope for the restoration of the country now that that tyranny is overpast. There were also oil-wells at Hit from which bitumen was obtained. It was in order to take advantage of these opportunities that the Hittites established themselves at Carchemish, and the position

<sup>1</sup> This identification has now been established beyond question. See Hogarth, *Carchemish*, p. 13. The origin of the modern (and ancient) name, however, is not certain.

of the Hittite sites shows the connexion which must have existed between the northern and southern capitals. These sites mark a very definite band of territory from Boghaz-keui in a straight line down to the sea, with an outpost at Bulgarmaden, where the silver mines were worked. In the north-east it included, in the later period, the kingdom of the Moschi; and going south, Tabal, Malatiah, Kummuh (Commagene), Gamgum (or Gurgum), and Kue.<sup>1</sup> South of the Amanus mountains they took in Hattin, the great city of Aleppo, Kadesh on the Orontes, and, at one time, Hamath. To the south-east there was Mash (? Mount Masius), Carchemish itself, and, more or less dependent, Mitanni or Hanirabbat. We know that many of these regions were, sometimes at least, under independent kings, whose allegiance to the suzerain at Boghaz-keui or Carchemish was more or less sincere at different times according to different circumstances. At its best it was a most formidable confederacy, and at its weakest it still formed a serious barrier to the extension of Mesopotamian power. The chief bonds which kept the allies together were the need of mutual protection against Assyria, and the advantage to be gained by facilities for trade passing through friendly territory. Trade follows where armies lead, and it was for both military and commercial purposes that the great high road from north to south was maintained.

If the mention of Hittites at Hebron in the time of Abraham is really to be dated as early as 2000 B.C., that would seem to have been the time of their greatest expansion. After over-running most of Asia Minor they seem to have effected a peaceful penetration into south Palestine and even held land there. They were therefore not merely passing through—but still less did they originate there. It looks as if this were part of a considered *Weltpolitik*, for it was about the same time that their first attack on Babylon occurred—a bold scheme for securing the resources of Mesopotamia, and the control of the whole world as they knew it. The plan, if it was one, failed, and so Semitic civilization was, perhaps providentially, saved for the world. It would be interesting to consider what might have been the course of history if the Hittites had mastered Babylonia.

We have then this solid barrier blocking the way of the Mesopotamian powers on the north and west. As early as 3800 B.C. we find Sargon of Accad making his way to the utter-

<sup>1</sup> i.e. Eastern Cilicia. The name probably occurs in I Kings 10<sup>22</sup>.



most west as he conceived it, and washing his weapons in the Great Sea—the Mediterranean in north Syria. This was a *tour de force* which came to nothing permanent, but it does show what Babylonia and Assyria would have done if they could. At some time in the next 1,800 years Babylonian influence must have spread over Cappadocia, but it was checked before the time of Hammurapi by the growth of Hittite power. If Hammurapi was the Amraphel of Gen. 14 and fought in Syria, the expedition can have been no more than a small raid, and one wonders why he made it. So great a king would not have been defeated in any serious purpose by the local potentates of south Palestine, even reinforced by Abraham's 318 trained men. Later on, the Kassite kings of Babylon had influence of some kind in north Syria, but they clearly found it prudent to remain on good terms with the central Hittite power. After the rise of Assyria we find campaign after campaign conducted against one or other member of the Hittite confederacy, or allied tribes. The Assyrians collected the tribute on which they lived, but there was never a break-through in the north. They did indeed conquer Syria after the Hittites had lost their hold upon it, but the country beyond the Taurus remained untouched by them. When, centuries later, Assurbanipal received an appeal from Gugu (or Gyges) of the important and growing kingdom of Lydia, for help against the Cimmerians, he has to confess (or did he say it in scorn?) that neither he nor his fathers had ever heard of such a place.

It was not till two centuries after Sargon II had broken the Hittite power by the capture of Carchemish, when one by one the related kingdoms had been reduced, when Nineveh was destroyed, Babylon captured, and Semitic civilization at an end, that the new Persian empire, with an even larger ambition than its predecessors, could stretch out its hands over Asia Minor, construct the great royal road from Susa to Sardis, bridge the Hellespont, and even enter Europe to attack Greece.

Now consider what all this means. It was because of that rampart against Semitic influence, and because the attention of the Hittite power was always inevitably directed in self-defence towards the east and south, that the west of Asia Minor and the Ionian states were left to self-determination, to develop in their own way. Yet such is the gratitude of the human mind, that by the time Greek history begins the very name of the Hittites was forgotten, and barely a reminiscence of their power is to be

found even in Herodotus (i. 76), who himself belonged to Asia Minor.

In their history, as sketched in the first lecture, we find two great periods, which might be called the Northern and the Southern.

The Northern period was first revealed by the TA letters, when the Hittite head-quarters were in Cappadocia, with influence over Syria. The excavations at Boghaz-keui, which showed that to be the site of their capital, showed also that their leading position began with Subbiluliuma in the time of Amenophis III (say c. 1400). But they were there much earlier, probably before 2000 B.C., if only as one of several related or allied tribes.<sup>1</sup> It was during this earliest period that their influence (with our limited knowledge it is better not to use a more definite word) extended over all Cappadocia and westward to the sea, as we may infer from monuments to be mentioned presently. We have also direct evidence that this westward influence lasted on till 1300 B.C. at least, since according to the Egyptian account they were aided in their war with Rameses II by Dardani<sup>2</sup> and people from other parts of Asia Minor. We may then picture a group of peoples extending from eastern Cappadocia to the sea, able to combine for offensive and defensive purposes, probably under the suzerainty of the strongest group settled at Boghaz-keui. As long as they held the coast they prevented colonization from the West, and effectually cut off the coast from Semitic influence from the East, but they also acted as a connecting link. They were a trading people, and exchanged the goods of the mainland for the wares of the Aegean, since objects of Aegean workmanship have been found in Mesopotamia, where they could hardly have penetrated by any other means. But even at this early date (say c. 1400 B.C.) they were turning their attention to the south and south-east, and it was this tendency which ultimately prevailed. Some time in the fourteenth century they appear to have been established at Carchemish, and soon after that the archives at Boghaz-keui stop. It would seem then that as their attention was more and more diverted from the west, Boghaz-keui gradually lost its supremacy, or perhaps

<sup>1</sup> Many Cappadocian cuneiform tablets, in Semitic, are known. In the *Philadelphia Museum Journal*, 1918, p. 149, Sayce points out that some belong to c. 2500 B.C. They are native commercial documents, probably Hittite, though in the Babylonian language.

<sup>2</sup> Not so Petrie, *History of Egypt*, iii, p. 49.

succumbed to hostile attacks. If we put the decline of it roughly at 1200 B. C. when the archives cease, this coincides in a remarkable way with other events, some of which at least must have been connected with it. Those were stirring times. The defenceless state of Palestine made possible the entry of the Israelitish tribes. Soon afterwards, on the break-up of Cretan power, the south of Palestine was equally open to colonization by refugees from the island,<sup>1</sup> who eventually gave their name to the country (Philistines, פְּלִשְׁתִּים, Pulasata, Πέλασγοι). In the north-west as the Hittite power gradually contracted, or was diverted from the sea, the allied states were left to take care of themselves. Their old allies the Dardani of the Troad were attacked by the Greeks and their city destroyed in the Trojan war (traditional date 1184).<sup>2</sup>

The object of the Trojan war was no doubt to promote colonization. At any rate it was soon afterwards that the Ionians began to establish themselves on the coast. The same cause, the removal of the suzerain power, led to the rise of various states in Asia Minor as independent kingdoms. Thus while the Hittite power, in its earliest period, protected the west from Semitic intrusion, its withdrawal from the west gave the opportunity for fresh developments there.

What then can we discover as to the racial character of the people who played so great a part in the history of Asia Minor and indirectly of Europe? The old view was that their original home was in Syria, along with the other tribes associated with them in the Old Testament, and that they radiated from that centre northwards to Cappadocia and eastwards to Carchemish, &c.

<sup>1</sup> The wars of Cyprus and Edom are mentioned in the mediaeval ספר הישר (ed. Frankfurt a. M. 1706, fol. 106), where the Cypriote king is named אַבְיָאִים, but I do not know the source of the account.

<sup>2</sup> In Homer's catalogue of the Trojan allies, Sayce has pointed out that the name of the Ἀλιζῶνες (*Il.* ii. 856) may be connected with that of the Halys, therefore Hittite. They came τηλόθεν ἐξ Ἀλύβης, ὅθεν ἀργύρου ἐστὶ γενέθλη, 'far off from Alube, where there is a vein for the silver' (Bulgar-maden?). This name, though not the place, may be compared with Khalupu, Aleppo, and both with the Χάλυβες, cf. Lagarde, *Beitr. zur baktr. Lexicog.*, p. 14. Themistagoras, quoted in the Ἐπιμερισμοί, says that Ἀλύβη was Lycia. In the *Odyssey* (xi. 521) there is a mention of the Κήτριοι, under their king Telephus, to the south of Troy opposite Lesbos. Sayce again identifies these with Hittites. The story was that Agamemnon, on his way to Troy, got lost, had to fight the Κήτριοι, made an alliance with them, and was afterwards thrown over by them when they joined Troy. But Agamemnon probably knew what he was doing, and saw the necessity of removing a danger to his subsequent operations. The Κήτριοι would then be relics of the former Hittite occupation.

But the recent discoveries make this quite untenable. As far back as our records go we find the Hittites established in Cappadocia, making their capital at Boghaz-keui, and evidently with a long history behind them there. Where they came from and what their racial character was can only be discovered, if ever, by a thorough examination of all the remains of their art, their religion, and their language. This of course cannot be done here, and in fact any conclusions reached now would be only provisional. I shall therefore only indicate very shortly those conclusions which for the present seem probable.

It has been suggested lately that the race originally came down from the mountains of Armenia or the Caucasus. Jensen makes it the former, and identifies the language of the hieroglyphic inscriptions with modern Armenian—without much success. Others, and I think Sayce was the first, propose the Caucasus. Sayce calls attention to the turned-up boots,<sup>1</sup> or snow-shoes, as indicating a mountainous origin. Prof. Rostovtseff, of Petrograd, the chief authority on south Russian archaeology, tells me that in studying the antiquities of the east end of the Black Sea, he finds that the earliest culture resembles that of Elam, and at a date which he roughly estimates at about 1500 B. C. he discovers a clear connexion with Hittite art; that is to say, the art developed in the same way as the Hittites developed it. Unfortunately the proofs of this have not yet been published, but as one small instance compare the metal girdle on the figure at Boghaz-keui with the bronze girdle found in south Russia<sup>2</sup> (fig. 10). If such a connexion is corroborated, it would be direct evidence of Caucasian<sup>3</sup> origin.

Coming then from the east, probably from the Caucasus, they first established themselves at Boghaz-keui, and then spread westward. There can be no doubt about this westward expansion, since monuments<sup>4</sup> manifestly Hittite are found on their path (see map), and right up to the coast, at Sipylus and Karabel (figs.

<sup>1</sup> These are characteristic and have a reason, in spite of the fact that modern oriental shoes are rather like them.

<sup>2</sup> See Farmakovski, 'Arkhaicheski period v' Rossii', in *Materiali po arkheologii Rossii*, no. 34 (1914).

<sup>3</sup> Here was Colchis, or Aia, where the Argonauts went in their mysterious quest of the golden fleece, a very early legend, and near was Ashkenaz, which Jeremiah associates with Ararat and Minni, and which gave its name to the πόντος Ἀσθενος. Hüsing derives the name of the Caspian similarly from the Kassites with an Elamite formative (Memnon, iv, 1910, p. 22).

<sup>4</sup> Accounts of these are most accessible in Garstang.

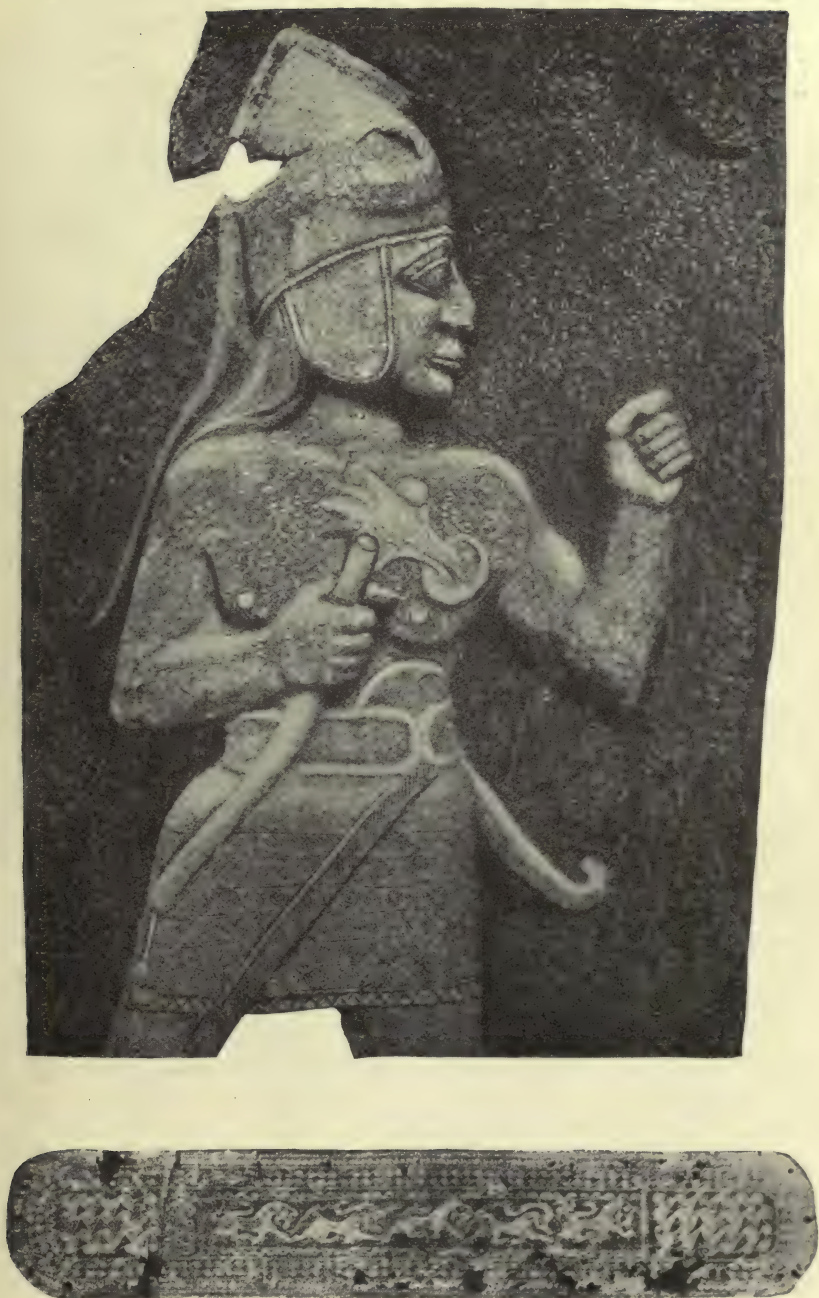


FIG. 10.

11, 12), with hieroglyphic characters on them. We have no historical evidence of this march towards the sea. We may get it when the Boghaz-keui records are read. Meanwhile the very silence of history is an indication that the movement must have taken place



FIG. 11.

in the earlier, northern period before the Hittites were in contact with Mesopotamia, and this is corroborated by the meagreness and archaic character of the hieroglyphics on the monuments, as though the system of writing had not yet been developed.

What we can gather from early monuments as to Hittite

religion points to the same westward extension. The chief god seems to have been Addu, who is Hadad or Rammānu, Rimmon. He was primarily a storm-god, and was represented with lightning in his hand (fig. 13). But the place of honour belonged to a



FIG. 12.

goddess, whose Hittite name is unknown. She is probably represented in the figure at Sipylos, and is certainly found in the Boghaz-keui sculptures<sup>1</sup> in close relation to the god. Now it cannot be a mere coincidence that we find the worship of Cybele and Attys prevalent at a later date in Phrygia, and the mother

<sup>1</sup> See above, fig. 7.

goddess at Ephesus. Sayce has referred <sup>1</sup> at length to the account (attributed to Lucian) of the worship at Bambyce (Membij), near Carchemish, which he takes to be a survival of the ancient Hittite religion. If so, there are some remarkable points in which it

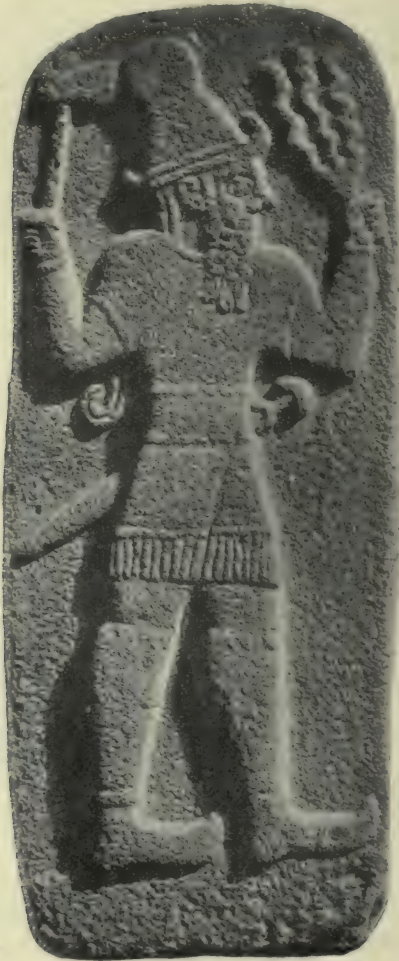


FIG. 13. (From Koldewey.)

may have influenced Jewish ritual, or been influenced by it. But the account is late, and we must beware of relying implicitly upon it.

The physical appearance of these people is shown by a number

<sup>1</sup> In *The Hittites* (1903), p. 105.





FIG. 14.



FIG. 14 a.

of portraits. For the early period we have those of the Egyptian monuments (figs. 14-16, thirteenth century B.C.). They are evidently faithful drawings from life, not merely conventional



FIG. 15.



FIG. 16.

representations of foreigners. Note the curiously Mongolian<sup>1</sup> type, no helmet, the pig-tail (?), the lack of hair on the face.

<sup>1</sup> One cannot help comparing them with some of the Etruscan types, and recalling Herodotus's story of the origin of Etruscans from Lydia. There is also the curious resemblance of Tarquinius to Tarku.

We may reasonably assume that they do represent the prevailing Hittite type of the thirteenth century B.C., though it is also possible that the particular figures which happen to be preserved are those of some of the many confederate peoples, or even caricatures. Now for the later period (say ninth century B.C.) we have the native portraits at Carchemish—in a quite different

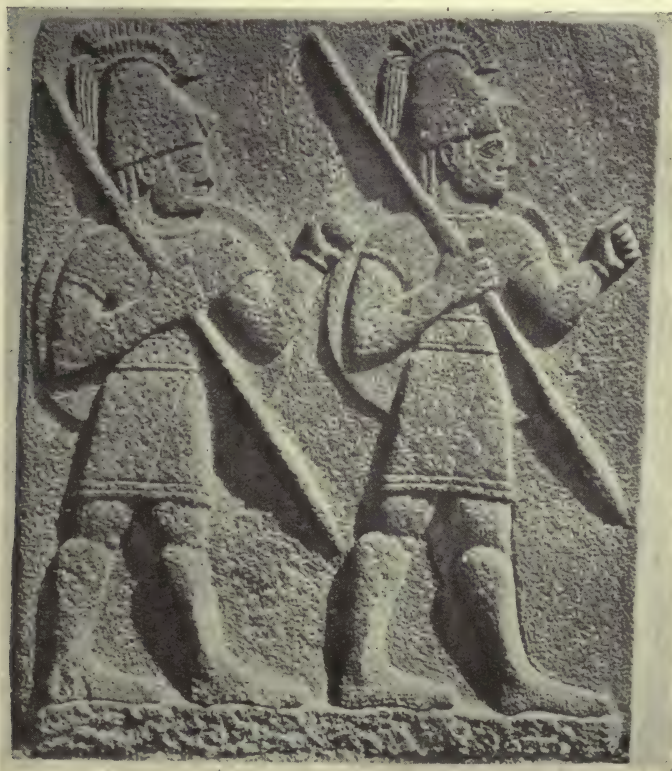


FIG. 17. (From Hogarth, pl. B: 2.)

style (figs. 17-19). The god at Ivriz (fig. 20) also belongs to this later style, and no doubt represents the general type of the Hittite population of Cilicia. The representations of Hittites from north Syria on Shalmaneser's bronze gates (figs. 21-23) give us little information, as they are evidently conventional. Thus we find two distinct types which would seem to represent different races. The Egyptian portraits look as if the people were dolichocephalic and probably of a Mediterranean stock. The native sculptures, so far as I know, never represent this

type, but that of an apparently brachycephalic Armenoid stock. If then there were two races we might take the Egyptian pictures to represent an aboriginal race in Asia Minor, and the native sculptures to represent an intruding conquering race from the Caucasus, which ultimately dominated the aborigines.<sup>1</sup> The rank and file of the army at the battle of



FIG. 18. (From Hogarth, pl. B. 4.)

Kadesh would naturally belong to the native race. The king would, of course, be of the conquering race, and so it is not surprising to find that the portrait (if it is not merely conventional) of Hattusil bringing his daughter to Rameses II is quite unlike those of the warriors of Kadesh, and more like those of the later period (fig. 24).

The peculiar style of Hittite art is due to the conquering race,

<sup>1</sup> But see Sergi, *Origine e diffusione della Stirpe Mediterranea*, p. 54.

who brought it, and probably the rudiments of the hieroglyphic writing,<sup>1</sup> with them from the Caucasus. The monuments in the west also belong to the same race, and from their scattered situation and small numbers, we may assume, until further discoveries are made, that the expansion was in the nature of a conquest, not of a settlement, though no doubt trade settlements (as the *Κήτριοι*?) were made at some points. It is to be noted that no monuments



FIG. 19. (From Hogarth, pl. B. 11.)

of this kind have so far been found in Caria, Lycia, or western Cilicia, i.e. in the south-western corner of Asia Minor, which was directly connected with Crete via Rhodes and the islands. Even apart from this corner we have no evidence that the tribes in the rest of Asia Minor were all of the same race, and if the view I have put forward is correct, it is not probable that those tribes were of Hittite (i.e. the conquering) stock.

In trying to clear up the ethnography valuable work has been

<sup>1</sup> Cf. the signs on a sceptre from Kedabeg, in Trans-Caucasia, *PSBA* 1899, p. 238.

done by Kretschmer,<sup>1</sup> Fick,<sup>2</sup> and Sundwall<sup>3</sup> on Asia Minor names, but for the present the arguments based on them are not fully convincing. The names require much sifting before we can be satisfied as to whether they belong to different strata of the



FIG. 20.

population. Language is not a proof of race, and the argument from names is largely linguistic. Hence we want to know the

<sup>1</sup> *Einleitung in d. griechische Sprache* (1896), p. 289.

<sup>2</sup> *Vorgriechische Ortsnamen* (1905). He boldly assumes that all names in Asia Minor which do not look like Greek are Hittite, and since such names are also found in Crete and European Greece, he is forced to admit Hittites there also.

<sup>3</sup> *Die einheimischen Namen der Lykier, u.s.w.* (1913) [Klio, Beiheft 11].

meanings of the non-Greek names in Asia Minor, and we want to know more of the language or languages of the Hittites. Then if we find the names explicable from a Hittite language, we shall be justified in concluding that they were given to the places by Hittites who were there either as conquerors or as settlers. That is all. In proportion as such Hittite names prove to be few or many, we may argue as to the extent of their occupation. They may, of course, also have left traces of a Hittite



FIG. 21. (From King, *Bronze Reliefs*, pl. L.)

language in some of the districts occupied. The question of language, to which I will return later, is therefore of exceptional interest, and concerns not only the Orientalist, but also the Greek scholar, since names of the Asia Minor type are found in Greece and the islands. All such names may therefore be Aegean, and the aborigines of Cappadocia (the Hittites represented by the Egyptian portraits) as well as some of the Asia Minor races *may* also belong to the Aegean stock. When the Cretan tablets are published, we may hope to decipher them and get some light.

Now when we come to the later period (which I called the Southern period) of Hittite history, we find them withdrawn from western Asia Minor, and tending south and east. I sug-

gested that this movement was due to the development of trade with Mesopotamia, which made the centre at Carchemish more



FIG. 22. (From King, pl. XXXII.)



FIG. 23. (From King, pl. LXXIV.)

valuable than the older capital. Such a development of trade may well have been occasioned by the decline of the Cretan



power (say about 1200) and the consequent loss of trade with the Aegean on the coast of Asia Minor. But the Hittite retirement eastward may have been hastened by pressure from the west, caused by the immigration of colonists from the Aegean. At any rate in this period they appear to have followed the same policy, absorbing the petty states of Muški, Tabal, Ƙue, Gamgum,



FIG. 24.

Mitanni, &c., on the east and south, as they had previously absorbed the tribes on the west.

To these two periods the two kinds of Hittite writing roughly correspond — namely, the cuneiform to the Northern and the hieroglyphic to the Southern period. This can hardly be accidental. It may be argued that the hieroglyphic writing was suitable only for monuments, while cuneiform was more naturally used for literary purposes. That is no doubt true, yet at Boghaz-keui

only one inscription<sup>1</sup> in hieroglyphics has been found, and on the important sculptures both there and at Eyuk there is not a single continuous line of hieroglyphic text. The same is true of the scattered monuments on the west, at Doghanlu-keressi, Karabel, Sipylus, which we took to be of this period. All or most of these have small groups of hieroglyphic signs, placed like cartouches or monograms near figures of gods or persons, evidently to indicate their names, but there is never any descriptive text in hieroglyphics, as on the sculptures found on southern sites. On the other hand, at Carchemish for instance, where there are many hieroglyphic inscriptions accompanying sculptures, hardly any cuneiform has been found. It may be that cuneiform tablets have perished or that their hiding-place has not yet been discovered, but only the merest scraps<sup>2</sup> of cuneiform monuments have survived, though that form of writing is perfectly well suited for monumental purposes. We must therefore conclude that, as far as the Hittite language was concerned, the hieroglyphic system of writing eventually ousted the cuneiform, not vice versa, and the first beginnings of it are to be seen in the monograms at Boghaz-keui. These were no doubt originally symbols, such as we find on Sumerian seals, as it were coats of arms, but their component parts are identical with signs found in the hieroglyphic inscriptions elsewhere. The elements were already in existence in the fourteenth century B.C. (introduced by the conquering race?). The system of writing seems to have been developed later with the spread of Hittite power southwards.

Does this difference of writing imply a difference of language? We cannot yet answer the question conclusively and must investigate both sets of texts independently.

First with regard to the cuneiform language of the early period, this may be the language either of the conquering race, in which case it should be Caucasian, or (if my view is correct) more probably that of the aborigines, in which case we might hope for some light from one or other of the languages of western Asia Minor. It is a faint hope, however. Of the inscriptions in western Asia Minor now extant, all are nearly 1,000 years later

<sup>1</sup> So much defaced that it is not even certain that the writing is Hittite.

<sup>2</sup> I copied seven, but none contained a complete word. Prof. King thought that the character was of the time of Nebuchadnezzar II. If so, they would date after his capture of Carchemish in 604, and would not affect the question. In one case a line of cuneiform had apparently been added to an earlier Hittite monument.

than the Boghaz-keui tablets, and much had happened in the meantime. The Phrygian<sup>1</sup> language is Indo-European of a barbarous kind, the new Phrygian inscriptions being more influenced by Greek than the old. They give the impression of an imported language (that of the Briges?) grafted on to a native stock, which it finally killed, but the character of the original stock is very difficult to discover. Winckler suggested that it is Moschian (whatever that may have been), identifying Mita of Muški with the Phrygian Midas. At any rate, the Phrygian inscriptions, as we have them, are not likely to be of much value for comparison.<sup>2</sup>

The Lydian language has only lately become accessible, through the publication<sup>3</sup> of some of the inscriptions found at Sardis by the American expedition. Among them is a bilingual, Aramaic and Lydian, eight lines of each, which (though the Aramaic is difficult to translate) gives the meaning of about twenty words. It is dated in the tenth year of an Artaxerxes, therefore at least as late as 455 B. C., and the other inscriptions are apparently of the same period. They are all in a Greek alphabet, with some additional letters of which the values are not all ascertained. We know that Lydia and Ionia mutually influenced one another, and the use of the Greek alphabet is an instance of this. But there is no trace of the Greek language in Lydian, as there is in Phrygian. It seems to be quite un-Indo-European.<sup>4</sup> As Hittite monuments are found in Lydia, it is not impossible that the two languages may turn out to have some affinity when we have studied them more. Greek tradition always connected Lydia with the East.

The Lycian language has long been accessible in inscriptions,<sup>5</sup> though little is really known of it. The inscriptions are not much earlier than about the fifth century B. C., and are written in a Greek alphabet, with additions. There is no trace of Greek in the language, though there are Greek bilinguals. Though the

<sup>1</sup> See Calder in *JHS* 1911, p. 161, and references there to Ramsay. Also Arkwright on Lycian and Phrygian names in *JHS* 1918, p. 45.

<sup>2</sup> The fact that Phrygian inscriptions (in Greek characters, like all the rest) were found at Eyük (Chantre, *Mission en Cappadoce*, p. 165) does not affect the question.

<sup>3</sup> By E. Littmann, *Sardis*, vol. vi, pt. i, of Publications of the American Society for the Excavation of Sardis (1916).

<sup>4</sup> There may perhaps be some connexion with Etruscan. Cf. Hdt. i. 94.

<sup>5</sup> In the excellent Corpus of Kalinka (*Tituli Lyciae*, Vienna, 1901). The best work on the language is that of Torp and Arkwright.

language has not yet been satisfactorily assigned to any family, it is safe to say that *au fond* it is not Indo-European.<sup>1</sup> But no Hittite remains are known in Lycia, and probably the conquerors never penetrated into that mountainous and difficult country. Moreover, Lycia looks west, and is more naturally connected with Crete, via Rhodes, so that its language is likely to be Aegean and would have an affinity with (cuneiform) Hittite only if the latter is an aboriginal language of Asia Minor. In Homer's list of the Greek forces (*Il.* ii. 654) Rhodes comes next to Crete, and Aegean remains have been found there. The elder Sarpedon of Lycia was a brother of Minos, though his grandson joined the Trojans, the former allies of the Hittites. When the linear tablets from Crete<sup>2</sup> have been published and deciphered, it may be possible to affirm or deny their linguistic connexion with Lycia, or other parts of Asia Minor. For the present, at any rate, the Lycian language is not likely to give much help in elucidating Hittite.

Of other languages in western Asia Minor, we have only the slightest traces, which may be disregarded for practical purposes. Thus the most probable affinity in the west with Hittite (the language of the conquering race) is to be sought in Lydian, which has still to be deciphered. Or if cuneiform Hittite be an aboriginal language of Asia Minor, it may be connected with Lycian, and belong to the Aegean group.

On the east the prospect is not much more promising. We may from the first exclude any comparison with Semitic. In the Northern period the Hittites used the Semitic-Babylonian language largely, and early 'Cappadocian' tablets in Semitic are common, though the language and writing are peculiar. Even in writing the native language (in cuneiform) they used Semitic ideograms and words, and the same is true of hieroglyphic Hittite, if my decipherment is correct, but this did not affect the structure of the native language. We have to look elsewhere for a real linguistic connexion. The most obvious is with the people of Mitanni, with whom the Hittites were in close contact, as we know from the Boghaz-keui documents. It has long been recognized that the personal names<sup>3</sup> of the two peoples contain many

<sup>1</sup> Against Savelsberg, *Beiträge zur Entzifferung der lykischen Sprachdenkmäler* (1874).

<sup>2</sup> See Evans, *Scripta Minoa* (1909), p. 38, &c.

<sup>3</sup> Collected by A. T. Clay in *Personal Names . . . of the Cassite Period* (1902), and Tallquist, *Assyrian Personal Names* (1914), in the *Acta Soc. Sci. Fennicae*, with a good introduction.

elements in common, and this implies either a racial or linguistic connexion. Of the actual language of Mitanni we possess only one long letter in the Tell-el-Amarna collection, which has been very much discussed. The most recent treatment of it is by Bork,<sup>1</sup> who assigns the language to the Caucasian group (Georgian). It must be admitted that at first sight the language does not seem to bear much resemblance to cuneiform Hittite. Perhaps the ruling class (to which the extant names mostly belong) in Mitanni was racially and linguistically different from the bulk of the population, to which the language of the letter belonged (see below, p. 45).

Another people with which the Hittites were in very close relations is the Kassite.<sup>2</sup> During all the Boghaz-keui period Kassite kings ruled Babylon, and at times had a 'sphere of influence' in north Syria. The Boghaz-keui records show that diplomatic communications passed between the two kingdoms, and it is generally assumed that the Hittites had a share in introducing the Kassite dynasty to Babylon, as they certainly had in maintaining it there. Whether this implies any racial affinity is another matter. Of the Kassite language all we know is comprised in one short vocabulary,<sup>3</sup> compiled evidently with the object of explaining Kassite names to Semitic-speaking people. The value even of this has been questioned (I think too severely) by Hüsing.<sup>4</sup> There are also many Kassite names,<sup>5</sup> but there is no native literature. The language is not Semitic and may be connected with Elamite, but the opportunities for comparison are too few to be decisive.

Finally there is Vannic. This is the language of the inscriptions, written in cuneiform, found round Lake Van. They belong to kings of that region in the ninth and eighth centuries B. C., so that they are contemporary with the southern Hittite period. The language was first deciphered, in a brilliant manner, by Sayce,<sup>6</sup> and work has since been done on it by Belck.<sup>7</sup> It was certainly not Indo-European, nor Semitic, and was eventually supplanted by the Aryan language of modern Armenia.

These are all the languages we know with which the Hittites

<sup>1</sup> 'Die Mitannisprache', in *Mitteilungen d. VAG* (1909).

<sup>2</sup> See Delitzsch, *Die Sprache der Kossäer*, 1884.

<sup>3</sup> Delitzsch, *op. cit.*, p. 25, and Pinches in *JRAS* 1917, p. 101.

<sup>4</sup> In *Memnon* iv (1910), p. 22.

<sup>5</sup> Clay, *op. cit.*

<sup>6</sup> *JRAS*, xiv (1882), p. 377.

<sup>7</sup> Die Kelischinstele, in *Anatole* i (1904).

can have come in contact. They certainly are not very helpful. It remains to consider what is known of cuneiform Hittite itself. Among the Tell-el-Amarna letters there are two, from and to Arzawa,<sup>1</sup> written (in cuneiform like the rest) in a strange non-Semitic language. It is still quite uncertain where Arzawa was, but evidently this is its native language. In 1907 a tablet obtained at Yuzgat near Boghaz-keui was published by Sayce and Pinches,<sup>2</sup> and turned out to be in the same language. Some small fragments of a similar kind were found by Chantre.<sup>3</sup> The discovery of the archives at Boghaz-keui with their native documents showed that the language of all these was really Hittite, i. e. the language of the head-quarters of the early Hittite confederacy. Many efforts have been made to elucidate the Arzawa letters. Owing to the use of ideograms and determinatives in cuneiform writing, the occurrence of some Semitic loan-words, and their formal style, it is possible to get some idea of the general sense of them.<sup>4</sup> In trying to go farther, Knudtson, in his edition with Torp and Bugge, advanced the astonishing theory that the language was Indo-European, but afterwards gave up the view. It has since been revived by Hrozný with whom Ed. Meyer (rather doubtfully) agrees.<sup>5</sup> Independently (having heard of Hrozný's view but not seen his article) Holma<sup>6</sup> produced some remarkably ingenious comparisons. It is on the still unpublished texts from Boghaz-keui, which he has studied at Constantinople and Berlin, that Hrozný bases his argument, and until these are accessible, we cannot form a just judgement of his results. Yet

<sup>1</sup> Nos. 31, 32 in Knudtson's ed. (1907). Also edited by him (*Die zwei Arzawa-Briefe*, 1902) with Torp and Bugge. Some corrections by O. Schröder in *OLZ* 1915, p. 231.

<sup>2</sup> *R. Asiatic Soc. Monographs*, vol. xi, 1907.

<sup>3</sup> *Mission en Cappadoce* (1898), p. 40.

<sup>4</sup> Especially of no. 31, from Amenophis to Tarhundaraba. No. 32, written from Arzawa, is more difficult, perhaps as being more idiomatic.

<sup>5</sup> *MDOG*, no. 56 (Dec. 1915). Hrozný has since published *Die Sprache der Hethiter*, pts. i, ii (1916-17). The theory has been approved by Cumont in *Comptes-rendus de l'Acad. des Inscr.* 1917, p. 119; by E. Brandenburg in *Frankfurter Zeitung* for Jan. 20, 1916; by an anonymous writer in *Bergen Morgenbladet*, Oct. 21, 1916; and in *Times Lit. Supp.*, Apr. 3, 1919; by S. J. Crawford in *JRAS* 1919, p. 1; and C. J. S. Marstrand in *Videnskapsseelskabet's Skrifter* (Christiania), 1919. Böhl, in *Theol. Tijdschrift*, 1916, pp. 66 and 305, thinks the language is mixed. Weidner, *Studien zur hethitischen Sprachwiss.*, i (1917), p. 33, is against Hrozný: so Bork in *OLZ* 1916, p. 289; Herbig in *Deutsche Lit.-ztg.* 1916, p. 421; King in *Journal of Egyptian Archaeology*, 1917, p. 190.

<sup>6</sup> 'Études sur les vocabulaires . . . de Delitzsch', in *Journal de la Soc. Finno-Ougrienne* (1916).

the question is too important to be dismissed without consideration. Although they lived surrounded by non-Indo-European peoples on all sides, Hrozný contends that the Hittites spoke not only an Indo-European language, but one belonging to the western branch, more akin to Latin than anything else. He begins by observing that Hittite has a present participle in *-an* or *-anza*, with oblique cases formed on a stem in *-ant-*, as *da-a-an* = 'giving', plural *da-an-te-eš*. In Arzawa i (Knudtzon no. 31) he takes *hûman(za)* as nominative = 'being complete', and finds elsewhere a genitive *hûmandaš* (cf. ferentis), dative *hûmandi* (ferenti), accusative *hûmandan* (ferentem), ablative or instrumental *hûmantid(-ed)* (cf. Oscanpraesentid). There are six cases, at least in the singular, as above, with a locative in *-az*. Now if this declension were established, it would settle the question at once. But one naturally asks, is *hûman* a participle, and are the other words case-forms of it? We certainly want other instances of the form. In Arzawa i, as Knudtzon pointed out, according to analogy it should correspond to Bab. *danniš*, adverb, 'very much'. In a vocabulary (see below) it is rendered by the noun *kellatum* 'entirety'. Of the alleged case-forms, none occur in the Arzawa letters, so that we must wait for more texts before deciding whether the explanation fits the facts. On the other hand the form *hûmanda*, which is not in Hrozný's scheme, does occur in Arz. i, 26, and terminations like *-andu*, *-anta* are common in Arzawa, but are not in the scheme. This series, or some of it, seems to suggest rather a connexion with the termination *-vða*,<sup>1</sup> &c., common in Asia Minor place-names (locative?, cf. Stamboul = *εις τὴν πόλιν*), and so to be not Indo-European.

Very important, if correct, is his list of personal pronouns: 1st person singular *ug* or *uga* = *ego*: dative *ammug*<sup>2</sup> = *ἐμοίγε*; plural *anzás* = *nos*, *uns*.

2nd person *zig*, *ziga* = *σύγε*; accusative-dative *tug* = *dich*; plural *šumáš* = Persian *šumâ*.

No doubt the particle *γε* may be regarded as pronominal, but if it is to explain the *g* in *tug* as well as in *zig*, we have still to account for the change from nominative *zi-g* to the oblique case *tû-g* which should correspond to *σέγε* or *σοίγε* (for *τῆγε*).<sup>3</sup> We must then assume that *tug* is for *tivig*, and that phonetic decay was

<sup>1</sup> Cf. Kretschmer, *Einleitung in die griech. Spr.*, p. 294.

<sup>2</sup> Why not make it plural and compare Aeolic *ἄμμε*?

<sup>3</sup> Not *τφοι-* as we used to be told. The *ο* merely represents the digamma (= *w*).

more advanced in Hittite in 1400 B. C. than in Latin of classical times. Why too should the first letter be *z* in the nominative and *t* in the dative, when in each case it stands for original *t*? The change may possibly be due to special phonetic laws in Hittite, but it requires corroboration. Further, since the Arzawa documents are *letters*, we should expect the 2nd person<sup>1</sup> of the pronoun to occur, but neither *zig* nor *tug* is found.

The comparison of *šumāš* with the modern Persian is unfortunate, for *شما* is worn down from an original Indo-European *šusme-* (Sanskrit *yusma*). The essential part is *yu-* (as in Zend) which Hittite would thus have already lost in 1400 B. C. Moreover, in a vocabulary (see below) 'for your benefit', *ana itikunu*, is translated into Hittite as *šuraš enzan kuššan*, where *šuraš* can hardly be a mistake for *šumāš*.

To each of these pronouns Hrozný finds a corresponding possessive: *ammêl* = *ἐμός*; *tuêl* = *tuus*; *anzêl*, *our*; *šumêl*, *your*. This regularity of formation would be very convincing, if it could be proved. *Anzel* occurs in the vocabulary for 'our' or 'us', but 'you' or 'your' is *šuraš enzan*, as already mentioned. *Tuêl* occurs in Arzawa i, where it may perhaps mean 'thy'.

Lastly, Hrozný makes out the present or future tense of the verb thus (from infinitive *iauwar* 'to make', &c.): *iyami*, *iyasi*, *iyazi*, *iyaueni*, *iyatteni*, *iyanzi*. Cf. for the formation *ἴημι*, *ἴης*, *ἴησι*, &c. Here again if these can be shown to be all forms of a verb, we need no further evidence. Only two of them occur in the published texts (Yuzgat, obv. 7, rev. 39), where Dr. Pinches gives reasons for thinking them to be nouns. Moreover, the forms *iya* (Arzawa ii, 3, 23) and *ias* (Yuzgat, obv. 28) require explanation.

These are only a few instances of Hrozný's arguments in support of his view, but they are crucial instances, and it must be admitted that they are not conclusive. At the same time it would be unfair to reject the view without having all the evidence before us. The Indo-European theory does not enable us to translate the Arzawa letters, nor has Hrozný translated more than isolated sentences from his texts. We must wait for the publication and explanation of continuous texts.

He has been influenced evidently by a very remarkable discovery of Winckler's among the Boghaz-keui documents. A treaty was there found, between Hatti and Mitanni, in which both sides invoked their gods. On the Mitanni side these in-

<sup>1</sup> The 1st person is supplied by *šamši*, 'my majesty'.



clude, according to Winckler's reading,<sup>1</sup> Mithra, Varuna, Indra, and the twins Nāsatya, pure Aryan gods among a number of strange names. This does not prove that either the Mitanni race or language was Aryan. No one would be bold enough to say that of their language. But it does show that in some way Mitanni was influenced by Aryans. Winckler conjectured that this influence was due to the Ḫarri (a people mentioned in the Boghaz-keui documents) whose name he identified with Arya. Hrozný on the other hand has found traces of the Ḫarri language, which he says is certainly not Aryan. If at this early date (1400 B. C.) Aryans were on the move, from the north perhaps, along the road which eventually brought them to India, they may in the course of their migrations have descended upon Mitanni and formed a ruling caste there. We might thus account for the Aryan appearance of some personal names in Mitanni.

Another point may be mentioned. These god-names have two interesting suffixes: *ilāni mitraššil . . . ilāni našatīyanna*. [Note also the admixture of Semitic.] Now farther east, in Turfan, Le Coq found some fragments of Brahmi writing, in an unknown language which has been called Tokharian. This has been deciphered by Sieg and Siegling,<sup>2</sup> who find that it is Indo-European with the same definitely *western* character as Hrozný now claims for Hittite. It is of course very much later. Among the peculiar terminations are *-assil* for the comitative case, and *-añ<sup>6</sup>* for the plural. One cannot help comparing<sup>3</sup> these with the forms in the Boghaz-keui treaty. We should then translate 'the gods with Mithra . . . the gods Našatya'. One cannot feel sure of this explanation, especially as the terminations<sup>4</sup> do not seem to occur elsewhere in the few published texts. It is a curious coincidence that the word for 'twin' (*išuwaniḍwatar*) is given in the vocabularies, which were drawn up apparently for religious purposes, and the Nāsatya were twins.

Granting then that Indo-Europeans were in the air, in contact directly or indirectly with the Hittites, it would not be surprising to find traces of their influence on the Hittite language, just as we find evident traces of Semitic influence. We can

<sup>1</sup> Contested by W. E. Clark, in *Amer. Journ. Sem. Lit.* 1917, p. 261. But the names cannot be seriously questioned.

<sup>2</sup> *Sitzungsb. d. Preuss. Akad.* 1908, p. 915.

<sup>3</sup> So L. von Schröder independently in *Vienna Or. Journ.* 1908, p. 348.

<sup>4</sup> Or *-aššil* may be connected with the suffix in such names as Ḫattušil, Biyaššili (*MDOG* 35, p. 39).

detect the Semitic because we know the contemporary forms. We do not know the contemporary forms of Indo-European or Aryan. But even if we admit the possibility of such influence, it still seems unlikely that the *fund* of the language is Indo-European. What then is its fundamental character? A few indications only can be noted here:

Names in Hittite are often compounded with *Tark(u)*, e.g. Tarkunazi, Tarkulara, Tarkundaraba, Tarkutimme, and this element is no doubt rightly assumed to be either a god-name, or the native word for 'god'. It is found in similar combinations, in Cilician names, as *Ταρκύμβιος*, *Ταρκυνδβέρρας*, *Ταρκόνδημος*. The language of Cilicia is quite lost, but next to it is Lycia, where we have a large number of native inscriptions. In these the element *Trk* (Trq, Trh) is common, in words which must be names, as *Τροκόνδας*, and in others which are probably not. On the stele of Xanthus such a name occurs also on the western face, which seems to be in a non-Lycian language, perhaps Carian. Again, Hittite names occur in the form *Ḫattušil*, *Muršil*.<sup>1</sup> In the newly found Lydian inscriptions, patronymics (and adjectives) are formed with *-l* (as in Etruscan). Other names, like *Arandaš*, *Arnuantaš* (which Hrozný would call participial) would seem to be related in some way to place-names in *-avda*, *-avθ-*, common in Asia Minor. On the evidence of the names then we should expect the original basis of Hittite (i. e. the cuneiform language of the early period) to be connected with one or more of the languages of Asia Minor, making allowance for difference of date. But each language must be interpreted from itself before we compare them.

For cuneiform Hittite we have some very important help, and we only await the publication of more, and more varied, texts in order to make use of it. Mention was made above of vocabularies. These are on tablets found at Boghaz-keui, and are compiled in the same manner as those we know in Babylonian. They were published in 1914 by Delitzsch.<sup>2</sup> They are in three columns, Sumerian, Semitic, Hittite, all in cuneiform, but very fragmentary, so that frequently one or more columns are missing and the equivalents lost. Still they supply some (and, in fact, the only) positive evidence as to the nature of the language. Hrozný considers that they support his view, and Holma

<sup>1</sup> Cf. *Μυρσίλος*, the 'Greek' name of *Κανδαύλης*, which Herodotus (i. 7) explains as meaning 'son of Μύρσοι'.

<sup>2</sup> In the *Abhandlungen d. Preuss. Akademie*.

examines them with great ingenuity in the same sense. Regarded without prejudice, they seem to me to show a language as un-Indo-European as it is un-Semitic.

To sum up: The name Hittite was vaguely applied by the Assyrians to peoples of various states at various times. The main stock appears to consist of two strata, which may imply two distinct races. The linguistic affinities of the earlier stratum may perhaps be sought in western Asia Minor, those of the later stratum more probably in the east and south.

The problem of the hieroglyphic inscriptions is quite different from that of the cuneiform texts. In the next lecture I propose to show what little can be done in the way of deciphering them.

### LECTURE III

BEFORE dealing with the decipherment of the Hittite hieroglyphic inscriptions (as distinguished from cuneiform Hittite), I must beg you not to expect too much. We can only begin with a few steps, tentatively feeling the way. At first we must guess, and the results can only be convincing when they corroborate one another. You will remember that in reading the cuneiform writing Grotefend made his first conjectures, most of them correct, in 1802, but it was more than thirty years before any real advance was made on them, and many years more before the whole system was established. So perhaps I need not apologize for having but few results to offer. I cannot translate, with certainty, a single inscription, nor even show conclusively the nature of the language, but so much has been wrested from the unknown during the last century, that we need not despair of solving this greatest of linguistic puzzles. It may justly be called so, for this reason. In every decipherment two things have to be considered, first, the value of the signs, and secondly, the nature of the language. Now in deciphering Egyptian, Cuneiform, Cypriote, the values of the signs were unknown, but as soon as some of them were correctly established, a language began to emerge, which in each case was found to be allied to some well-known language or group. In Lycian, Lydian, Etruscan, the alphabet was to a great extent known, but the language has not yet been satisfactorily proved to belong to any group. In hieroglyphic Hittite, so far, we have two unknown quantities, a system of signs which we cannot read, and a language which we do not recognize. It is, therefore, a problem worth solving, but it requires some optimism.

Attempts at decipherment, differing entirely in their results, have been made by many scholars—by Conder, Peiser, Jensen, Campbell Thompson, and others. Some of these failed owing to the inaccuracy of early copies of the inscriptions (see Lecture I), others owing to a fundamental defect of method. The only real advance so far is due to Sayce, who has worked indefatigably at the baffling problem for forty years. It is to his sagacity, his

wide knowledge and keen interest in every branch of archaeology that we owe the recovery<sup>1</sup> of the first small bilingual inscription.

This is generally called the boss of Tarkondemos, but is probably a seal, of silver, with a cuneiform legend round the edge, and some Hittite signs and a figure in the middle (fig. 25). The help it gives is very small, and one could almost imagine that Tarkondemos was laughing at us when he had it made, so disappointing and elusive is it. First the cuneiform is a difficulty. It has been read Tar-ku-u-tim-me ŠAR MAT ERI-me-e, and translated 'T. king of the land of Erime', with various speculations as to the unknown name Erime.

As a matter of fact, I believe that the legend begins with *Me*. A workman so skilful as the maker of this seal would not have



FIG. 25.

miscalculated his space, nor, if he had, would he have separated the *Me* from the word to which it belonged, and attached it to the beginning of the legend. We must, therefore, read Me-e Tar-ku-u-tim-me. This is good Sumerian. Dr. Langdon tells me the style of the writing is that of the Kassite period, probably of the thirteenth century B.C., though there is always a possibility that it is archaistic. (Note the sign for *tim* or *dim*, a crucial character, and cf. Amiaud No. 6 a<sup>4</sup>). This is also the opinion of Hilprecht,<sup>2</sup> who dates it 1300–1200 B.C., i. e. late Kassite period, though I do not accept his reading. Dr. Langdon has also shown<sup>3</sup> that Kassite seals were often inscribed in Sumerian. Thus we are confirmed in reading Me-e Tar-ku-u-dim-me LUGAL KUR

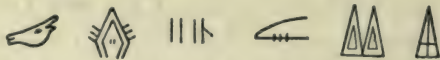
<sup>1</sup> The account is well worth reading in Wright's *Empire*, ed. ii, p. 163. It is not a forgery.

<sup>2</sup> *Assyriaca*, p. 114.

<sup>3</sup> In *Revue d'Assyriologie*, 1919, p. 69.

ERI—'I am T. king of the land of the city'. The reason why this reading has not been settled before is that the strange expression 'king of the land of the city' was unknown, but the texts found at Boghaz-keui have shown that this is a characteristic expression in Hittite.





Now take the central legend, which is written twice, once on either side of the figure. The only order in which they can be read the same on both sides is







and the values, if they are the same as in the cuneiform, would be





FIG. 26. (From Hogarth, pl. A. 11 b.)

 = *Tarḫu*,  = *dim* || | = *me*  = 'king'  = 'country'

 = 'city'. This gives the values of two common ideograms—'country' and 'city', but we do not know in the least how they were pronounced. A single one of them spelt out would be far more useful. The only probable phonetic values are || | = *me* and  = *tim* (*dim*). The other two signs  *Tarḫu* and  'king' are uncommon.

The only other bilingual, called the seal of Indilimma, gives no help, as its reading is uncertain and its interpretation doubtful.






In addition, Sayce has pointed out the sign for 'god', which

in sculptures is found over figures evidently representing gods, and the usual sign for king, , which is often confused with that for city .








If we look carefully at some of the inscriptions (figs. 26, 27), several



FIG. 27. (From Hogarth, pl. A. 6.)

facts are evident at once. The figure is certainly the beginning, and the characters face the beginning. But in the next line they face the other way, and so on. The writing is, therefore, boustrophedon. Also the lower lines in  and  and the side-stroke in  slope in the direction of the writing. The sign  seems to divide words, though it is irregularly used. The sign  is

perhaps used to mark an ideogram. Putting all this together, we have as preliminary results:

	= Tarku?		= city.
	= ti(m)?		= king.
†	= m(e).		= god.
	= king.	∩	= word-divider.
	= country.	∪	= mark of ideogram?

It is not much to begin with.

Now since we know only these values, and have no knowledge of the language of the inscriptions, our next step had better be to identify names if possible, and from them to collect values of other signs. Of course it is difficult to say which group out of an unintelligible mass of signs, is a name. We must guess, as carefully as possible. As a precaution, which has been neglected by most scholars, it is very important to determine the grouping of the inscriptions, and, if possible, their dates. Roughly, the following seem, on internal evidence<sup>1</sup> to be the chief groups: (a) M 15 (= A 1b); (b) M 9, A 2, A 3, A 11; (c) M 11, A 6, A 7; (d) M 16, A 1a?; M 2-4, M 6, M 21, M 22, M 25, M 52, A 7?; (e) M 31, M 32.

As to the dates of the several groups, there is very little evidence. Yet this is a very important question if we are to know what names are possible in a given inscription, for obviously we shall not be likely to find the same persons or places prominent in 1400 as in 800 B.C. It has sometimes been assumed that the incised inscriptions are later than those carved in relief, but the contrary might equally well be maintained. As a matter of fact this difference of style is probably not a necessary criterion, and inscriptions may be of the same date, though differing in this respect. From what was said above (p. 35) it would follow that the hieroglyphic inscriptions belong to the Southern or later period of Hittite history. On artistic grounds Garstang dates the carvings which he found at Sakche-geuzi about 900 to 850 B.C., and considers the lions at Mar'ash to be of the same period. These again belong to the same group (d) as the Hamath inscriptions, and cannot be far removed in date from those of

<sup>1</sup> Especially the recurrence of the same combinations of signs, some of which are names.



Carchemish. That is to say, they may all belong to about the time of Shalmaneser III, c. 850.<sup>1</sup> Cf. also the figure of the god on M 2 (fig. 13) with that at Zenjirli (fig. 28). As mentioned above (p. 8) hieroglyphic texts are not found at Boghaz keui, and it may



FIG. 28.


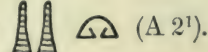


reasonably be doubted whether this system of writing had been developed in the earlier period, beyond its first rudiments. It is curious that, if this is correct, the system was developed at about the same time as the so-called Phœnician alphabet. Was

<sup>1</sup> But e.g. M 31, 32 are probably later, and M 7, 46 later still.

it also alphabetic, or partly so? Was it one of several competing attempts to invent an alphabet, of which the 'Phoenician' survived on its merits?

The next preliminary step is to separate groups of signs in the texts. This also has not been sufficiently done by previous investigators. Yet it is manifest that no progress can be made if we divide the words wrongly. Some help is given by the word-divider  $\text{IC}$ , but as this is not consistently used, and in some inscriptions is not used at all, we have to rely mainly on a laborious inspection of the texts. After all this has been done (and the results of it will appear incidentally as we proceed) we may try to identify names among the various groups of signs.

Since we know that  $\text{A}$  means city, we shall be justified in conjecturing that a group of signs standing immediately before or after it may perhaps be a place-name. It must be a group which recurs, and which can be clearly detached. Or if we find the sign  $\text{K}$  'king' in such a connexion we may presume that the group is either a place-name or a personal name. One of the most probable of such names is a group which occurs repeatedly thus:

	(A 11a <sup>1</sup> , 11 b <sup>1</sup> , M 9 <sup>1.2.4</sup> , cf. M 10 <sup>1</sup> , 11 2.3, A 2 <sup>3</sup> ).
	(A 2 <sup>1</sup> ).
	(A 2 <sup>4.6</sup> ).
	(A 3 <sup>1</sup> , 11 a <sup>4</sup> ).


That is to say, ten times unmistakably, and four times probably. It is found, usually in the same position, in the Carchemish inscriptions, and, as far as I know, not elsewhere. Sayce has taken it to be the name Carchemish, and there can be no doubt that he is right. Then how are we to assign the values? In cuneiform documents the earliest form<sup>1</sup> of the name is Karkamisu (2000 B. C.), later Kargamis (-miš), and from the ninth century Gargamis (-š). The Egyptian form is Krkmš or Kṛkmš. There is, therefore, not much variation in the form.




As we do not yet know whether the Hittite system is syllabic

<sup>1</sup> See Hogarth, *Carchemish*, p. 17.








or alphabetic, we had better for the present treat the signs as purely consonantal, and divide as follows:






					
K	R	G (K)	M	Š	or
K R	G (K)	M	Š	?	

Since we already have reason to make ||| = *m*, the latter is more probable, and, moreover,  is such a very common sign that it can hardly be always š. Thus we have three more values:

	= <i>g</i> or <i>k</i> .
	= š.
	= <i>kr</i> (i. e. <i>kar</i> ) or <i>gar</i> .

The value *kar* is confirmed by another name. It occurs in only one other group:

				 (M 1, A 11 a <sup>3,6</sup> )
_____				 (A 11 b <sup>4</sup> , A 11 c <sup>3</sup> )
_____				 (A 11 b <sup>5</sup> ).

This I take to be the god-sign followed by Karduniash, the Kassite name for Babylon. I will speak of this later. Returning to Carchemish, some other details may be gathered. It is followed by the king-sign, with or without . Hence it appears that here (and I believe always) the king-sign (and similarly the god-sign) is not a determinative, but an ideogram, and the groups mean respectively 'of Carchemish, king', and 'god of Karduniaš'. This is really a very important point, because if it is correct the word after  is not necessarily the name of a god, but may be a place-name—and so with . Secondly, the sign  at the end of Carchemish is then a formative syllable, used (as other instances show) especially with names of countries or peoples, to mark the genitive or to form a gentilic adjective. This again is important because, in conjunction with the king-sign, it will enable us to pick out national names. Thirdly, the  is, I think, in all three forms the phonetic complement of king, but in one form it may belong to Carchemish, so that

we have either 'of Carch. King' or 'Carchemisian King', meaning the same thing. The genitive or adjective stands before the governing noun. The language is therefore not Semitic, for in no form of Semitic, even when the cases existed, can you ever have that order.

The  $\Delta$  has been taken by Sayce to be  $\xi$ . If the three forms of Karduniaš are to be pronounced the same, and are not different cases, this value is confirmed, for  $\Delta$  =  $\text{𐎡}$  =  $\xi$  =  $\text{𐎢}$ .

As to Karduniaš, if the identification is right, the values must be assigned thus :

$\text{𐎠}$  = *kar.*

$\text{𐎡}$  = *d(u).*

$\text{𐎢}$  = *n.*

$\Delta$  =  $\xi$ .

The second character,  $\text{𐎡}$ , is found usually after the sign  $\text{𐎢}$ , either as a separate group, preceded by  $\text{𐎠}$ , and therefore as a god-name (or place-name), or as a group in combination with other characters following it (as in M 2<sup>1</sup>), and therefore as the first element in a personal name compounded with the god-name. This god is so frequently mentioned that he must be the chief god, or one of the chief gods, of the authors of the inscriptions. Sayce takes the name to be Sandes, the god of Cilicia. Others propose Tešub, the god of Mitanni. But though this occurs often enough in compound names, it is apparently never used as the first element.

On M 2, where  $\text{𐎢}$   $\text{𐎡}$  occurs, there is a portrait of a god who can only be Rammanu or Adad (Addu). The latter can form the first element in compound names. We may therefore take  $\text{𐎢}$  to be the ideogram of Addu (since it occurs alone), and  $\text{𐎡}$  as its phonetic complement. (It is not impossible, however, that we should read San-du, which also occurs as the first element in names. In cuneiform Hittite the god-name occurring most often is written ideographically, <sup>11</sup>IM, which is read as Tešub, or Addu, or Ba'alu [cf. Gressmann in *Beiheft ZATW*, 33, p. 191], or <sup>11</sup>U-ub, which must be Tešub.)

The third sign,  $\bigcirc$ , elsewhere seems to be some sort of *k*, and one naturally thinks of the Carduchi, living on the borders of Armenia, near the source of the Tigris.<sup>1</sup> But the *k* or *kh* does not seem to occur in cuneiform, and must be due to the Greeks.<sup>2</sup> They occupied much the same country as the  $\text{Ḫutu}$  in cuneiform (not in TA), and that is the name which would probably have been used for them in Hittite. Moreover,  $\Delta$  would not be the natural termination for a people, though it would be for a place-name. Another difficulty is the lack of any vowel-sign for *-ia-*. But an early form of the name (e.g. in



FIG. 29. (From Messerschmidt, pl. 21.)

Sennacherib's copy of the seal of Tukulti-ninib) is  $\text{mātu Karduniši}$ .<sup>3</sup> We might therefore transcribe the Hittite form as *Karduniš*.


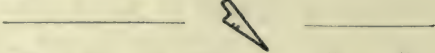
Now some more names.

There are two inscriptions by the same person, on lions, found at Mar'ash, in cuneiform *Marḫašu* (fig. 29). In both of them occurs a group (found nowhere else) which I take to be the name of the city. In M 52<sup>1</sup> it is preceded by the ideogram of city. The values will then be distributed as follows:

<sup>1</sup> Cf. Xen. *Anab.* iii, 5, 15, &c., and Strabo, xvi, 747, 750.

<sup>2</sup> But cf. Pliny, vi, 44 'Carduchi quondam dicti nunc Cordueni'.

<sup>3</sup> Cf. also the form *Ginduniša*. This supports Hüsing, who (in *OLZ* 1906, 664) rejects the usual explanation 'City (&c.) of Duniaš', a god otherwise unknown, and proposes 'Sea-land', assuming an Elamite word, *duni* = sea.

M 52<sup>1</sup>   
 M 21<sup>2</sup>   
 M U R K A Š Š

The final sign, which is apparently a grammatical suffix, will be considered later.

We thus gain a new sign for *m*, a sign for *r*, two signs for *k* (or similar guttural, see below), and we find that the vowel  $\Pi$  may be omitted. (I will discuss the two vowels later.)

The new sign for *m* occurs in a group in the great Carchemish inscription (A.6<sup>3</sup>):



which is particularly illuminating. The ideogram of *king* divides it into two groups, each ending with  $\uparrow$ , which was

mentioned before as forming the gentile adjective or genitive. We have therefore two national names here. With the values

already ascertained the first is  $M-\xi-\uparrow$ , cf.  $\text{מִשֵׁ}$  in Gen. 10<sup>23</sup>, a 'son' of Aram—perhaps the same as the Masu,<sup>1</sup> who were allied with the Hittites against Egypt at the battle of Kadesh.

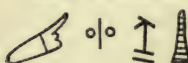
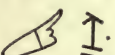
The second group is  $M-\text{wing}-k(g)-\uparrow$ . This I take to be the  $\text{מִשֵׁ}$  of Gen. 10<sup>2</sup>, a 'son' of Japhet, in cuneiform *Muški*, in Greek *Mόσχοι*, allies of the Hittites in Mesopotamia in the later period. Then  $\text{wing}$  is provisionally another  $\xi$ , and the whole group is, 'Of the Masians, king, of the Moschians, king'. A third king is mentioned, but I have not yet identified his people.

To return to Mar'ash. The sign  $\text{cross}$  or  $\text{diagonal}$  must be some sort of guttural. On the ground of the cuneiform *Marḫašu* I have called it *k*, but it might be *g*, or some strange sound like the Arabic  $\text{g}$  (or Sumerian  $\text{g}$ ), since in the modern pronunciation it has been weakened to  $\text{مرعش}$ . But  $\text{diagonal}$  (and therefore  $\text{cross}$ ) is not the same as  $\text{wing}$ , since it always points the other way, and

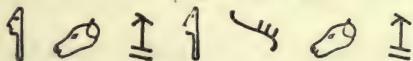
<sup>1</sup> Hardly Mysians. Even in cuneiform it is hard to identify some of the obscure tribes mentioned, and the difficulty is greater here, because we do not know where to look.

the direction of the signs is carefully observed in the inscriptions.

The omission of the vowel is common. Another instance is

, 'King of the Kuans' (Eastern Cilicia) in A 11 b<sup>3</sup>, elsewhere (A 4<sup>d</sup>) spelt .

One more pair of names may be mentioned, because they occur so frequently, though the reading of them is uncertain. The group



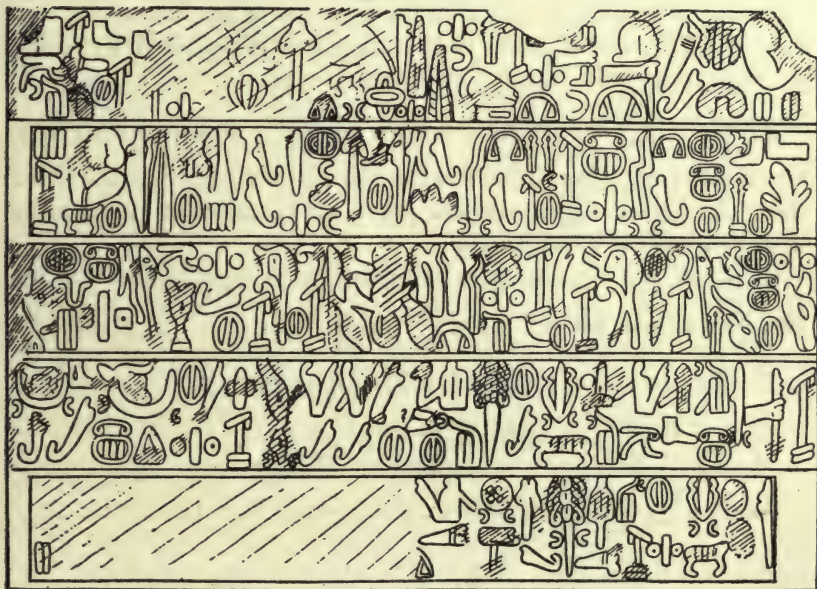





FIG. 30. (From Messerschmidt, pl. 6.)



is found in A 2<sup>4</sup>, A 6<sup>2.9</sup> (and to be restored in A 6<sup>1</sup>), A 11 a<sup>6</sup>, A 11 c<sup>3</sup>, and to be restored in M 6<sup>3</sup>, though it is unrecognizable in the published copy (fig. 30). It is divided into two parts by the sign





which shows that it contains the names of two peoples or countries. The first name occurs alone in A 2<sup>2</sup>, and perhaps in M 2<sup>2</sup>: the second occurs alone in A 1 b<sup>3</sup> (= M 15<sup>3</sup>). That is to say, the names are mentioned in the Carchemish texts only (and perhaps in M 2, from Babylon), but not at Hamath, nor in the (apparently) later inscriptions. They seem to be a pair naturally connected together like Sumer and Accad, but also

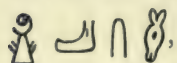
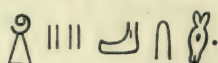
associated by similarity of sound. The combination may have continued in use long after it had ceased to have a political significance. We require then two place-names (differing only in that one has an additional syllable in the middle), which must at some time have had an importance in Hittite affairs. In Delitzsch's vocabularies *halanta* is 'head', so that perhaps the head () may have the value *ha*. Elsewhere I have suspected

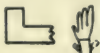





the value *n* for  and *t* for . Hence the two names may be *Hana* (in North Mesopotamia) and *Hattin* (from there to the Orontes). That is not proved, for the values are uncertain, but two small points of some importance are established by comparing the ways in which they are written:



(1) That  is a simplified form of <sup>1</sup>, and that both may occur close together in the same inscription (A 6<sup>9.2</sup>, fig. 27), so that the linear forms do not prove an inscription to be later than one with the full forms.




(2) That the system of writing employs ligatures, since we find (M 15<sup>3</sup> = A 1 b<sup>3</sup>, cf. A 2<sup>4</sup>)  for .




Another common ligature is with |||, as in A 6<sup>2</sup>




, also written .

A 6<sup>4.8</sup>    (cf. M 4<sup>2</sup>, 6<sup>2</sup>, showing how the copies may mislead), written    in A 11 b<sup>3.5</sup>, A 11 c<sup>6</sup>.

Others are  =  (M 2<sup>6</sup>),

 =   (M 2<sup>4.5</sup>, cf. A 1<sup>6</sup>, &c.),

and perhaps  =   (M 3<sup>21</sup>),

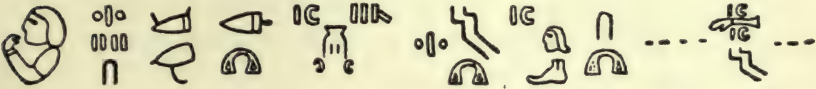
 =  ? (A 6<sup>3</sup>, &c.).

<sup>1</sup> So also Campbell Thompson.



So far I have dealt only with names, and the results, though few, are fairly certain. We will now go on to something more

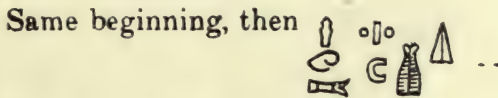
M. 2 (Babylon).



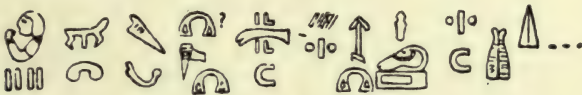
M. 3 B (Hamath).



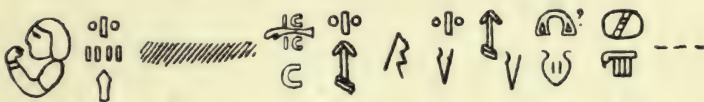
M. 4 A and B (Hamath).



M. 6 (Hamath).



M. 7 (Kirchoghlu)



M. 9 (Jerabis).

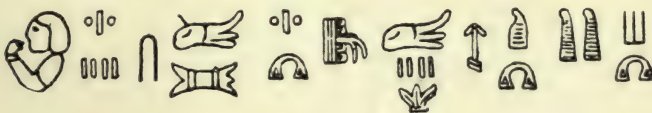


FIG 31.


conjectural. Let us take first the beginnings of the best preserved inscriptions in Messerschmidt's *Corpus* and examine them in detail (figs. 31, 32.) These clearly represent a regular formula with variations. They begin generally with a head or












Hittite, begin with the Semitic word *umma*, 'thus (says)'. I suggest that this group, following the speaking figure, is a Semitic loan-word, *umma*, and that, therefore,  $\circ|\circ = u$  and  $\cap = a$ .

These values seem to me to agree best with other indications.

We might then expect a name to follow, and in fact the next group is in most cases marked by a slanting stroke *over* the first sign. I take this stroke to indicate a personal name. (So Campbell Thompson, independently.) After the name we ought to have titles—king, prince, governor, &c.—and here, as might be expected, there is considerable variety. The groups mostly end in  $\Delta\Delta = \check{s}$ . (Sayce thinks this is the mark of the nominative case. It may be so, or it may be demonstrative or pronominal in character, like the  $\aleph$  of the emph. st. in Aramaic. We do not yet know whether the language had or had not cases.)

After the titles there is generally the sign , with a phonetic complement—a hand turned down in the act of giving or placing. This should be the verb, and since it follows *umma*, &c., it should be in the first person singular. Thus the normal formula would be something like: 'Thus (says) X, the king, I dedicated this (temple, &c.)'. Taking the first line on the list: *umma* is followed by a name (marked with the oblique stroke) which I read Kiakkis (for reasons which do not matter at present), then a title which occurs very commonly in various forms, nearly always in this position in the introductory formula, and rarely elsewhere.

id.	K (?)	U	N	Š	
	k	o o		$\Delta\Delta$	(M 2, 52).
<hr/>				c	 (M 21).
	k	o o	c	$\Delta\Delta$	(A 11 a <sup>1</sup> ).
		$\cap$	$\Delta\Delta$		(A 7 j <sup>1</sup> ).
	c	$\cap$	$\Delta\Delta$		(A 11 b <sup>1</sup> ).
? 	c	$\cap$	$\Delta\Delta$		(M 32, 33, A 6 <sup>1</sup> ).
	$\Delta\Delta$				(A 2 <sup>1</sup> ).

The only constant parts are  $\overline{\text{u}}$  (with or without  $\text{c}$ ) and the termination  $\Delta\Delta$ . Sayce has conjectured that  $\overline{\text{u}}$  is a picture of the priestly apron. It probably is used as the ideogram of priest. In an unpublished inscription at Carchemish it occurs with a name under a figure evidently representing a priest. In A 6<sup>1</sup> and A 11b<sup>1</sup> the group precedes the ideogram of *god*, hence clearly 'priest of the god so-and-so'. The rest of the signs are then the phonetic complement, or (in the fullest form) perhaps the complete spelling of the word.<sup>1</sup> We get thus  $\overline{\text{u}} = \Delta\Delta = \text{š}$ , as before, and  $\text{c} = \text{L}$ . Also  $\overline{\text{u}}$  may be omitted, as before. As to

the value of  $\text{c} = \text{L}$ , I venture the following suggestion: the

IL . . . N . . . I

⊕ . . . c . . . ↑









sign ⊕ is the ideogram of *god*, and ⊕ c ↑ (frequently) seems to be the plural of it [⊕ c ↑ the genitive plural (A 4 d)]. If ⊕ was pronounced *ilu* as a Semitic loan-word,<sup>2</sup> its plural would be *ilāni*, and  $\text{c}$  would be *n*, and ↑ would be *i*. (We already have the vowels *a* and *u*.) In the inscription from Ördek-burnu,<sup>3</sup> אלה is certainly used as a loan-word for *god*, and a word כן seems to mean *priest*. The word for *priest* here would end in *-u-n-a-s* (the *-as* being only the ending of the grammatical form), and we may perhaps conjecture that  $\text{kk} = k$ . In a Greek inscription from Lydia<sup>4</sup> we have the word *κaveiv* for *priestess*, which may well be a Hittite (or native Lydian) loan-word. The Hittite word may then really read *ka-u-in-a-s*, the *u* being a semi-vowel. Of course one naturally thinks of the Hebrew כהן, but it is impossible at present to say on which side the borrowing is.




<sup>1</sup> Not, as suggested in *JRAS.*, 1917, p. 568, two words.


<sup>2</sup> As in cuneiform Hittite.





<sup>3</sup> Near Zenjirli, in Aramaic characters but in a non-Semitic language, perhaps a Hittite dialect. See Lidzbarski, *Ephemeris*, iii, p. 192.

<sup>4</sup> *Amer. Journ. of Archaeology*, 1913, p. 362, &c.

The next group occurs several times in this position, i.e. as a title. In M 21<sup>1</sup>    , and in M 52<sup>1</sup>    .


Hence we may conclude that  =  = .


Elsewhere (M 21<sup>5,6</sup>) it is preceded by , and must, therefore, be a place-name (or possibly a god-name). We, therefore, want a word which will fit both, as a title and as a place-name. After thinking over it for a long time, it struck me that the place must be Kadesh (the Hittite city on the Orontes), and the title *kata-š*, a Hittite term for some kind of officer, found in Egyptian. Cf. also Kadaš-man in Kassite proper names, represented by *tukulti*. We might translate it 'prince' or 'governor'. Then,

 =  =  = *k* or *k̄*, and  = *t* or *d*.












Translate: 'Thus says Kiakkis, priest, governor . . . I set up . . .'

The same formula occurs in the other beginnings:

M 3, 4, 6. 'Thus says (title) (name) I set up . . .' Here  alone is used instead of *umma*. Perhaps it is the pronoun *I*. After the verb is something like 'this memorial of the king of the city'.





M 9. 'Thus says Katus, of Carchemish king, the great king.' The double king-sign with  occurs often at Carchemish. It is not 'king of kings', which is not the Hittite title.

M 21. 'Thus says X . . . governor, priest, (title), king of the land-of-the-city, of Y . . . son, I set up this . . .'

Here    , &c., might be 'priest of Kadesh', but at the end of l. 1 it occurs again without  and with the addition of  (and so in l. 2), which seems more likely to be a defining termination ('the said governor') than to be another case of Kadesh. The title after 'priest' is still obscure.<sup>1</sup> In M 4, 6, 'king of the city' was  , here 'city' has the addition   and the defining . This *-da* may, of course, be merely a phonetic complement, or a case-ending—a locative—literally 'king in the city', but since we know that the Hittite phrase





<sup>1</sup> The explanation proposed in *JRAS.*, 1917, p. 565, is purely conjectural.








was 'king of the land of the city', it is not improbable that we have this here.

The next group is marked with the top-stroke, indicating a proper name. If  is a ligature for   it ought to read *K-wi-n-s-a-s*, but I know of no such name. However, if it is a name, the next group should mean *son*. In l. 3, in a similar combination, 'son' is expressed as an ideogram by a rabbit, so that  here is only an abridgement of it. The same ideogram occurs in M 46 after a name, where the differences in the phonetic complement are instructive:




     (M 21<sup>1</sup>).

   (M 21<sup>3</sup>).









    (M 46<sup>1</sup>).


Hence it appears that the vowels may be omitted at will: that  =  = *k*:  =  =  = *š*. I think  is a linear form of  = *š*. The pronunciation of the ideogram is unknown.

The other Mar'ash inscription, M 52, is closely connected with M 21. It begins: 'Thus says X . . . , governor, priest, (title), king of the land of the city, of Markašu priest, I set up this . . .'



The name, which is the same in both, is puzzling. The sign  certainly represents a god-name. Sayce takes it to be Sandes, the god of Cilicia. It occurs at Hamath, Malatia, Agrak, Mar'ash, Bulgar-maden, but only exceptionally at Carchemish, therefore mostly round Cilicia (and at Hamath). It also occurs in M 2, an inscription which was found at Babylon, but clearly does not belong there (see below). Since  = *b* (*p*) the name might be Sandapi (Harper, *Ass. and Bab. Letters*, 167<sup>15</sup>), but that is doubtful, for names compounded with Sand- do not seem to occur commonly in Asia Minor (see Sundwall). M 2 (where  is used for the god, as well as to form a personal name) bears a picture evidently representing Adad = Rammanu (<sup>11</sup>IM), the storm-god, and the sign might be taken as the lightning which he holds in his hand. He might indeed be Tešup, the specially Hittite (and Mitanni) god, but Tešup is apparently never the first

element in a name. There is also Tarku (Tarḫu), a Hittite<sup>1</sup> or Cilician god who frequently forms the first element in the names of Asia Minor, as may be seen from the lists collected by Sachau and Sundwall from native inscriptions and Greek sources, as well as from names occurring in cuneiform texts. But in M 34 and 51




we have the group   , and if  is really *du*, as in Karduniaš, this god-name must be either Addu, or Sandu. Provisionally I read it as the former, and  as the ideogram of Addu, cf. M 2<sup>2</sup>, &c.,   , with the defining termination, <sup>11</sup>Addu-š.

The next sign is found only in this connexion here and in M 21 and M 3 B. It is no doubt the same as  in M 2, M 16.



The third sign is *b* (*p*). The whole name might be Addu-raba (*or* rapa), for there is no reason why it should not be Semitic, but further proof is needed.

After the name we have *kadawaš* <sup>det</sup> *kauinaš*, then the same unknown title as in M 21, but with  for , then 'king of the land of the city', then *Murkaš-š*, the final *š* being apparently the genitive ending, then 'priest' again. I at first took *Murkaš-š* to be in apposition to 'city' ('the city of Marash'), but the repetition of 'priest' makes it more probable that the combination is 'of M. priest', and it may then be questioned whether *Kadawaš* is not 'of Kadesh'.

M 31 and 32 are specially interesting because of the recurrence of the names. M 31 must be 'Adduas (son) of Adduwin', the genitive alone expressing the idea of son, as in other languages.

The next sign  I take to be the ideogram of *Sun*, a pillar of fire with rays. Its phonetic complement is   = *m-š*. In the Arzawa letter, i. 23, we have *-miš* as a suffixed pronoun = 'my'. Hence this group means 'my Sun', cf. Arzawa, i. 13, <sup>an</sup>*UD-mi*, the title which the Hittite king applied to himself, as it were 'my majesty', and see Meissner in *ZDMG* 72, p. 35, <sup>11</sup>*Šamši* . . . . M 32 has 'Adduwin, prince and priest, (son) of Adduas'. The reading of this inscription is not always certain. Hogarth differs from Messerschmidt, and Olmstead from both. The two signs

<sup>1</sup> Hommel says (*Grundriss*, p. 44) that the Kassite Turgu was = Bel-Rammanu.

at the beginning I do not yet understand. The value  $n$  for  is only conjectured from comparison with the similar name in M 31. The next group is the ideogram for 'prince' (or similar title) with phonetic complement  $-š$ . Possibly  is a ligature for  $\text{𐎧} + \text{𐎶} = \text{𐎧𐎶}$ , cf. on A 6<sup>1</sup>. Then follows  $\text{𐎶}$  with the word-divider over it. This can hardly be a prefix to  $\text{𐎶} \text{𐎶} \text{𐎶}$ , forming one word with it. I take it to be the conjunction 'and', cf. below on M 2. Elsewhere  $\text{𐎶}$  seems to be =  $n$ , and in cuneiform Hittite *nu* seems to mean (at least sometimes) 'and'. The second name should be the genitive of the first name in M 31. As suggested above,  $\text{𐎶}$  is probably a ligature for  $\text{𐎶} \text{𐎶}$ , and its value should then be *an*. It should correspond to  $\text{𐎶} (+ \text{𐎶})$ , the (genitive) termination of the second name in M 31. Then  $\text{𐎶} = \text{𐎶} = \text{𐎶} = n = ? \text{𐎶}$ .

After the verb ('I set up, dedicated') and 'this', the word *ubkušš* (if that is the right reading, cf. M 33<sup>1</sup>) probably means 'boundary stone', since it is followed by  $\text{𐎶} \text{𐎶} \text{𐎶}$  'the land of the city'. It has been suggested independently that the stone marks a boundary. This meaning is also suitable in M 33<sup>1</sup> and in M 34. The word does not occur at Carchemish, where, as all the known inscriptions are in the city itself, a boundary stone would not be found.

M 33 begins with *ubkušš*. Then a group which I took<sup>1</sup> to be a name, on account of the top-stroke. I now believe that Sayce must be right, and that it is a derivative form of Tuna or Tyana (= Bor) where the stone was found. The whole group will then read *T-u-n-u-n-š*, and the two words will mean 'boundary of Tyana'. This confirms the value  $\text{𐎶} = n$ . The sign  $\text{𐎶} = t$  is then to be distinguished from  $\text{𐎶} = k$ . Next follows 'king of the land-of-the-city, priest'. The next group is  $\text{𐎶} \text{𐎶} \text{𐎶}$ -*k-a-š*, where

<sup>1</sup> *JRAS.* 1917, p. 577.



I take  $\nabla$  to be a linear form of  $\oint = r$ . If  $\int = \xi$  the word will be *šarkaš*, cf. *šarkuš* = 'chief' in Delitzsch's vocabularies, possibly derived from the Semitic *šar*. The same title occurs in M 51, and is probably the same as  $\Delta \nabla \oint \text{III}$  in M 7 (a later inscription), where the values  $\xi$  and  $k$  are already known. The value  $\text{III} = \xi$  (or  $z$ ) is confirmed by M 3 B<sup>2</sup>, 4 A<sup>2</sup>, 4 B<sup>2</sup>  $\text{Q} \subset \text{III} \underline{\text{I}}$  'of Kinza'.

The beginning may then be translated: 'Boundary of Tyana. The king of the land of the city, the high priest X (name broken), set up this monument (?) . . .'

We will now compare the beginnings of the new Carchemish inscriptions (fig. 33). These are almost certainly to be translated as follows:

A 1 b (= M 15 B). 'Thus says my majesty X . . . the great king. My majesty . . .'  $\text{X} ||| \Delta$  is no doubt to be read in both places, though they are defaced. For the expression see above (p. 67) on M 31.

The name is the same as in A 2, A 11 a, A 11 b, but the values of the first two signs of it are unknown.

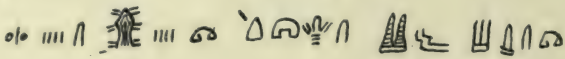

The next group, since it occurs (with variations) elsewhere after the sign for 'king', can hardly be anything but 'great king'. Whether it is all one compound word, or  $||| \text{II} \cap \Delta$  is a separate word meaning 'great', is not yet certain.

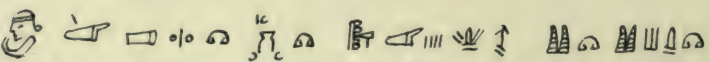
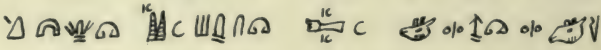
A 2. 'Thus says Katus, priest, of Carchemish king, great king, (son) of X . . ., the great king: I set up this monument (or similar term) in honour of (?) the god Addu(š), ruler of Hana (?) . . .'


The name occurs also in M 9, A 3, A 11, therefore only at Carchemish. The second sign of it is the same as in Tyana (M 33), and is a less ornamental form of  $\text{B}$  in A 11. There was a Kate-i of Kue in the time of Shalmaneser III, cf. also *Kadovias*, a king of the Scythians in Suidas, Katova in the new Lydian inscriptions, and *Kóvus*.<sup>1</sup> The second name is the same as in A 1 b, with the addition of  $\Delta$ . I take this to be a genitive expressing 'son of', as in Etruscan and often in Greek (see above


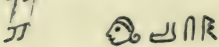
<sup>1</sup> A connexion with the Kassite name Gaddaš (for Gandaš) is less probable.

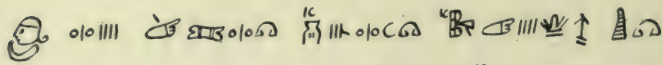
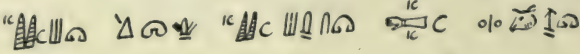

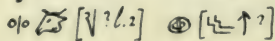
p. 67). If it were a nominative, we should have two great kings as joint donors of the monument, which is unlikely. The verb, too, is in the same form as when only one name is the subject. Katus was then son (and no doubt successor) of the author of A 1 b.

A 1 b    


A 2    


A 3 

A 6    


A. 11 a    
   
   


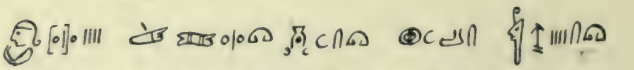
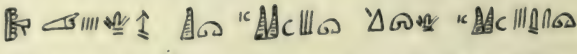
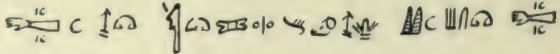
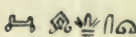

















A 4 b    
   
   


FIG. 33.









'This' is elsewhere    , so that  is only a linear form of the calf's head—value not ascertained. The next words     (omitted in fig. 33), may be compared with M 33<sup>2</sup>      , so that



 =  = *n*. The meaning 'in honour of' is only a guess.




Something of the kind seems necessary since a god's name follows. On *Ḫana*, see above, p. 60. On 'ruler', see below, p. 78.

A 3. 'This (?) is the Carchemisian, king of the-land-of-Addu, *Katuš* the great king . . .'





This beginning differs in form from that of the other inscriptions. The first word (*wi-ā-b-u*) seems to be connected with *wi-š-b-u*,<sup>1</sup> &c., in A 7. There is no word for 'set up'. Hence we should expect that originally the stone (a door jamb) had a portrait attached to it.

The suffix in     may be merely grammatical, but it may be the same as in   , which we took to mean 'the land-of-the-city'. The construction is not quite clear, but at any rate the god  (*Addu*?) is represented as the god of Carchemish.






'Great king' here differs from the usual form by the insertion of  , which may be only the phonetic complement of 'king'.





In that case    would seem to be a separate word, meaning 'great'.

A. 6. 'Thus says *Anaas*, priest of the god X . . . my lord, lord of the land of B . . . , governour of . . .'

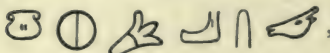
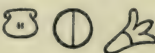
With the name cf. the Cappadocian names *Ania*, *Anima* (Tallquist), and *Avva*, &c., Kretschmer, *Einleitung*, p. 344. In A 7 b it is spelt    . He was not a king, and his dress is not royal. Note also (in Hogarth) the girdle, as still worn locally.

The god's name is written with an ideogram value unknown.


In the next group, I take      *-maš* to be the suffixed pronoun 'my', like *-miš* in *Arzawa* i. Cf. also *Kadašman* in Kassite names, translated by *tukulti* 'my helper'. The whole group here is then *š-n-š-m-a-š*, used like *beli-ia* in a similar position in Assyrian texts, 'my lord'.

The next group is probably the same word *š-n-š*, but written with the ligature  =   =  c.

<sup>1</sup> Elsewhere *-bu* is apparently used as a mere grammatical suffix.

Then a place-name would naturally follow, but the reading of it is difficult, because the value of the second sign (a compound?) is unknown. The name is perhaps the same as , &c., in M 5<sup>1</sup>, 6<sup>2,3</sup>, where the value of the third sign is unknown. The *-da* in both cases looks like the termination meaning 'land of', so that  might be either the name of a god or a city. The group is hardly *Pakhudu*, the name of the Aramaeans in Mesopotamia.\*

A 11 a. 'Thus says Katusš, priest, of Carchemish king, great king, (son) of X . . . the great king, I set up this. *H-s-t-u-t?-n-wi-š-a* the great king had built the temple(?) . . .' (lacuna).

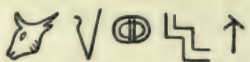
The king is the same as in A 2, A 3, &c., and the titles are as usual. The father's name is here again in the genitive (to express 'son of') but without .

The group after the verb would naturally be read *u-m-wi-š*, but in A 2<sup>1</sup> a similar group (if the animals' heads are the same in both) in the same position would read *m-u-wi-š*.

The long group following is a name, since it is marked with the top-stroke, and is followed by 'great king'. Unfortunately the first and fifth signs are uncertain. Perhaps the end should be read *-wi-a-š*.

The most important fact ascertained from this line concerns the verb. This occurs twice, the hand being upright the second time (merely to fit the space), but with the same meaning, cf. A 11 b<sup>1</sup>. In the first occurrence it is in the 1st person, and has the phonetic complement C, as elsewhere. The second time it is in the 3rd person, without phonetic complement, and therefore probably the bare stem. Thus the distinction confirms our analysis of the sentence.

The next group occurs, with variations, several times. The meaning 'temple' is a mere guess, based on the proximity of the god-sign.

The effaced signs following are no doubt to be restored from 1. 2 as  *u-m-n IL-n-i* (see p. 64). *U-m-n* must be a substantive (or preposition?), since in 1. 2 we have *u-m-n-m-a-š* 'my umn'.

A 11 b. 'Thus says Katusš, priest of the (land of the?) gods my lords, of Carchemish king, great king, (son) of X . . . the great

king, I set up this. *H-š-t-u-t-n-wi-š* the great king had built the temple (?) . . .

Cf. the beginning of A 11 a.

*IL-n-d-a* may be only an oblique case of *ilāni*, or it may mean 'land of the gods'.

The next group (as in A 6<sup>1</sup> in the same connexion) must be 'my lords', and  $\text{𒌷}$  is the (genitive?) plural termination as in gentilic names. The ideogram for 'lords' (plural) is used in A 6<sup>1</sup> as parallel to  $\text{𒌷}$   $\text{𒌷}$  or  $\text{𒌷}$   $\text{𒌷}$  (singular), and it is tempting to make  $\text{𒌷} = \text{𒌷} = \text{š-n}$ , but it may be a different word. If

$\text{𒌷} = \text{š}$  it cannot of course be  $\text{h}$  in  $\text{𒌷}$   $\text{𒌷}$   $\text{𒌷}$ , as proposed above.

The long name has  $\text{𒌷}$  for  $\text{𒌷}$  in A 11 a.

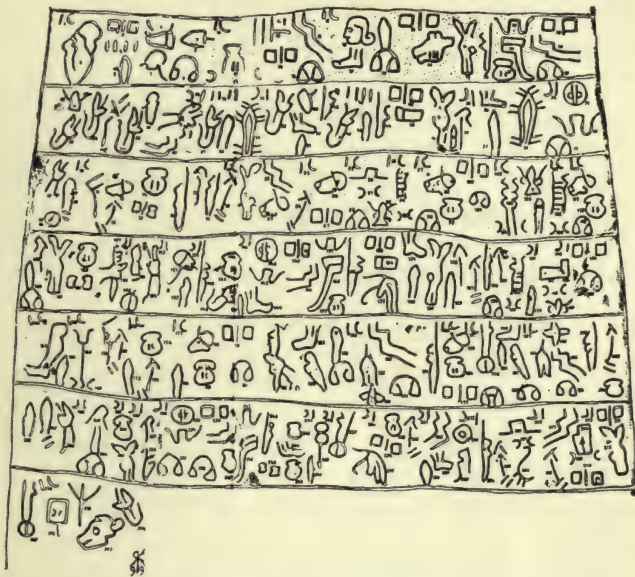


FIG. 34. (From Koldewey.)


We will now take the inscription M 2 (fig. 34), and consider it in detail. I choose this because it is well preserved, mostly legible, and raises many interesting questions. It seems to be historical in character. It is fairly easy to divide into its component words, partly owing to the free use of the word-divider

10. We must not, however, assume (as is done by Koldewey in his *editio princeps*<sup>1</sup>) that every word is so marked, and that, therefore, from e.g. the middle of l. 1 to the beginning of l. 2 is all one word or name. The sign is used irregularly, or perhaps we should rather regard it as some sort of mark of punctuation. If so, it might be dropped where words are used in close connexion, the connected words being regarded as forming one compound phrase.

Two facts about the inscription must be noted at the outset:

(1) It is on a block of dolerite, such as is not found in Babylonia. It was, therefore, brought there as a trophy. Koldewey<sup>2</sup> points out that the portrait of the god on one side of it is like that at Zenjirli,<sup>3</sup> and that it probably comes from the same district and time, i.e. from the land to the north of Syria in the tenth century B.C. At any rate it was not set up in Babylon by any Hittite invaders.

(2) According to Koldewey, the beginning, i.e. most of l. 1, has been re-carved on an erasure. That is to say, the man who is represented as speaking took an old inscription, had the original beginning chiselled away and his own name put in its place. Perhaps he wished to appropriate a distinguished record. It is hardly likely that the erasure is due to a mistake, for in that case they would have re-dressed the whole surface of the stone. This falsification increases the difficulty of studying the text.

The name contains the same sign twice, and this has been shown to interchange with . It is therefore a *k*. The second sign recurs in l. 4, where I take it to be *ia*.<sup>4</sup> Hence we may read the name (with the top-stroke) as *K-ia-k-š*. The final *š* is, of course, merely the grammatical termination. A man named Kiakki was king of Tabal in Sargon's time, and was deposed by him and sent to Assyria in 718. This may be the






<sup>1</sup> *Wiss. Veröff. d. Deutschen Orient.-Ges.* i (1900).



<sup>2</sup> *Das wieder erstehende Babylon* (1913), p. 163.



<sup>3</sup> See Messerschmidt, *Corpus*, Tafel i, and above, figs. 13 and 28.



<sup>4</sup> We are not yet in a position to discuss Hittite palaeography, and it is wiser for the present not to speculate on the relation of this sign to others somewhat like it elsewhere. M2 is peculiar in its writing. Several common signs do not occur in it at all, and several signs occur frequently which are otherwise not found, or found only rarely. It must therefore be interpreted largely from itself. The same is true of other texts and groups of texts. Some signs seem to have had a local currency only.

man in question, and the stone may have been sent to Babylon, when he was deported to Assyria. (Sargon made himself Sakkanak of Babylon in 709.) But it is more probable that he was an earlier king of the same name, since we know that the names did recur, and the stone may have been taken to Babylon as one result of some earlier success against the Hittites. In either case, since the bulk of the inscription is earlier than Kiakkiš, we must be prepared to find that it is to be dated long before 718 B.C.













He does not call himself king, but *kadaš utrn Addubunš*. The reading of this latter name is only conjectural, see above, p. 66. In the preceding word the second sign is uncertain. The word should be the same as  $\circ^{\circ}$     $\checkmark$  in l. 4, so that  =  = *t*, if the readings are both right. But in A 2<sup>1</sup> the calf's head corresponds to  = *m* in A 11 a<sup>1</sup>. Since *kadaš* is some kind of officer ('deputy', 'satrap') *utrn* should be a higher title, 'overlord'.

The second line begins the matter of the inscription with a new sentence. The sign  is again used for 'sun', a title applied to kings. Its termination  marks an oblique case. In the next group the first sign seems to be merely the common  $\uparrow$ , as in l. 4. The word is probably a noun, since in l. 4 it has the oblique case-ending. I suggest that *i-ya-r* is a form (plural?) of the Kassite *iaš* 'land', so that the two words are equivalent to *šar matāti*, literally *šamaš matāti*.




Then  $\circ^{\circ}$    $\checkmark$   $\parallel\parallel$   $\wedge$   ought to be a verb<sup>1</sup> with pronominal suffix, 'made me'—the suffix being the same as that used with nouns. Less probably the word might be *utrn-man* (with the *r* accidentally omitted), 'my overlord', as in ll. 1, 4.







Cf. also A 11 b<sup>5</sup>  $\circ^{\circ}$   $\parallel\parallel$    $\checkmark$   $\parallel\parallel$   $\wedge$  .






<sup>1</sup> Dare we compare this *utn-* with Etruscan *utin-ce*, meaning probably 'dedicated', &c.? Cf. Torp, *Etruskische Beiträge*, ii. p. 105.

The group ends certainly with , and a succeeding group begins with , a proper name with the top-stroke. Therefore  stands alone between them, as also in ll. 3, 4, 5, 6. I suggest that, as in M 32 (see above, p. 68), and often, this is the word for 'and', cf. *nu* in cuneiform Hittite. In the name following, the first sign is uncertain. If it is meant for  the name will be the same as    in M 1 on a bowl, also found at Babylon, i.e. Kadan. After 'my Sun' follows the verb  , elsewhere meaning to 'set up' (*ávέθηκε*). Here it is much the same, *έθηκε*. Note that the phonetic complement is *n*, as in the previous verb, *ut-n-man*. The subject of both is    at the beginning of the line.


The next six signs are unintelligible.



Line 3.   , as in l. 4 (see note), cf. l. 6.


The next sign seems to be a bad form of the head-sign, and the group to be    (see above, p. 59). Here it has the addition  , apparently some sort of case-ending. If so,  marks the plural, or noun of multitude, not necessarily genitive.








After  'and' *i-n-wi r-n-n-wi* must agree, since they have the same termination, which is plural. *Inwi* (or *wi-ni*) occurs elsewhere with 'gods' and other words. It must, therefore, have some very common meaning, such as 'these' (cf. Vannic *ini*) or 'all'. The word *r-n-n-* should then mean 'countries', 'cities', 'allies', or something of the kind. [Cf. Arinna, and Ar(e)n(e)na in the Treaty, 'the city'? or Vannic *arni*—'fortress'.] Then follows a series of *n-b-u-s* with a place-name, three times. (In the first occurrence the  has been omitted by an accident.) Cf. the Vannic *nu-u-s* 'king', or here more generally 'ruler'. As the place-names are written by ideograms they can only be guessed at present. All three have the same phonetic complement  or  , which must, therefore, be some formative








termination. This sign is no doubt meant for   $\xi$ , which otherwise would not occur in this inscription.

 in l. 6 has the phonetic complement  = *t*?

 (perhaps two heads) is not known.




 is no doubt for    $\bigcirc$ , as in M 4 A<sup>2</sup> and often, since the sign occurs only in this combination. As the middle sign is a ligature for  ||||, one is tempted to read the name as Halman = Aleppo, but  = *l* is not certain. Each name is followed by  , which looks like an ideogram (a tower) for a fortified city (בירה, cf. the Hittite gloss *buru* = 'fortress').














Line 4.      as in l. 2, with a case-ending, 'in these lands'.

*Utrn* must be the same word as in l. 1.


The termination in *Addu-da* is difficult, 'by (the grace of) Addu?' ( $\sigma\delta\nu\ \theta\epsilon\omega$ ).


The next group is marked as two words connected by  $\surd$  = 'and'.

As there is some reason to think that two values (*l* and *n*) are confused in , it is easy to guess that *b-*  *n(u)*  -*a* is the common pair 'Bel and Ea' (*Ia-a*). The sign  $\bigoplus$  then refers to all three.


   $\bigcirc$  here follows god-names (as    in l. 6) and precedes a personal name. In l. 3 it precedes a place-name. If it is a noun it should mean something like 'lords' or 'protectors'. The first sign is apparently a conventionalized upright hand, not the same as  because it slopes the wrong way. It appears as  in A 2<sup>5,6</sup>, where it corresponds to  $\text{u}3$  in A 6<sup>4</sup>. The word then will be another form of    in A 2<sup>2</sup>, cf. A 3<sup>1</sup>, and is perhaps the same (written phonetically) as   , &c., a common word,

in which the first sign is an ideogram (therefore = *bia* or *pia*<sup>1</sup>). The precise difference between the form here and that in l. 6 is not certain. This may be plural and that in l. 6 singular. I have noticed, however, that where two or more words (names) are closely connected, the last sometimes has the ending  $\textcircled{\text{D}}$ , which would thus seem to be an enclitic conjunction in some cases, like Latin -que. It is perhaps hardly suitable here.

The last group in this line was a great surprise to me when I first read it. It consists of an ideogram followed by six phonetic signs which, with the values already found, read *b-u-r-i-a-š*. There can be no doubt that we have here a name compounded with *Buriaš*, the name of the Kassite storm-god. One then naturally guesses the ideogram to be = *Burna*, and this seems in fact to be right. The hand with the dagger is used as a title, and Sayce long ago deduced the meaning 'powerful', &c., for it.<sup>2</sup> Now in the Kassite vocabulary<sup>3</sup> *bur-iaš* is translated *bel matâti* 'lord of lands', whence it has been concluded that in Kassite *bur* = 'lord' and *iaš* = 'land(s)'.<sup>4</sup> Then  may be *bur* in

Hittite also. The ideogram here is not merely a more elaborate form of this, for in A 1<sup>6</sup>, A 2<sup>5,6</sup>, and elsewhere, it is written quite clearly .

It is, therefore, a compound ideogram, and the upper part is not the handle of the dagger, as has been supposed, but the sign  $\sqrt{\quad}$ . Hence the whole should read *bur-n* or *Burna*, and, if *bur* means 'ruler', *burna* should mean 'ruled', of which *kidin* ('protégé') in the vocabulary is a fair if free translation. If this is the Kassite king, *Burnaburiaš* II, of Babylon, the inscription was written at any rate after 1350 B.C. Certainly the Kassites had influence in N. Syria then, of which this may be a record. It is not a late inscription, so far as one can judge palaeographically, for it does not use debased forms such as

. It does use IC carefully, whereas M 46 (certainly late) never has it. Some of its peculiarities make me

<sup>1</sup> Cf. the element often found in Asia Minor names (Sundwall, p. 178), so that e.g. *Ταρκύμβιος* would mean 'Tarku is lord'. The  $\mu$  is inserted only to emphasize the medial  $\beta$ , like the  $\nu$  in *Ταρκόνδημος*.

<sup>2</sup> Cf. too the early form for EN in Langdon, *Sumerian Grammar* (1911), p. 267.

<sup>3</sup> Pinches in *JRAS.* 1917, p. 106.

<sup>4</sup> Hüsing has questioned this, but see below on l. 6.

wonder whether it can possibly be in the Kassite language. That question cannot be answered till we know more of Kassite and more of Hittite. If, as suggested above, the date is about 900, Kassite would be unlikely.

Line 5. The beginning is unintelligible to me. It should be some title of Burnaburiaš or statement about him.

Then follows *nābuš* as in l. 3, but with  $\cap$  in the first syllable.

Then an interesting name, of which all the signs are now known, Burnadakaš. I do not know the name elsewhere, but it is perfectly good Kassite. The vocabulary gives *dakaš* = *kakkabu* 'star', and, as it was drawn up expressly to explain Kassite names, this word was no doubt used in the formation of them. It should mean then 'protégé de l'étoile'.

The next group, clearly a title, has two strange signs:  $\text{⌘}$  perhaps the same as in the name Murkašu in M 52 (above, p. 57), where it answers to  $\text{𐎧}$  = *k*. The other in Koldewey's copy has a form which does not occur in any other inscription. It is just possible that the word may be *k-a-(wi)-n-aš* 'priest', but it is very uncertain.

Then  $\text{𐎧}$  = 'and'. This must be a separate particle, since we know the preceding and succeeding words.

After *nabuš* again is another name compounded with *dakaš*, 'star'. Unfortunately we do not know the value of  $\text{𐎧}$ , a compound ideogram occurring frequently, and I do not like to hazard a guess about it.

The last word, carried on into l. 6, is *wi-b-u-r*. As the word following has the gentilic ending, this is probably the phonetic writing of  $\text{𐎧}$ , *bur*, with the prefix  $\text{𐎧}$ , of which the meaning is not clear. It is found elsewhere, and also with other prefixes, as  $\text{𐎧}$   $\text{𐎧}$   $\text{𐎧}$   $\text{𐎧}$ , A 6<sup>9</sup>,  $\text{𐎧}$   $\text{𐎧}$   $\text{𐎧}$   $\text{𐎧}$ , A 3<sup>2</sup>, and, without prefix,  $\text{𐎧}$   $\text{𐎧}$   $\text{𐎧}$ , A 2<sup>4.4</sup>, A 11 a<sup>5</sup>, A 11 c<sup>3</sup>.

Line 6 begins with a place-name, ending in  $\text{𐎧}$ , but the ideogram is not explained.

Then another name, probably the same as in l. 3. Note, no 'and'. Then  $\sqrt{\quad}$  = 'and', followed by a personal name (?), of which the first sign is unknown. The second sign is apparently  $\mathcal{R}$ , but as that does not occur elsewhere in the inscription, it may be wrongly copied. If it is  $\xi$ , one naturally thinks of the Kassite *šindi*, 'gift', but that is always the first part in a compound, or some name like *Πασάνδα*, or Alakšandu.

The next word is *wibur* again, the same as in l. 5, so that

$$\text{☞} = \text{☉} = r.$$

The next group occurs frequently. It is certainly a place-name, since it is usually connected with a word for 'ruler'. The essential part is  $\text{☉} \text{☉}$  (two ideograms), to which more or less phonetic complement is added. Here it has a full phonetic complement and also the termination  $\text{☉} \text{◦}$  (if that is the right reading), which seemed before to form a genitive or derivative adjective. (But the reading is not certain, and the ending might be  $\text{☉} \text{◦} \text{☉}$  as in A 2<sup>4</sup>.) The other occurrences are:

$$A 2^4. \quad \text{☉} \text{☉} \text{∧} \text{☉} \text{☞} + \text{☉} \text{◦} \text{☉}.$$

$$A 3^3. \quad \text{☉} \text{☉}^{\text{IC}} \text{☞} \text{∧} \text{☉}^{\text{IC}} \text{☉} \text{◦}.$$



$$A 6^9. \quad \text{☉} \text{☉} \text{☞} (= \text{☞} t).$$






A 11 a<sup>9</sup>.  $\text{☉} \text{☉} [\text{∧} ?]$  where  $\text{☉} \mathcal{R} c \text{☞}$  is again ruler of it (and cf. M 25<sup>5</sup>).






$$A 11 c^3. \quad \text{☉} \text{☉}^{\text{IC}} \text{☉} \text{∧} \text{☉} \text{☞} + \text{☉} \text{◦} \text{☉}.$$


Cf. also M 19 c end.

It is generally connected with  $\text{☉} \text{☞} \text{☉}$  and  $\text{☉} \text{☞} \text{☉}$  (Ḫana and Ḫattin?). I take the group to be Ḫani-rabbat (or -galbat), the country of Mitanni or part of it. (Note that in A 3<sup>3</sup> the first ideogram is written in two parts, of which  $\sqrt{\quad}$  may

be *n*, though it is turned the wrong way.) In A 2<sup>4</sup> it is spelt with the phonetic complement *-a-b-d* (or *t*) + *bur*. In A 3<sup>3</sup> the second ideogram is spelt out *-r-a-b + bu*. In A 6<sup>9</sup> the phonetic complement is only *-t*. In A 11 c<sup>3</sup> the second ideogram is spelt out in full *-r-a-b-d* (or *t*) + *bur*, so that  = *r* and  = *r*.

The only other new word in this line is     .

There are three word-dividers over it, of which the first, I think, really belongs to  'and', the second is intended to mark the beginning of the word, and the third stands before the last two signs. As this group is connected by 'and' with the god *Addus* (like *Bel* and *Ea* above), and followed by the word for 'lords', it must be a god-name. The first sign is not found elsewhere, and may be wrongly copied. The rest are *-b-r-ia-š*, the *ia* being expressed by a ligature  =  . In the Kassite vocabulary<sup>1</sup> *Ubriaš* is the equivalent of the Assyrian <sup>11</sup>IM, variously read as *Hadad*, *Ramman*, *Tešup*, &c. There can be no doubt that this is the name meant here, and therefore  is some sort of *u*. It is particularly interesting that the word-divider is placed unmistakably before *-iaš*, showing that this was regarded as a distinct word in the compound. The name has been taken as merely another way of writing *Buriaš*, but it is more probable that *Ubr-* is to be compared with Mitanni *ivri* (Messerschmidt *ipri*) 'king' and Vannic *evri*, and is therefore a synonym of *bur*. As *Buriaš* is rendered by *bel matāti*, so *Ubriaš* may be rendered *šar mātāti*.

Line 7.  may be *Ḫani*(*rabbat*), but the remaining four signs are unintelligible.

The whole inscription may then be translated somewhat as follows:

- (1) Thus says *Kiakkiš*, priest, viceroy of king (?) *Addubunaš* :
- (2) The god *Adduš* as *Sun* (i. e. king) of the lands appointed me, and *Gadan* my *Sun* he set up . . .
- (3) Protector of *Ḫana* and of these cities, governour of . . . the fortress, governour of . . . the fortress, gover-

<sup>1</sup> Pinches, *op. cit.*, p. 102.

(4) -nour of Aleppo (?) the fortress. Of these lands was Addubun(aš) overlord, by (the grace of) the gods Addu, Bel, and Ea, protectors of Burnaburiaš.

(5) . . . the governour Burnadakaš, priest (?), and the governour . . . -dakaš, priest and lord

(6) Of . . -un (and) of . . -t, and . . -sanda, lord of Ḥanirabbat and Addubunaš. May the gods Adduš and Ubriaš, protectors

(7) Of Ḥana (?) . . .

Much, of course, is uncertain in this first attempt at translation, but it may be hoped that the main outline is tolerably correct.



FIG. 35. (From Hogarth, pl. A. 1 b.)

We will now take another inscription (fig. 35) A 1 b (= M 15 B, a poor copy). This was found at Carchemish, where it still was in 1914. Unfortunately the readings are often very uncertain, and it is probable that something is lost on the left-hand side, for in l. 3 we have the word-divider at the end, a position which it never occupies. It should stand before or over the word to which it refers. The edge of the stone, though damaged, seems not to have been broken off, to judge from a photograph in my possession. Hence the inscription must have been continued on a contiguous block, and the lines are not consecutive on that side. We can, therefore, only treat the rest as a fragment.

The beginning has been already discussed.

Line 2. The first sign is lost. Sayce, who copied the text on the spot, read  $\uparrow$ . If the lines were continuous, we should

expect  $\triangleleft \cup \cap \triangle$  'of the land of the city'.

The next group  $\circ \cup \vee \parallel \cap \triangle$  (?) would seem to be the same word as  $\circ \square \vee \parallel \cap \searrow$  in M 2, corroborating the values  $\square = t$  and  $\cup = d$  or  $t$ . The reading *-maš* is not certain, and the distinction between *-maš* (if so) here, and *-man* in M 2, is not clear.

The next group is 'and (?) kadaš', but the  $\vee$  is doubtful. Sayce's copy omits it.

Then follows  $\uparrow \cap \searrow \uparrow \searrow \cap$ . It has already been suggested that  $\uparrow$  is the ideogram for  $\searrow \parallel$  in M 2 (pronounced *bia?*), and means 'lord' or something similar. If so, the combination should mean 'lord of the lords' or even 'king of the kings'. Then *-ra* looks like another genitive ending. Whether the *-r* in the first of the two forms is for *-ra*, and the meaning is 'viceroy of the king of the kings of X', or whether the first word is nominative singular 'viceroy, king of the kings of X', cannot be decided. Probably it is the former.

Line 3. The first sign is the ligature for  $\searrow \cup$ , and the word is the common group provisionally read *Hattin-wi* 'of Hattin'.

Then *a-i-a-š-r-a*. The word *aiaš*, &c., occurs frequently, e. g.

$\cap \uparrow \cap \oplus$

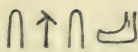


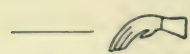
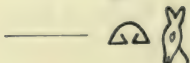
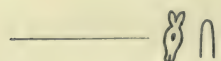
(M 1, M 6<sup>2</sup>, M 11<sup>3</sup>, M 19 A B<sup>5.8</sup>, M 52<sup>5</sup>, A 6<sup>4</sup>,  
A 11 a<sup>5</sup>.)

$\cap \uparrow \parallel (?) \oplus$

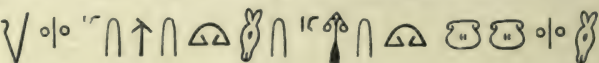
(A 3<sup>4</sup>.)

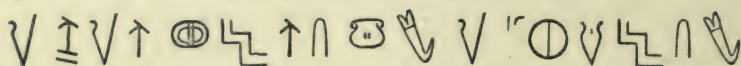
—  $\uparrow$

(M 46<sup>1</sup>.)

	(M 11 <sup>5</sup> .)
	(M 53.)
	(M 3, M 4.)
	(M 7 <sup>2</sup> , M 15 A <sup>3</sup> , M 19 C <sup>6,7</sup> .)
	(A 6 <sup>5,5</sup> .)
	(M 15 B <sup>3,4</sup> = A 1 b <sup>3,4</sup> , A 1 a <sup>5</sup> .)

The stem is therefore *a-i-a*, which may be compared with Vannic *aia* 'country', and perhaps is the same as Kassite *iaš* with a prothetic helping-vowel *a*. At any rate it is clear from the above list that *-ra* is a formative suffix and not part of the stem. If it forms a genitive, as suggested above, the word might either be construed with the preceding *Ḫattinwi* 'of Ḫattin the country' or with the following word 'of the (i. e. this) country ruler'. The latter is the more probable, if we compare the similar

phrase in A 1 a<sup>5</sup> 

 'and

of the(se) countries king (and) ruler, and these my (?) gods Bel and K-r-n-a-l', where *aiašra* must be connected with what follows.

The next group reads *m-b-u-r*, i. e. *bur* with the prefix *m*. In A 1 a<sup>5</sup>, just quoted, we have *b-b-u-r*, and in M 2 *wi-b-u-r*. The three forms thus seem to be variants of the same word, so that *w* and *m*, as in Babylonian, are not distinguished, and *b* is a variety of the same sound. Hence the words should be pronounced *wibur*, *mibur*, *bibur*.

Then follows 'and' and the word-divider, proving that some words are lost. Perhaps the sentence continued as in A 1 a<sup>5</sup>.

Line 4. The beginning is too much injured to be read. The last word is *aiašra* again.

Translate somewhat as follows:

- (1) Thus says my majesty the great king: my majesty . . .
- (2) . . . of my over-lord, and viceroy of the king of the kings
- (3) Of Ḫattin, of this land ruler and . . .
- (4) . . . of the land.



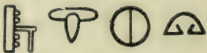
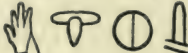


There are some indications that this inscription differs from M 2 in dialect, but only in dialect, since it has also much in common with M 2. The same difference may, therefore, be expected in other inscriptions, just as they also differ in the use of signs, so that progress in decipherment must necessarily be slow.

What, then, is the fundamental character of the language, or of that part of it which the texts may be found to have in common? The question cannot be answered until all the material has been subjected to a careful analysis, which cannot be made here. In what has been said above, I have tried to keep an open mind and to observe the slightest indications, in the hope that they may point in some definite direction. In M 2 we found undoubted Kassite names and probably Kassite words. There are also suggestions of affinity with Vannic<sup>1</sup> and Mitanni. These three languages, however, are so little known that they can help little in the decipherment of Hittite. Yet it does seem that this group (perhaps including Elamite) is inter-related and has some connexion with Hittite. At any rate no other family of languages (certainly neither Semitic nor Indo-European) shows any evident connexion with it at all. If these inscriptions are of about the ninth century B.C. and onwards, we should expect the language to be related to Vannic more nearly than to Kassite, which belongs to an earlier period. Unfortunately, owing to the large use of ideograms in the Vannic inscriptions, and the similarity of their contents, the number of words of which we know the pronunciation is relatively small, and the same difficulty meets us in the Hittite texts. In Kassite the words known are fewer still and we have no consecutive texts.

That cuneiform Hittite should be allied to the hieroglyphic language is to be expected, but the relation is not yet proved. To decide the extent of it we must wait till more cuneiform Hittite is published.

In the above attempt at decipherment I am painfully conscious of the poverty of the results. Many more suggestions might be made, but it is better not to pile up hypotheses until the foundations are assured. So far as the results rest on the identification of names they are probably sound, but when we venture farther imagination is liable to be too enticing. A single short bilingual

<sup>1</sup> Sayce considers that the Hittite system of writing was used in the region of Van before the introduction of cuneiform (*JRAS.* 1882, p. 418), and that Vannic is related to Georgian (*ibid.*, p. 410).

text would confirm or confute the whole system. It must be remembered, too, that we are at present wholly ignorant of the phonetic laws which governed Hittite writing. One instance is sufficiently instructive. It has been mentioned that the use of the sloping side-stroke is not clear. The group  = Karduni(a)š, is at least probable. But in A 11 b<sup>c</sup> the name is written  (in an oblique case). Elsewhere  was shown to be *k* (or *k̄*), and the variant thus corroborates the value of . But the side-stroke cannot always mean *r*. What then is the reason of the variant? Was *r* liable to be dropped, or assimilated?

With regard to the principle of the hieroglyphic writing, there can be no doubt that it is partly ideographic and partly phonetic. It is not always possible to be sure to which class a particular sign belongs. Some common signs are unquestionably ideographic, but speaking generally it may be assumed that the less common are ideograms and the more common are phonetic. The total number is not large—nothing like that of the Assyrian syllabary for instance—but the texts are relatively few, and more texts may provide more signs. Even from the existing material it is not possible to draw up a complete list of signs, because some of the inscriptions are much defaced, and of others the copies<sup>1</sup> are not to be trusted. Consequently we cannot always be sure whether similar forms are really identical, and whether exceptional forms have not been mis-read (as in M 6). When we know the language we shall be able to read much which is now illegible.

As to the pronunciation of the ideograms we may never attain to certainty. In the case of the phonetic signs there is more ground for hopefulness. It is indeed still uncertain whether they are syllabic or purely alphabetic. In the course of studying them I have sometimes thought that there were indications of syllabic values, *as*, *is*, *us*, *sa*, *si*, *su*, but as the evidence was not convincing it seemed wiser to treat all phonetic signs provisionally as alphabetic, and to assume that vowels were often omitted in

<sup>1</sup> The best are those by Lawrence and Woolley in Hogarth's *Carchemish*; the worst are those in Olmstead, Charles, and Wrench, *Travels and Studies*, vol. i. Those of Messerschmidt are mostly good.

writing, as for instance in Phoenician and in Etruscan. If this should turn out to be really the system, it opens up some very interesting possibilities.










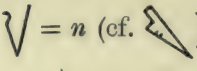






























Sayce<sup>1</sup> has noted a tendency in the Vannic inscriptions to change the cuneiform syllabary into an alphabet, as later the Persians actually did. Now, if our Hittite inscriptions began to be written about 900 B.C., and the Vannic soon after, they are not far removed from the earliest specimens of 'Phoenician' writing. (The Mesha inscription was written about 850.) It would seem then that all three developments were due to an alphabetic idea which was in the air about that time. The Hittites developed a partly alphabetic form of writing from their existing system of quasi-heraldic signs, the people of Van tried to do the same with the Assyrian cuneiform, and both failed; the originators of the 'Phoenician' alphabet developed their system from some unknown set of signs, and gained universal acceptance. It was as perfectly simple as it was simply imperfect.

Attempts have sometimes been made to show a connexion between Hittite and Cypriote or Cretan signs—without much success. The Cypriote signs are evidently much conventionalized (as are many of the Hittite), and as we do not know their original forms<sup>2</sup> it is useless to attempt a comparison. Of the Cretan signs no values are yet ascertained, so that comparisons are worthless. It is indeed probable that the Cretan and Cypriote systems will turn out to have something in common. Cretan, however, uses ideograms, while Cypriote is purely syllabic—a much more artificial system. Whether either will prove to be connected with Hittite we cannot say. In conclusion, as a summary of what has been suggested above, I add a list of ideograms and of phonetic signs, with their values where these have been made at all probable.

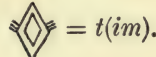
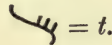
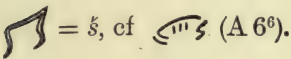
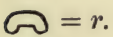
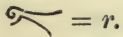
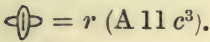
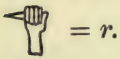
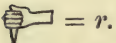
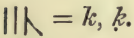
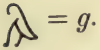
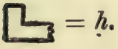
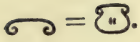
<sup>1</sup> *JRAS.* 1882, p. 417.

<sup>2</sup> Even the early terra-cotta balls in A. S. Murray, *Excavations in Cyprus* (1900), p. 27, do not help.

## Phonetic signs, with probable values.

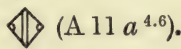
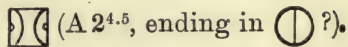
 = <i>a</i> .	 = <i>m</i> .
 = <i>wa</i> (or <i>ya</i> ?).	 = <i>n</i> .
 = <i>i</i> .	 = <i>n</i> .
 = <i>wi</i> .	 = <i>n</i> (or <i>k</i> ).
 = <i>u</i> (and <i>u</i> ?).	 = <i>n</i> (cf.  ).
 = <i>b, p</i> .	 } = <i>n</i> .
 } = <i>b, p</i> .	
 } = <i>b, p</i> .	 } = <i>n</i> (or <i>l</i> ).
 = <i>d, t</i> .	
 = <i>d, u</i> .	 } = <i>n</i> (or <i>l</i> ).
 } = <i>k, k̄, g</i> .	
	 } = <i>k, k̄, g</i> (or <i>ǵ</i> ?).
 } = <i>k, k̄, g</i> (or <i>ǵ</i> ?) [con- fused with  ].	 } = <i>r</i> .
	 } = <i>k, k̄</i> .
 = <i>k, k̄</i> .	 } = <i>r</i> .
 = <i>k, k̄</i> [confused with  ].	
 = <i>k, k̄</i> .	 } = <i>r</i> .
 = <i>kar</i> .	 = <i>š</i> .
 } = <i>l</i> (or <i>n</i> ).	 = <i>š</i> .
	 } = <i>l</i> (or <i>n</i> ).
 = <i>m</i> .	 = <i>š</i> .
	 = <i>š</i> , or <i>z</i> (z?).
	 } = <i>t</i> .

## Uncertain values.

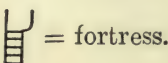
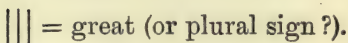
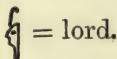
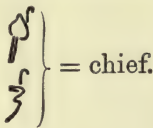
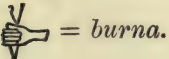
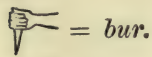
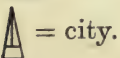
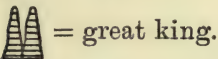
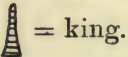
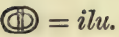



ic (word-divider).



xc





## Ideograms, with probable values.





 = Addu.


 } = Ea.  




 = priest.



 = sun.

 = ruler (read *bia*?).


 = Hani (or town?).


 = *rabbat*.


 } = son.  



 } = dedicate, &c.  




Uncertain values.



 = country.

 = *ilu*.


 = *ilani*.


 = Tarku.



 (a god-name) (cf. , &c.).

 =  = *šar*.

 = king.

 (place-name).

 (name of a people).

 (ends in ? A 6<sup>b</sup>).



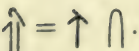


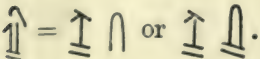


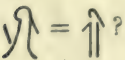


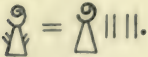


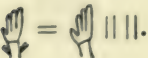
Ligatures.

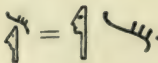


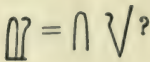


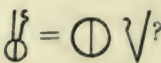


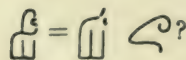












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