

## The Hittite Inscriptions.

### SECOND ARTICLE.

BY PROFESSOR FRITZ HOMMEL, PH.D., MUNICH.

IN view of the notice I gave in last month's EXPOSITORY TIMES (p. 424) of a fuller article on the above subject contributed by me to the *P.S.B.A.* (which, unfortunately, owing to the necessity of casting new type, will not appear before July), it would not have been out of place if Professor Jensen had delayed his reply to me till the publication of that article. In this way he might have saved many of his remarks. He was aware that such an article was forthcoming, but he has preferred to reply at once.

It would only weary my readers (who, besides, without seeing a facsimile of the disputed passages from the inscriptions, are not in a position to form an independent judgment, but will look at the subject through either Jensen's spectacles or mine), if I were to go into such detail as Jensen, and were to give by way of reply my argument as it will presently appear in the *P.S.B.A.* I waive entirely such a course of procedure, merely remarking that anyone who really desires to learn the true state of the case, should turn to my article when it appears, and he will be convinced that, stating the result in the way most favourable to Jensen, the whole question is still an open one. But Jensen's 'Reply' necessitates my correcting also in these pages a series of directly misleading assertions, in regard to which the judgment of the readers of THE EXPOSITORY TIMES cannot be a matter of indifference to me.

In the first place I am entitled to ask that matters shall not be introduced which have absolutely nothing to do with the position of the Hittite question. I had spoken of Jensen's false view about Aegeo-Armenians (= Lycians) and Aegeo-Zagrians alongside of Hatio-Armenians, as well as of his utterly impossible, as it seemed to me, analysis of Atargatis-Derketo, and then proceeded to remark that I could enumerate many other 'absurdities' of a like kind. Jensen interprets this to mean that I call all his results that are not acceptable to me 'absurdities,' and denies to me, on the ground of my book, *Ancient Hebrew Tradition*, the right to judge what is absurd and

what not. But what connexion is there in the world between the deciphering of the Hittite inscriptions and my book, about which, by the way, Zimmern, the eminent and sober Assyriologist, who on other points is so readily cited by Jensen as an authority, passes, in spite of his Wellhausenian standpoint, a very different judgment from Jensen?<sup>1</sup> It is surely pure spite to introduce in this connexion the saying, 'He who sits in a glass house ought not to throw stones.' In 1892, in my articles on the Astronomy of the Ancient Chaldeans (in the periodical *Ausland*), I showed the baselessness of whole parts of Jensen's *Kosmologie*, yet it would be ridiculous if I were now to maintain that on that account the author of *Kosmologie der Babylonier* is incompetent to decipher the Hittite inscriptions. My objections to the correctness of a great many of Jensen's Hittite results are, as ought to be self evident, wholly uninfluenced by what he has written formerly on other subjects. But the reader will now at least see clearly how Jensen everywhere introduces the personal element and can discuss nothing purely objectively, *sine ira et studio*. Thus he constantly attributes the worst possible motives to his scientific opponents, and supposes, for instance, that the sign read by him as 'Cilicia,' but taken by me for a serpent and interpreted as ideogram for the god Tarku, was intentionally<sup>2</sup> reproduced by me incorrectly as

<sup>1</sup> Cf. the account of Zimmern's review of my book (his judgment is to the effect that in spite of what he considers the failure of my arguments against Wellhausen, the book contains a number of noteworthy new results) in THE EXPOSITORY TIMES, ix. 448f. Jensen has not realized up till now the bearing of my discussion of the West Semitic in distinction from the genuine Babylonian method of forming names (cf. *ZA*, xiii. 345); how then could he judge objectively of my book?

<sup>2</sup> [Perhaps it ought to be mentioned that the sentence written by Professor Jensen on this point was added by him when returning his corrected proof, just in time for the press. He suggested that possibly the true explanation of the deviation from the correct symbol was that given above by Professor Hommel, and directed the sentence to be deleted if Professor Hommel had meanwhile taken exception to the form in which the sign had been printed in his article. As

W (see p. 369 of May number of EXPOSITORY TIMES), whereas in reality the sign in question has 'the same appearance on the left as on the right.' But in my MS. I gave distinctly W, for which the printer, having no type corresponding, substituted the above form, which Jensen rightly calls incorrect. But even still it is incomprehensible to me why this ideogram, whether it is to be read as 'Cilicia' or 'Tarku,' should not stand for an arched serpent, especially as in certain inscriptions (after photographs and also, as it seems, 'squeezes') the one end, while of the same height as the other, appears to be somewhat thicker than it, so that we should clearly have indicated on the one side the head and on the other the tail. And when Jensen seeks to ridicule my suggestion that δράκων is identical with *Tarku* (*Tarkond*, cf. δρακοντ-), he only shows his complete ignorance of the history of the Greek language, in which primitive Asia Minor (resp. Scythian) loan-words need constitute no anachronism; on the contrary, it would be positively strange if Greek showed no trace of such foreign elements. Besides, even in Homer, δράκων is a word with a strong mythological tinge, and for the metathesis (*Tarku*, δράκων) we may compare Τροκων side by side with Ταρκων—in Cilician proper names.

Nothing is more difficult than a correct analysis of Asia Minor proper names, therefore in my former article (p. 368) I left the possibility open to break up Syennesis into *Zua-nza-s*,<sup>1</sup> instead of treating it as equivalent to Zuanna-s (from Zuarna-s?). The idea that I could be ignorant of names like *Tarku-nazi* is perfectly ludicrous; in my treatise, *Hethiter u. Skythen* (Prag, 1898), I compared not only Tarkhu-nazi with Syennesis, but also the Lycian Τεύνασος (in inscriptions *Tävenäzoi*), and again not only Νήσιος (p. 28), but also Μολα-νείσα and Νεισίων. But all these analogies go to prove first of all that Σέννεσις (whether the second *s* be an original ending or an enlarging suffix, or a radical part of a word νεσ [nazi]) is formed quite in the same way as dozens of Asia Minor personal names, and cannot have been a divine or kingly title. Compare also the name of the Cilician admiral of

no such protest had been received, and as there was no opportunity of comparing Professor Hommel's MS., it was decided to let Professor Jensen's sentence stand.—EDITOR.]

<sup>1</sup> My words were: 'or, if one does not accept this, the cuneiform Zualzas would be identical with Syennesis.'

Xerxes, namely, Syennesis, son of Oromedon, (Herod. vii. 98), a circumstance which by itself makes Syennesis as a kingly title as unlikely as possible. The idea, again, that the Median *Zualza-s* is identical with *Syennesis*, notwithstanding Jensen's signs of exclamation (which in his vocabulary may stand for something like 'horribile dictu'), is not so completely out of the question, since in other instances also many Iranian and especially Median personal names have close points of contact with similar names from Asia Minor. If Jensen had studied my *Hethiter u. Skythen* carefully, he would not have found the above supposition so strange. In general it may be noted that the numerous parenthetical points of exclamation are an odious accompaniment of Jensen's style, which without this is far from an agreeable one. How does Jensen know, for instance, that *Kode* is a special 'Egyptian (!) term' for N.W. Syria and the bordering Cilicia? That to the Egyptians it suggested their word *kote*, 'circle,' is manifestly to be explained solely as a case of popular etymology; in the Tell el-Amarna tablets the district in question, which in all probability included also Cilicia, is called *Kutiti* (No. 79, reverse 13, between Khatti and Mitanni, as, with the Egyptians, between Kheta and Karchemish); perhaps one should recall כתיים and *Kataonia*. Here too, then, Jensen might have spared his sign of exclamation.

As to the king of the 'silver boss,' whose name is incorrectly read by Jensen as *Shilkuaššeme*, it is quite true that we find distinctly *Tar-ku-u-Mu-me* (resp. *Sil-ku-u-Mu-me*), but it is quite evident that this is to be read *Tarkudimme*. Jensen gives a melancholy evidence of his poverty as an Assyriologist when he writes: 'Now even Hommel perceives that before *me* there is no sign *DIM* but *MU*, yet in order to rescue Tarkondemos, he ascribes to it the phonetic value *dim*, which this sign has nowhere else.' Why, the very name of the sign *mu*, namely, *mu-hal-timmi*, shows that *mu* has also the values *hal* and *tim*; the word marked in the Great Syllabary (line 95) *u-dun* is written *u-mu* [read *u-dun*]; the value *lim*, 'year,' is a dialectic variant of *dim*; and, finally, the ideogram *mu*, when it signifies 'bread,' has the value *dim* (curtailed from *hadim*, *adim*), as is shown by *mu-hatimmi* (written *amelu*, 'man,' and *MU*) = 'baker.' With the exception of the last word, which was only quite recently explained by Zimmern, and the name *mu-hal-timmi*, which was first made known to us by


Delitzsch, all this was already to be learned from my *Sumerische Lesestücke*. At most one could still read *Tarkūlimme*, which, however, comes to the same thing (cf. *Tugdammu* and *Δύδαμυς*), the name being in that way, too, identical with *Tarkondemos*. Since, by the way, the sign *u* has also the value in Sumerian of *un*, a reading *Tarkū-un-dim-me* would not be quite beyond the bounds of possibility.

That Professor Ramsay rightly defends Professor Sayce against Professor Jensen's 'extraordinary misrepresentations' will be clear from what follows. It is nothing less than scandalous, the way in which Jensen strives to minimize the great merits of Sayce in the matter of the analysis of the Hittite inscriptions. It is a fact that Sayce was the first to discover in the inscriptions the ideogram for 'king,' and even if it should turn out that the sign in question on the 'silver boss' is merely a synonym of the ideogram for 'king' (and not the latter itself), this would not alter the fact stated. Another fact passed over in silence by Jensen in his latest publications, is this, that Sayce correctly recognized the sign for the nominative (whether this is a mere determinative, or, as I still follow Sayce in holding, the letter *š*).

Further, Sayce rightly deduced the sign for the syllable *mi* or *me*; whether the first or the second Babylonian sign *me* on the 'silver boss' (*Tarkū-un-dim-me sharru mât alu me-tan*, or, as was formerly read, *sharru mât Er-me-e*) answers to the particular Hittite sign has not even yet been made out with certainty, for the Hittite signs might read either *Tarkū-dim-me metan* + land + lord, or *Tarkū-dim me-tan* + land + lord. But now these three discoveries of Professor Sayce formed the basis of all further progress in the analysis of the inscriptions; in particular the first two must be regarded as a kind of key, and with this key alone Professor Jensen himself worked at first. For instance, the *x-y-z-x* (Jensen's *Syennesis*, my *Desandas*) could be recognized as a title, and not as a place name or the like, solely because it is followed by the above sign for the nominative (Ivritz 1; Bor, l. 1; Bulgarmaden, l. 1). Nor, apart from Sayce's discoveries, could Jensen ever have reached the conclusion that the group at Bor, standing between the title *x-y-z-š* and the ideogram for 'king' must stand for 'name of land (or city) + king,' or that the group before 'king' in the inscriptions of Jerabis stands for *Kargmi* (Kar-

chemish). When Jensen (p. 410 of his article last month) is completely silent about Sayce's discovery of the *nominative* (the question whether the particular sign is a real nominative *ending* or simply a *determinative* does not affect the importance of this discovery as a key to further results), an energetic protest must, in the interests of truth, be raised against such a procedure. In face of such a deliberate ignoring of facts, I can regard Jensen's expression of the desire 'that the true state of this important question may be learned in England as well as in Germany once for all' as only a bit of empty phrasing.

That a certain sign stands for 'queen' had already been recognized by Menant,<sup>1</sup> and seeing that at Fraktin it stands, along with other signs, to the right of a goddess, it did not need the exercise of any great acuteness to see in it a title of the great goddess.

If Jensen takes it as an indisputable fact that a variant of the well-known 'god' determinative discovered by Sayce is a 'land' determinative, why is it that this sign, in which Jensen thinks, however, that he recognizes the picture of a *city* (p. 406), is wanting precisely before the groups which precede the 'king' sign, and which Jensen interprets as 'Karchemish' and as 'Tarsus'? The fact, by the way, that the ideogram  which Jensen (in consequence of his false interpretation of this determinative) rendered by 'Cilicia' is really the name of a *god*, is now raised above all doubt by a seal cylinder published in Ball's *Light from the East* (p. 36), about which I will speak within the next few months in the *P.S.B.A.* As regards Mr. Ward's seal cylinder, it is evident at once that a legend such as 'Of the brave *x* of Cilicia and Arzapi (?) *y* worshipper' is quite improbable and out of all analogy, nay altogether impossible and inconceivable. On the other hand, two divine names and perhaps, further, the name of the author of the seal (what the third line means is, with the present means at our disposal, not yet made out), are something quite natural, and supported by numerous analogies.

Why the *Tarkū-nazi* suggested by me in Ordasu must be identical precisely with the *Tarkū-nazi* of Milid known from the Assyrian inscriptions, I cannot see; the reference may, of course, be quite as well to an earlier king of this name. Moreover, since we can only conjecture the phonetic value of the sign that follows *Tarkū*, the additional possi-

<sup>1</sup> *Memoires*, etc. Tome 34, ii. (Éléments du syllabaire Hétéen), Paris, 1895. (Première Lecture, Juill.-Sept. 1891).

bility of reading Tarku-tusa (cf. Girba-tusa of Ramses, and the Lycian Ova-tisäh, Oútraois) must be taken into account.

Herewith I take leave of this unpleasant subject. As soon as my paper appears in the *P.S.B.A.*, I trust that those who can form a judgment regarding it (I have in view especially Professor Sayce, and Mr. Rylands, Mr. Ward, and Mr. Ball) may give expression to such, and perhaps state their opinion, amongst others, to the readers of THE EXPOSITORY TIMES. Whether I have hit the mark with *Desandas* can be decided only when more materials are at our disposal. But it appears to me to be beyond all doubt that the serpent ideogram is a divine name, and the sign of the hand<sup>1</sup>

<sup>1</sup> On the other hand, it still appears to me to be extremely doubtful whether the sign from Boghazköi, claimed by

the general term for 'god' (probably *ghirpa*), and that both are used (as the first element in the compound) to form numerous proper names. As to the rest, let Jensen go farther on his way without deviation and always 'gain deeper insight into the contents of the inscriptions,' I am certainly the last to grudge him this pleasure. But I must enter my protest against the notion that my explanations are for the most part based upon his 'decipherments.' In my article which has been since April last in the hands of the editor of the *P.S.B.A.*, I have conscientiously noted all the advances which Jensen, founding upon Sayce, has made in the *analysis* of the inscriptions.

Jensen for a symbol of the great mother, really represents a clenched fist. I reached my explanation of the outstretched hand (*one* sign with many variants) in quite a different way.

## At the Literary Table.

### THE BOOKS OF THE MONTH.

FROM the office in Edinburgh (40 Hanover Street) we have received *The Monthly Visitor* (illustrated) for the year 1898, edited by Mr. R. Henderson Smith. *The Monthly Visitor* makes progress. True as ever to the heart of the evangelical faith, it is more alive to the variety of men's minds, more sensitive to the variety of avenues that lead to man's need of the gospel, than it used to be. There are those still who having named the name of Christ scoff at tracts. These are tracts;—it is sheer ignorance that would allow a Christian to miss their interest and usefulness.

THE GOSPEL PROBLEMS AND THEIR SOLUTION. BY JOSEPH PALMER. (*Allenson*. Crown 8vo, pp. 395. 6s.)

'Fools rush in where angels fear to tread.' That is much too severe of course, and we hasten to take the edge off it. But it is not possible after all that has been done on the Synoptic Gospels that Mr. Palmer should be able to settle the unsettled problems with a single sentence of bold type. He has keys—a master key, and three special keys. And these keys open all the locked doors and disclose all the hidden

treasures. There is no doubt that Mr. Palmer has hit upon a most seductive line of argument. Some day soon we should like to show the points of it. But there are things left out, and there are things that will not be explained in this way. Nevertheless, the book deserves to be read, and we can promise anyone who takes to the reading of it that in Mr. Palmer's hands the study of the Synoptic Problem will keep them wide awake.

A PRIMER OF FREE CHURCH HISTORY. BY A. JOHNSON EVANS, M.A. (*Allenson*. Crown 8vo, pp. 144. 2s. 6d.)

'Free Church History' means the history of the Free Churches, as they are coming to be called in England; that is to say, the Churches that are not established by the State. The name may still sound sectarian, the book is not sectarian. Mr. Evans has been scrupulous to discover fact and write impartial history. The only offence that his book can give is by its brevity. We could take much more with profit and with pleasure. It is too short, indeed, for justice either to the subject or to the writer. But it is welcome and well done.