

lost tribes in Bactria<sup>1</sup> have, it seems, forgotten the existence of this Jewish influence, lasting for five hundred years on the shores of the Caspian, and extending much further east, for there were Jews in China, as Ibn Batuta mentions—and even as early as the 2nd century A.D. A Jewish-Chinese text of 1511 speaks of a synagogue in Pien in 1164 A.D.

But, while the fact of the eastward spread of the Jews is thus historically traced, it is certainly curious that they regarded themselves as descendants of tribes other than Judah and Benjamin. They may have been preceded by Israelites of those tribes, but it is equally probable that the reason lies in their own knowledge of Bible history, which recorded the return of the two tribes under Ezra. Nor must it be forgotten that a descendant of Asher is mentioned in Jerusalem in the New Testament (Luke ii, 36).

C. R. C.

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## THE HITTITE MONUMENTS.

SOUTHAMPTON, 23rd May, 1888.

THE series of articles in *Nature*, based on the lectures of Mr. Thomas Tyler (January, 1888), having now terminated, I would beg to be allowed to make a few remarks on his work. I have no desire to raise controversy, or to force my own views on any who may not agree with me, but, Mr. Tyler having seen fit to allude to my work, and to bring charges of inaccuracy against me, it is evident that I may be considered as bound to answer. Another reason for speaking lies in the fact that many of Mr. Tyler's comparisons are either identical with, or closely similar to, those which I have put for-

<sup>1</sup> In a very interesting paper, Sutlej Pujahs (J.R.A.S. xvi, 1), Mr. Simpson quotes from Bellew's Journal of a Political Mission to Afghanistan, 1837, who again quotes from Afghan legendary histories an account of the Tabut i Sakinah, or "Ark of the Shekinah," among the Afghans, who call themselves Beni Israil. The Hebrew history may, as he suggests, have come into Afghanistan with the Moslems, but there is another possible origin. One of the authorities quoted is Habb ben Mania, "the Son of Manes." Now the Bundahish agrees with Mas'udi in making the Turko-Tartar tribes in Turkestan, and as far as China even, Manicheans, in the 10th century or earlier, and Manes included Jewish ideas in his great syncretic system.

The word Tabut is applied by Shi'ah Moslems (who have much in common with the Manicheans) to a model of Husein's tomb.

Arks were very commonly used in Asia by the Babylonians and Egyptians and Phœnicians, as well as by the Hebrews. The Canaanites had arks, according to a text translated in "Records of the Past," as early as 1600 B.C. The Khitai in Cathay, in the 10th century, had a consecrated tent used as a temple during their war expeditions.

As regards Manes, however, it should be noted that the legendary ancestor of the Kirghiz in Turkestan was the giant called Manias.

ward at different times between 1883 and 1887; and, although Mr. Tyler is clearly acquainted with my writings, he has not thought fit in these cases to acknowledge my priority. Not that I stand alone in this respect, because in some cases discoveries by Professor Sayce have in the same way appeared in these papers without note as to their origin.

I first began to study these monuments in 1880, and have devoted nine years of leisure time to the subject with the assistance of very well known scholars; and I may perhaps, therefore, be allowed to remark generally that Mr. Tyler's papers show only an imperfect knowledge of his subject. He claims to set forth "just principles," and to found his work on "very recent" discoveries. Yet, with exception of a seal from Tarsus, which, as he allows, advances our knowledge very slightly, he refers to nothing which has not been known for many years to students of the subject, while, as regards principles, I am at a loss to understand what these are, unless they be the Pythagorean and abstract meaning of one or two emblems—a speculation which Professor Sayce last year dismissed with a curt (and I fear I must say contemptuous) note in the Academy. Mr. Tyler has picked out a few emblems here and there in an arbitrary manner. He has not adopted the only methods which seem capable of giving scientific results, namely—1st, the comparison of all the combinations in which any emblem is known to occur; 2nd, the comparison of Altaic emblems with known phonetic and ideographic values in other systems; 3rd, the use of the *sounds* recoverable from Cypriote.

Before noticing details I would ask to be allowed to explain the principles on which I have attempted the decipherment, which are either not understood or else ignored by Mr. Tyler. It appeared to me that the emblems must be treated as a cypher is treated, by observing the relations which they bear to each other in as many cases as possible—a method by which Mr. Tyler might have avoided palpable errors. It further appeared to me that a knowledge of the ideographic (or picture) value of the emblems was attainable by comparison with the use of similar emblems in other systems—such as cuneiform Egyptian and Chinese—rather than by relying, as Mr. Tyler so often does, on conjectural values based on nothing but arbitrary suppositions.

Finally, in 1883, after consultation with the lamented Dr. Birch, with Dr. Isaac Taylor, and with Professor Sayce, I came to a conclusion which the first of these authorities suggested to my mind, but which was not then held by the other two, namely, that the Hittites were a Turanian tribe, and that their language was probably akin to the old Turanian speech of Chaldea and Media.

To Professor Sayce is due the discovery that in the Cypriote emblems we have the hieratic forms of the Hittite emblems. This suggestion seemed to me at first unproven in view of the eight comparisons in Dr. Taylor's "History of the Alphabet" (six of which are, I think, wrong), and of Professor Sayce's comparisons with the early and inaccurate copies of the Hittite texts. When, however, I became possessed of complete lists of these Cypriote characters it became clear to my mind that Professor

Sayce's principle was sound, and I have now proposed forty such comparisons.

From the Cypriote emblems *sounds* are derivable, which, as Dr. Taylor saw, might serve to fix the language of the texts. It does not appear to me that Professor Sayce made sufficient use of these sounds when he attempted to decipher the texts (*see* his proposed readings in Wright's "Empire of the Hittites"), and it was through these sounds, and through an analysis of the "cypher value," so to speak, of each emblem, that I arrived at the results published last year. No scholar had, as far as I know, before that date been able to show either the phonetic or the grammatical values of the emblems in such a manner as to connect them with an Asiatic tongue. The values assigned had always been as arbitrary as was the assumption that the texts are historical.

During the present year I have published (*Quarterly Statement*, April, 1888) the results of another year's study of the question, and have shown in a manner which has met with acceptance from many scholars that the proposed values agree, not merely with the sounds of the dead languages of Media and Akkad, but with those of the living Turko-Tartar and Ugriic languages—a result further confirmed by comparative study of 30 personal and 200 geographical names from the Hittite country.

The present state of the question is this: Dr. Isaac Taylor has lately expressed his present belief that the Hittite chiefs were Turanians, and his opinion on my recent paper is most satisfactory to me.

Mr. T. G. Pinches and Mr. G. Bertin, who are probably the best Akkadian scholars in England, have told me that the important words *Ku* and *Ma* on the bilingual are (as I urged) Akkadian. Mr. Bertin, author of the "Grammar of Cuneiform Languages," just published by Trübner, states that he thinks my comparison with Akkadian represents the safest method of study, and Mr. Pinches believes that a people speaking some such tongue must have lived near Carchemish. Professor Sayce, while regarding the Vannic language (akin to Medic) as the best for comparison, is also, I believe, now in agreement as to the Mongolic type of the Kheta, and as to the agglutinative character of their language.

I might then afford to disregard the opinion of a writer who seems as yet imperfectly acquainted with the subject, were it not that he charges me with inaccuracy in a manner which I feel to be undeserved.

All that I claimed in 1887 was to have discovered the group to which the Hittite language belonged, and to have commenced the decipherment on principles not arbitrary or conjectural. The method which I adopted has obtained increasing favour with scholars, and I feel no doubt that the discovery of the language will in time lead to a complete decipherment.

As regards the proposed comparison of Hittite, Vannic, and Medic with the Georgian and other languages of the Caucasus, all that can at present be said is that these languages have been tried, and have not

served to give such results as are obtainable from the Ugro-Tartar Group.

I now proceed to substantiate the statements which I have made as to Mr. Tyler's papers, in detail. So far as he reproduces the work of Chabas, Brugsch, and Perrot, and the later publications of Dr. Sayce, which are available to the general public in Wright's "Empire of the Hittites," his work may be useful, but these are not new discoveries, since Chabas wrote his monograph in 1866, and Professor Sayce's chief discoveries date from 1876 and 1880. I am also in agreement with him in those cases where he apparently adopts suggestions of my own. As regards his original work, I think that it will tend rather to produce controversy and confusion than to contribute to the cause of science.

Mr. Tyler states that certain hypotheses ("and vagaries") repugnant to the scientific spirit have of late been advanced, yet he himself adopts, without question, two most improbable ideas: 1st. The existence of Hittites (as an empire or otherwise) throughout Asia Minor. 2nd. The Hittite origin of all monuments on which a curly-toed boot is represented. As regards the first, the Bible, the Egyptian monuments, and the cuneiform texts agree in representing the Hittites as an important tribe of Northern Syria. The monuments speak of no Hittites elsewhere, nor does Herodotus or any other ancient author. It is certain that there were tribes of kindred race and civilisation north, north-west, and north-east (and I believe also south-east) of the Hittites; but when Mr. Tyler adopts my view<sup>1</sup> as to the independence of these various tribes he might also set an example in discontinuing to use the unscientific term "Hittite" in describing the Altaic or Turanian hieroglyphs.

As regards the curly-toed shoe,<sup>2</sup> Mr. Tyler might have satisfied himself that the Egyptians wore it, for there are several pairs in the British Museum. The Etruscans and Armenians alike wore it, as did and do the Crusaders, the Turks, and the Arabs and Kurds. It is not unlike the Chinese and old Japanese shoe; and it is somewhat absurd to class ancient peoples by their boots, especially when the information relied on is so partial and misleading.

The pigtails of the Kheta were, I believe, first noticed by Dr. Birch. I called attention to them in 1883.<sup>3</sup> Mr. Tyler does not refer to the latest information (the casts by Mr. Flinders Petrie), and he should have known

<sup>1</sup> See "Heth and Moab," 1st edition, p. 422, 1883; "Syrian Stone Lore," 1886, p. 23; "Altaic Hieroglyphs," 1887, p. 8, &c.

<sup>2</sup> Dr. Sayce calls this a "snowshoe," and says it is adapted for walking on snow. It is true that it resembles a skate of old-fashioned form in having a curl in front, but it bears no resemblance to any snowshoe worn by northern peoples. These are always large flat surfaces for distributing weight. As I have attempted to walk on snow in the ordinary Turkish shoe, I may be allowed to say that it is less fitted for the purpose than an English boot. Sir C. W. Wilson put forward a much sounder comparison some time since with the boot now worn in Asia Minor.

<sup>3</sup> See "Heth and Moab," 1st edition, p. 22.

that Rossellini's drawings have been found not to be thoroughly reliable. He would then have avoided the error of reproducing these in Fig. 2. The comparison with the Manchu Tartars which follows represents the theory which I have steadily advocated now for five years. Some of his observations on the point are, however, of value. The pigtail is certainly not characteristic of female figures, as he states Professor Sayce to have asserted; what is represented in the case of females is probably a long plait of hair like that worn by the Etruscan women. The British Museum contains a magnificent example of these long braids in the terracotta figure of an Etruscan woman.

Mr. Tyler allows that the word *Sar* cannot be purely Semitic when it is suffixed. The remark is not new, but Mr. Tyler omits to point out that in Akkadian *Sar* for "prince" or "chief" has just this position (as I noted in 1886),<sup>1</sup> and that it is a very common Turanian word, whence the Russian *Tsar* is derived. In the next sentence he refers to *Bek* as Mongolian, but unfortunately forgets that the words he quotes are names not of persons, but of towns. I have recently shown<sup>2</sup> that the word occurs as meaning "fortress" or "shrine" in many Tartar and ancient Turanian dialects.

Mr. Tyler thinks that the figures at Boghaz Keni represent kings and Amazons. He does not say what nations used to stand erect on the backs of lions and of two-headed eagles. He omits the figures with wings which occur in this sculpture, and he does not refer to the well-known representations of Asiatic deities erect on various animals. As to the supposed "mural crowns," he reproduces an observation by Professor Sayce, but I confess that Perrot's original drawings, from which he gives a somewhat inaccurate sketch, do not indicate mural crowns, but only bonnets such as are still worn by Tartar women. The sticks in their hands are not curved, as he supposes, and certainly do not represent bows. As to the Amazons, even they did not ride two-headed eagles—Herodotus mentions them as Turanians in Scythia—and it is to Professor Sayce that a suggestion of their connection with the Hittites is due. I hope hereafter to demonstrate that they were Tartar queens.

The mandrake theory does not demand more than a passing notice. In none of the known systems of hieroglyphic writing does the mandrake appear as an emblem. The Hittite (or Altaic) emblem appears to mean "male deity."

As regards the bilingual, which Professor Sayce first recognised as such, Mr. Tyler's proposals are ingenious, but not likely to be accepted. In the first instance, his reading of the name of the country is clearly wrong. The characters, as Mr. Pinches and Professor Sayce have seen, read *Urme*, not *Zume*. The emblem bears no real resemblance to the cuneiform *Zu* at any period, but is clearly the cuneiform *Ur* or *Eri*, even in comparatively early forms.

The suggestion that these hieroglyphs represent a Semitic speech

<sup>1</sup> "Syrian Stone Lore," p. 16.

<sup>2</sup> Palestine Exploration Fund, *Quarterly Statement*, April, 1888, p. 82.

contradicts the opinion of all who have given much attention to the subject. Semitic writing, as known to us, is syllabic or alphabetic—as is natural for an inflexional language—and not ideographic, as is natural to non-inflexional speech. A word like *Saeer*, containing a strong guttural, would probably be represented by two syllables, and if the second emblem on the bilingual be compared with the earliest form of the emblem *dim*, used in Akkadian, it will be seen that this value is more probable than is the tri-syllable *Kutimme*. As to the cone, Mr. Tyler follows Professor Sayce in rendering it “king,” but regards the double cone as meaning “people.” This is purely arbitrary, since in no known hieroglyphic system does any parallel exist, whereas, if it mean “country,” which would upset Mr. Tyler’s reading, it may be compared with emblems for country and mountain in Egyptian, in cuneiform, and in ancient Chinese.

Mr. Tyler is, I think, wrong in regarding the fifth emblem as unique, since it resembles an emblem often found on other texts; and certainly it in no wise suggests an idiogram for country. As regards the sixth, he adopts a comparison which I suggested in 1887<sup>1</sup> for the first time, only he gives the Assyrian value instead of the Akkadian,<sup>2</sup> in which he is, I think, wrong, since the Akkadian is the older, and because a series of strokes represents the plural in other hieroglyphic systems. According to the values which I obtain from Cypriote sounds, the Hittite emblems (so called) read *Tark-dim-Ku-ma-erme*. *Ku* and *Uk* occur for “king” in Akkadian, according to many scholars, and recall the Chinese *Chu* (older *Ku*) and the Tartar *ok*, meaning “Lord.” *Me, Ma, Mu*, is a common word for “country” in Ugriic speech. *Me*, for the plural, is known in the Medic, and, according to Professor Sayce, also in Akkadian.

Mr. Tyler is also unaware of the meaning of the word Tarku, or Tarkon. It is a common Tartar word for chief, and has been traced from Siberia as far as Italy, where Dr. Taylor has recognised it in the Etruscan Tarquin.<sup>3</sup>

As to the idea that some of the emblems on the boss are inverted, it may be noted that ancient scribes were not accustomed to write upside down as a rule.

Mr. Tyler regards the comparisons of Cypriote and so-called Hittite as “visionary.” I must leave him to settle this with Mr. Perrot, Professor Sayce, Dr. Taylor, Dr. Deecke, and the other well-known scholars who hold the opposite view. The resemblances are often so remarkable and exact in detail as to leave no doubt on the mind. It is natural to suppose that the Cypriote or Asianic script, belonging as it does to the same country in which the older Altaic hieroglyphs are found, bears to them the same hieratic relation that the later Egyptian writing does to the monumental hieroglyphics of Egypt, or that the cuneiform

<sup>1</sup> “Altaic Hieroglyphs,” Plate IV, Fig. 10.

<sup>2</sup> “Altaic Hieroglyphs,” p. 161, Plate II, Fig. 14, p. 48.

<sup>3</sup> See Palestine Exploration Fund, *Quarterly Statement*, April, 1888, p. 78.

bears to the archaic Akkadian ideograms. We have also transitional inscriptions found of late, which serve to connect the early and the later incised forms.

Mr. Tyler states that I have not given the Hittite emblems in my latest work "with such essential accuracy as is desirable," but he cannot substantiate this assertion. I have studied the original monuments and the casts, and have drawn the emblems from these. Mr. Tyler's rude sketches are often clearly from drawings and photographs, and I am prepared to point out the inaccuracies of these sketches, and of his reproduction of the Yuzghád Seal, of which a good photographic reproduction was published in 1886.<sup>1</sup>

As regards the texts occurring on statues of the gods, I adhere to my previous statement. At Boghaz Keui some figures, not reproduced by Mr. Tyler, are winged. The others, standing on animals, have been recognised as deities by Professor Sayce, and any student of ancient symbolism will know that he is right. A text from Jerabis, not yet in England, occurs on a plaque presenting a winged female figure. One of the figures at Ibreez certainly represents a deity. The Babylonian bowl is generally admitted to have on it a votive text, and the question of religious connection is thus reduced to a few texts which present a very similar group of emblems to those found on the texts above noticed.

Of course the "analogy of (historic) Assyrian inscriptions" cannot show that other texts must be historic, for this is purely begging the question. There are innumerable ancient texts, Akkadian, Egyptian, Assyrian, Etruscan, and Greek, which are votive, not historic. The Akkadian magic texts, as I have before noticed, present a very close parallel to my proposed readings of the so-called Hittite texts. The great stone lion in the British Museum, which I compared with the Lion of Merash (a comparison which Mr. Rylands afterwards adopted), bears an invocation to Istar; and I am unable to see that "heads of oxen and asses" have any necessary connection with war.

Mr. Tyler has not thought fit to explain my views or to state the language which I recognise on these monuments. He selects one of the most fragmentary and defective of the texts, and gives the impression that the gaps on the monument are gaps in my decipherment. Even then there is more that is consecutive than in his own arbitrary selection of portions of certain groups. If the order of the words as I place them were in accordance with English syntax, that would be a certain mark of ignorance on my part. Turanian syntax is entirely different from either Aryan, or Semitic, or Egyptian syntax. We have to deal with a language of suffixes, with a verb placed at the end of the sentence, with post-positions and affixes. It is because I am able to identify these in their proper grammatical position, and because I have recognised (as Professor Sayce admits) "packets" as in agglutinative speech, and small suffixes with larger strong roots (as he also admits), that I

<sup>1</sup> Proceedings Soc. Bib. Arch., November, 1886. Note by Mr. E. A. W. Budge.

feel safe in identifying the general structure of this new dialect of Turanian speech.

Mr. Tyler states that "the Hittite inscriptions are in the main ideographic or pictorial." That they are not purely pictorial is very easily proved. That they are ideographic is one of my chief contentions, and indeed I do not feel sure that I have not rather exaggerated than otherwise the exclusively ideographic value of some of the emblems.

As to the Yuzghâd Seal, I do not think any scholar will suppose it to represent the "successful chase of the stag," and Mr. Tyler's reproduction contains errors on which he bases assertions. The seal, like the Tarsus Seal, is one of a large class, possibly of Turanian origin, and probably used as amulets. The Babylonian sun deity and the bull-horned Ea occur on it, but it is as doubtful if any of the emblems have value as ideograms as in other cases. The supposed "trident" does not even in Mr. Tyler's sketch (and still less on the original) resemble a trident at all, but a tree. As regards the curious emblem (Fig. H.), Mr. Tyler adopts my suggestion that the crescent moon is intended,<sup>1</sup> but there is no reason why he should select the triangle only out of the several phonetic emblems which occur with it. The supposed "baby" on the Yuzghâd Seal is not a baby on the original, nor do monumental females hold unfortunate infants by the neck, but nurse them in their arms. I am not acquainted with ancient races who adored triangles, though the cone was a Phœnician sacred emblem.

Mr. Tyler's suggestion to read all the texts "hind before" will not meet with approval. All scholars are agreed that the Hittite emblems face to the beginning of the line (a comparison of H. 1, 2, 3, 5, and J. 1, is sufficient evidence), just as in Egyptian or in the early Akkadian cuneiform. It follows that all Mr. Tyler's attempts to read are vitiated by his error in reversing the texts, as well as by his arbitrary selection of a few emblems, which is contradicted by comparative study of the groups.

Mr. Tyler regards the emblem, which he incorrectly describes as a parallelogram and two squares (which is not the normal form), as "the sign for plurality." Further study will show him his error. The emblem is known in more than ninety cases, and is very frequently a prefix. In no Semitic tongue is the plural prefixed, nor is the plural emblem prefixed in any Asiatic system. It is true that Bantu languages have prefixed plural sounds, but Mr. Tyler will not find such a language in Western Asia. Clearly he is wrong about a very important emblem, and wrong because of insufficient comparative study. The Cypriote *Ne* so exactly compares with the emblem in question as to make it certain that the common demonstrative and personal pronoun of that sound, found in numberless Turanian dialects, ancient and modern, is to be recognised, and this identification, which I proposed last year,<sup>2</sup> has been admitted to be probable by various scholars, including Professor Sayce.

<sup>1</sup> "Altaic Hieroglyphs," p. 182.

<sup>2</sup> "Altaic Hieroglyphs," p. 39.

That the closed hand<sup>1</sup> represents "power" is also not a discovery of Mr. Tyler's, but a remark which I believe I was the first to make, and he cannot claim priority in fixing the value of the "foot" for going.<sup>2</sup> I might add a great deal in confirmation of the ideographic values which Mr. Tyler here gives—a year later than myself. The idea that the figure (Fig. K.), which commences several texts, represents "a servant," rather than a king, also suggests an observation which, I believe, was not made by any previous writer, but which I have somewhat lengthily elaborated, to the effect that this attitude signifies "supplication."<sup>3</sup>

The suggestion that the Babylonian bowl was carried to Babylon as a trophy I made in 1886,<sup>4</sup> and I am not aware that it is to be found in any book before I first made a note on the subject. As to the supposed "agricultural implements," I am familiar with the threshing sledge used in the East, and see no resemblance to the Hittite emblem, nor is Fig. N. at all like a plough, or like any ancient representation of a plough. If Mr. Tyler had compared the various recurrences of this emblem, he would not have selected this cut. The figure above (M.) is like the oldest form of the cuneiform *Ri*, and like the Cypriote *Ri*. It occurs in the name of a deity on the bowl, and there was a well-known Akkadian deity called *Ri*.

As regards the Shaduf, the suggestion is due to Professor Sayce, as should have been acknowledged. The explanation of the final group of the Hamath stone No. 3 is, to my mind, most improbable. The emblems are really used phonetically, and the supposed ideographic value is based in part on a copy from a very imperfect cast. The very abstract and philosophical meaning attributed to the emblems is not supported by our knowledge of other hieroglyphic systems.

As regards the emblem which Mr. Tyler supposes to represent Ashtoreth (and apparently he thinks that all gods had this name), no sound argument is given in support, and the group, Fig. Q., No. 2, is not correct. The identification of the sacred tree is due to Professor Sayce. The identification of the heads below, as representing "spiritual beings," agrees with a view which I proposed last year, only I regard them as demons and Mr. Tyler as gods.<sup>5</sup> The gods are not, however, so represented by ancient peoples, whereas similar heads representing demons are often found in Etruria and are known in Chaldea. The opposed attitude is also that in which demons are often represented, as I have long since remarked.<sup>6</sup> Horns are proper to demons in many ancient systems.

Mr. Tyler takes up Professor Sayce's discarded view that the Hittite emblem for deity really represents city. There is an argument in favour of its meaning deity which he overlooks. The very group which he

<sup>1</sup> "Altaic Hieroglyphs," pp. 52-192.

<sup>2</sup> "Altaic Hieroglyphs," p. 54.

<sup>3</sup> "Altaic Hieroglyphs," p. 53.

<sup>4</sup> "Syrian Stone Lore," p. 26.

<sup>5</sup> "Altaic Hieroglyphs," p. 206.

<sup>6</sup> "Altaic Hieroglyphs," p. 93.

sketches (Fig. R.) occurs on a seal, with a star (the common Akkadian emblem for deity), instead of the conventional eye.<sup>1</sup> As regards the central emblem, it is not always lozenge-shaped; and, as regards the eagle, Mr. Tyler adopts my view that it is the celebrated sun eagle, but does not acknowledge the loan.<sup>2</sup>

The remaining groups require no notice. They are read backwards, and separated from their context in an arbitrary fashion. The value *su su* is given to the emblem, which a careful comparative study would show Mr. Tyler to be the *Me* of the bilingual, and two ideographic values are borrowed from my system without acknowledgment. The remarks on Fig. U. are due to Mr. Tyler's having worked from a photograph. Had he carefully inspected the original text, he would have seen that the weathering is represented on the photograph in a misleading fashion.

The Tarsus Seal belongs to a large group. Mr. Tyler should have mentioned the Lydian and Cappadocian cylinders figured by Perrot, which are much more instructive, but in the same style, representing deities of Turanian or Semitic origin.

As regards the supposed triangle, it is, I think, a cup, like that often held by deities. It is unfortunate that in Fig. Y. a broken cuneiform emblem is reproduced lying on its side. There are numerous complete examples of this archaic form known, and in these a stroke, broken off in the specimen selected, exists, and shows that it is not a triangle, but perhaps, as Mr. Bertin thinks, a cup.

The comparison of the ankh with a Phœnician and a Hittite emblem is, I think, sound; I first proposed it in 1863, and have compared the Phœnician and Egyptian also in 1886.<sup>3</sup>

It would have been more to the purpose, had Mr. Tyler noted that the Cypriote value is *Er* or *Ra*, which as a Turanian word means "power."<sup>4</sup> The triangle has clearly no connection. The Indo-Scythian coin is also very different, and these coins are much too late to compare safely.

Mr. Tyler doubts the age of the texts. He forgets that in one case at least there is evidence that the text is older than 1340 B.C.<sup>5</sup>

It is no great pleasure to me to write this criticism; but before charging me with inaccuracy Mr. Tyler must look at home. He has not fairly represented my method or principles, and his proposals, as I have shown, repeat those which I have made ideographically in the majority of the emblems of which he treats. I have much yet to say on this important subject, but the above is a sufficient answer to Mr. Tyler's papers.

C. R. CONDER.

<sup>1</sup> "Altaic Hieroglyphs," p. 245.

<sup>2</sup> "Altaic Hieroglyphs," p. 82.

<sup>3</sup> "Syrian Stone Lore," p. 72.

<sup>4</sup> Palestine Exploration Fund, *Quarterly Statement*, April, 1888, p. 100.

<sup>5</sup> "Altaic Hieroglyphs," p. 156.