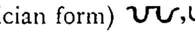
# The Hittite Inscriptions.

By Professor P. Jensen, Ph.D., Marburg.

When in the June number of The Expository Times I declared my intention to decline for the present any further controversy with Professor Hommel, I was entitled to expect that the latter would do his best to facilitate my purpose by confining himself, in any polemic directed against me, strictly to objective facts. This expectation has not been realized in his 'Reply' in the July number, and hence I find myself compelled, to my sincere regret, to ask once more for the kind indulgence of the Editor and the readers of The Expository Times while I devote a few words more to our *querelles allemandes.*

If Professor Hommel had observed more accurately what he himself, what Professor Ramsay, and what I have said hitherto in The Expository Times, and what Professor Zimmern has said about his *Ancient Hebrew Tradition*, he might have spared a good many of his strictures upon me, or would have in some instances expressed himself somewhat differently. He would have seen—(1) (cf. p. 459) that *no human being*, without the aid of his (Hommel's) commentary, could have referred the 'absurdities' of which he spoke in the May number (p. 371) to anything else than *the whole* of the foregoing remarks directed against my views; (2) (cf. p. 459 f.) that in my words on p. 410 of the June number *no* suspicion is implied; (3) (cf. p. 460) that of course I did *not* say that the name *Tarkhunazi* was unknown to Hommel; (4) (cf. p. 460) that I *did not assert* that, but on the contrary left it doubtful whether, the Egyptian *Ḳode* includes Cilicia; (5) (cf. p. 461) that Ramsay's expression, 'extraordinary misrepresentations,' had *not* the reference that Hommel gives it; (6) (cf. p. 461) that I am *far from* regarding the discovery of a hieroglyph for 'queen' (or 'mistress') as anything considerable. I have now learned for the first time from Hommel that Menant in this particular has preceded me. Some day, perhaps, it may be recognized that I was justified in not troubling about Menant's work on the Hittite inscriptions. I may, however, remark once again that 'Si duo faciunt idem, non est idem.' For the hieroglyph in question I deduced the meaning 'queen' on the strength of a passage, which Menant, in his paper, could not yet turn to account. So little, however, does his interpretation of the sign rest upon any logical ground, that immediately following in his list he provisionally renders the same sign as 'high priest.' But as to the distinction between groundless assertions and logical conclusions, Hommel and I are, to be sure, not at one. (7) (cf. p. 461) that in the passage in my article cited by Hommel I did not allege that the famous determinative preceding 'Cilicia' is the picture of a city, but of a city *along with the surrounding district*; (8) (cf. p. 462) that I did *not* say that Hommel's explanations are for the most part based on my decipherments, and indeed could not have said this, seeing that I know nothing about the article he announces as forthcoming in the *P.S.B.A.*, although I certainly believe that hitherto *he* has not furthered the work of decipherment, and that he can further it only by continuing in the future, as he has done in the past, to adopt from me one result after another; finally, (9) (cf. p. 459) that my judgment of his *Ancient Hebrew Tradition* stands in *no* contradiction with that expressed by Zimmern. The latter recognizes 'Gutes und Neues' in the book, and so do I in my critique, which will appear shortly, in which I say: 'The expert and he who is competent to judge may learn from it in many ways, and often derive stimulus to fruitful reflexion.' But Zimmern considers that the conclusions which Hommel draws from his materials are of very different values, and at the same time regrets that he 'has not restricted himself to submitting his materials *sine ira et studio*, but has at the same time proceeded to use these with an avowedly apologetic aim,' and it is the way in which Hommel from the first line of his book to the last has done this, while only too frequently he presents the airiest speculations as irrefutable facts—it is this which I have called 'absurd,' and it seems to me that I have at least as much right to do so as Hommel has to apply that term, in view of his own interpretation, to a modest *suggestion*—which, by the way, I have since abandoned—of mine concerning *Tarkhu* and *Atargatis*, and to a very well-grounded conclusion in favour of the existence of a '*Teshup* population,' as distinguished from a '*Tarkhu* population,' in Northern Syria and the adjacent districts. I have called attention to the fact that in numerous personal names compounded with the divine name *Tarkhu*, the divine name uniformly comes first, while in others—the number of which has meanwhile been somewhat increased by fresh discoveries—compounded with *Teshup*, it stands uniformly in the second place. Further, it is established that, for the periods of time accessible to us, the first class of names is unexampled to the east of the Euphrates, and the second to the west of the Taurus. Hence I conclude that two distinct populations were found in Northern Syria and the adjacent districts. If Hommel calls *that* 'absurd,' his terminology, as happens, indeed, in many other instances as well, is different from what is generally current. If *such* conclusions are absurd, the same term must be applied to a great many scientific inferences in which one has till now seen an enrichment of our knowledge. And which of Hommel's own conclusions, then, would not be absurd? By the way, I now learn from Hommel for the first time that by his arguments in the *P.S.B.A.* (xix. p. 79 ff.) he has overthrown my position. I have even failed, after repeated reading of these, to discover how, even if it be granted that we are to take seriously such identifications of Hommel (*loc. cit.*) as *Mars-Mavors* (genitive *Martis*, *Mavortis*!) = *Maura* in Hittite (!) *Maura-ser-*. Or, does he hold to *Ḫattu-shar*, a form of name which goes back to the authority of Winckler, but is as good as impossible, in preference to the incomparably more probable *Ḫattuḫi* (or *Ḫattuṭi*)? I should have thought, by the way, that the first of all requisites for fair controversy was a conscientious statement of how far one has evidence for the counter positions he maintains.

As to the extremely meagre objections Hommel has to offer to my 'Reply,' the following may be said in brief. No one except Hommel (see p. 460) has hitherto inferred from names like *Tarkhu-lara*, Ταρκυ-αρι-ς, Τροκο-αρβασι-ς, Ταρκυ-μ-βι-ης, Ταρκο-ν-δημος, etc., nor could he have inferred that the first common part of the names, instead of being *Tarkhu-*, Ταρκυ-, Τροκο- (**Trkho-*) is *Tarkond-*. The form *Tarkond-* is—I may mention for the benefit of readers of THE EXPOSITORY TIMES not acquainted with the facts—created by Hommel *ad hoc*, in order to lend more weight to his famous identification of the word with δράκων, δράκοντος. What name is one to give to such a procedure, and what is one to say by way of answer to it?

On none of my casts, squeezes, or photographs have I been able to discover *any trace* that the hieroglyph for 'Cilicia' has not precisely the same appearance on the right and on the left, or that it shows on the one side a thickening which might represent a serpent's head, and on the other a thinning which might indicate his tail. And of the different forms of this ideogram that which is given by Hommel with approximate correctness is—according to my inferences—not only the oldest but at the same time also that which is most like the figure of a serpent. There are forms such as (the Cilician form)  and  which do not at all resemble a serpent. By this I do *not* mean to say that the hieroglyph may not have been originally the picture of a serpent. But I may venture the assertion that this hieroglyph itself does not justify such an assumption.

As to *Syennesis*, Hommel, then, frankly admits (p. 460) that formerly he found, with me, in the second *s* of the word a radical consonant. But in his first article he spoke of this view of mine as extremely improbable. Why this change of mind? It appears—for it cannot be well conceived of otherwise—to have unconsciously arisen *along with* or *through* the conviction that I had made a mistake in my reading of x+y+z+x as = *Syennes-i-s*. But in that case he ought not to support this last opinion by appealing to that other.

His new objection (p. 460) to the title *Syennesis* is equally wide of the mark. It is quite true that *Syennesis*, son of *Oromedon*, was commander of the Cilician contingent in the second Persian War. But why this Syennesis may not have been king of the Cilicians one fails to see. In the opinion of my colleague, Professor Niese, the historian, the internal probability is all in favour of the admiral of the Cilicians having been also their king and satrap, to which it must further be taken into consideration that we know with absolute certainty of three kings of Cilicia who bore precisely this (throne) name *Syennesis*. And if the father of Syennesis of the second Persian War is called *Oromedon* and not *Syennesis*, this is no proof that *Syennesis* was a personal name and not a title. For—as one can assume without difficulty, and as has been assumed by others before me—the royal title may have been borne only by the living reigning king. To what an extent the Cilician royal title—only as such is it established—x-y-z-x, read by me as *Syennes-i-s*, forced the individual name into the background may be perceived by any one, even without any knowledge of the meaning of the inscriptions. In the inscription of Bor it is the first word, and according even to Hommel, the real name only comes in later! Other circumstances tending to prove the same I cannot notice here, because I should have to presuppose such an intimate acquaintance with the inscriptions as is possessed by no one of the certainly small number who have occupied themselves with these at all.

As to *Ḳode* (p. 460), Hommel might be right in holding that it is a native word for the district it designates and not an Egyptian word, if he could really *prove* its occurrence in non-Egyptian inscriptions. Several years ago I myself, like Hommel, thought of a connexion between *Ḳode* and the *mātāti kutiti* of one of the Tell el-Amarna letters (sent by one of his subjects to the *Egyptian* king!). But this connexion is not demonstrable, because, in spite of Hommel's confident assertion, we do not know what is meant by *Kutiti* nor even precisely by *Ḳode*. And if Hommel, on account of the position of the word *kutiti* between *Khāti* and *Mitanni*, draws an inference as to the situation of the territory designated by it, he will see, on looking at the passage again, that in his haste he has made a slight mistake. Moreover, I repeat that we do not know whether *Ḳode* really includes Cilicia, and in any case there is absolutely *no* ground for extending it beyond Syria. Should Hommel, however, object that Cilician slaves make *Ḳode* beer for the Egyptian king, I would remind him that, according to my deciphering, it is Cilicians, Hatio-Cilicians, from whom the Hittite inscriptions, *e.g.* of Hamāt in Syria, emanate. And even if—what we are not in a position to affirm absolutely—*Ḳode* did embrace Cilicia, then the king in the inscription of Bor would be, according to Hommel, king over Cilicia—as I maintain.

Hommel thinks (p. 461) my interpretation of the new seal or amulet published by Hayes Ward 'quite improbable and out of all analogy, nay altogether impossible and inconceivable.' I should like to know, Why? Still the interpretation may now be suffered in one point to drop. I have no intention of embarking upon long discussions, but simply state here, for behoof of those who take a special interest in these things, that the sign below the serpent, a pointed filled-up triangle, *may* at least equally well be the royal cone as the sign for 'servant,' which, where it *certainly* occurs, is not, according to my latest results, filled up. Further, the signification of the semicircle has been anew subjected by me to a very searching examination, with the result that it is quite certainly a synonym for 'king,' and used only to designate kings. Therefore I read provisionally: 'Of Cilicia (and) Arzauia (?) the . . . brave (?) prince, "Serpent," the king.' That is to say, I see provisionally in the serpent a personal name, as in the serpent upon the Seal 12 in Plate xvi f. in Wright's *Empire of the Hittites*—standing perhaps for the same person. Names of animals as personal names have been recognized by me also in Hamāt, Karkemish, Mar'ash, Bulgarmaden, Bor, and in the 'Bowl' inscription. As the modern Armenians are descendants of the Hittites, it is not an unimportant circumstance that amongst these very Armenians the names of animals recur with frequency as personal names.

The only additional remark I have to make on this point is that these new possibilities are of importance for the explanation of the Bilingual, and may at the same time contribute to modify still further Sayce's explanation, which has already undergone such radical modification. I note, with satisfaction, Hommel's (p. 461) acceptance now of my interpretation of the sign for 'lord,' as well as his assertion about the hand hieroglyphs which play so important a rôle in the inscriptions. Independently of me, of course (p. 462), although I maintained a similar position as long ago as 1894, he has meanwhile worked his way to the conclusion that the outstretched hand (with variants) is a hieroglyph for 'god.' As one sees, *La vérité est en marche*. I congratulate Professor Hommel on this further recognition of the truth as I have recognized it, and trust that, like myself, he too will soon have outgrown the childlike belief that it was simply from the love of variety that in certain quite definite instances the hand was portrayed in one position and in others in another. But I will refrain from passing judgment upon Hommel's variants. Possibly he is partially on the right track in what he says, for, in point of fact, a number of hand hieroglyphs which in my *Hittiter u. Armenier* I had still given under different numbers, coincide as variants of several primitive forms, No. 3 at Boghazkoi of the god hieroglyphs having probably to be identified with No. 6, No. 5 at Ordasu with No. 7, Nos. 8, 9, 10 all with No. 15, and Nos. 12 and 13 both with the sign for 'great.' Certainly it is now permissible to ask what, then, Hommel makes now of the sign to which he, in conjunction with Sayce and in opposition to me, attributes the meaning of 'god'? Are there two ideograms for 'god'?

But all these things are trifles, on whose account it would not have been necessary for me to pay any attention to the 'Reply' of Hommel. There are two points, however, that unreservedly demand to be set right.

Hommel (p. 461) calls my criticism of the merits of Sayce (a criticism, by the way, called forth by himself) 'scandalous,' but he has not refuted it. I said (and say still) that Sayce, by means of two *false* conclusions, deduced from the small Bilingual—which, indeed, owing to its brevity, imposed relatively narrow limits on the sphere of interpretation—the *correct* interpretation of the sign for 'king,' and by means of another *false* conclusion arrived at the reading of the sign for *me*, without, however, proving it by a single further correct inference, and I added that I could not recognize in such discoveries 'the intuitive perception of genius.' This setting forth of the naked truth is to be called, then, 'scandalous.' Why, I cannot imagine. In that case one might surely well call it 'scandalous' also when Hommel, on behalf of his friend Sayce, brings forward my own demonstration and uses it against me. For it was not Sayce but *I* that formulated *one part* of my proof for the phonetic value of the sign *me* in the way that Hommel exhibits it in his plea for Sayce against me. But I added that such considerations did *not* suffice to establish the phonetic value as certain, and to-day I can add further that it is doubtful whether the sign in the Bilingual belongs to a name at all, in other words, the Bilingual perhaps supplies *no help* for the reading of the sign. It was my discovery of the group for *Karkemish* that first gave us certainty that the sign comprises at least an *m*! It is truly strange that Hommel, misconstruing the facts, shows such zeal for his friend Sayce, the very man who, as Hommel himself implicitly concedes, has done me an injustice in his criticism of my deciphering results. Is this Hommel's idea of the 'sine ira et studio' which he misses so sorely on my part?

But there is one point in which I must concede to Hommel (p. 461) that he is right as against me. It was scarcely justifiable to ignore Sayce's interpretation of the nominative sign. For, although the explanation was bound to occur to any one who gave so much as a glance at the inscriptions, although it did not absolutely hit the mark, although it was based merely upon an assertion instead of resting upon evidence, and although it remained unfruitful, not being justified by further correct inferences, still it was at least half correct, and therefore in a criticism of the merits of Sayce it ought to have found mention. I readily admit that I, like others, am not free from the disposition to underrate the merits of my predecessors, and that this disposition has played me a trick in the present instance. Whether, however, my criticism was on that account 'scandalous' let others judge. The question whether or to what extent I stand upon Sayce's shoulders I need not enter upon here. That also I leave to the objective judgment of the future. Hommel's disquisition on this subject is superfluous in a polemic against me, for I have not raised the question, but have simply aimed at defining with accuracy and precision the services rendered by Sayce—and nothing more.

In conclusion, however, I must protest against a method of procedure, for the characterizing of which words are completely wanting in my vocabulary. In my article in the June number I said that Hommel, in order to rescue the name *Tarkondemos* for the Bilingual, attributes to the cuneiform sign, which elsewhere is read *mu*, the phonetic value *dim*, which this sign has nowhere else. In this Hommel sees (p. 460) 'a melancholy evidence of my poverty as an Assyriologist,' and then proceeds further to offer his argument in favour of this phonetic value *dim*. He writes: 'Why, the very name of the sign *mu*, namely, *mu-ḫal-timmu*, shows that *mu* has also the values *ḫal* 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 *ḫadim*, *adim*), as is shown by *mu-ḫatimmu* (written *amelu*, "man," and *MU*) = "baker." . . .'! Although all this is, properly speaking, quite irrelevant to the main question whether the sign *MU* really *has* the phonetic value *dim* or not,—which I have denied, and which even Hommel does not assert,—I must use this example to illustrate Hommel's fashion of proof. What he brings forward against me in order to prove the theoretical existence of something which is not present *in praxi*, consists in large measure of constructions and purely arbitrary assertions *ad hoc*. Because: (1) nothing can be inferred from the name *muḫaltimmu*, seeing that we do not know its nature. Hommel's explanation could be accepted only if on other grounds the readings *ḫal* and *tim* or *dim* for *MU* could be proved. 'Year' is certainly expressed by *MU*, but (2) *MU* in Assyrian does not signify *līmu*, and (3) *līmu* does not signify 'year' but 'eponymate'; (4) a (Sumerian) word *lim* is as yet unknown in the sense either of 'year' or 'eponymate,' and (5) a dialectic form *dim* for it is a pure coinage of Hommel's; (6) even if *MU* has the phonetic value of *dun* in Sumerian, yet *dun* is not *dim*, and (7) what *can* be read in *Sumerian* is not on that account *present* in *Assyrian*; (8) *MU* never signifies 'bread,' and (9) 'baker' is not *muḫatimmu* but *nuḫatimmu*. The innocent reader, unused to such methods, will imagine that I am treating him to a parcel of lies. Well, if he doubts my regard for the truth, I have to ask him to apply to unobjectionable Assyriologists, for instance to one whom Hommel himself rightly calls 'sober,' I mean Professor Zimmern. He will be able thus to assure himself that Hommel upon the basis of a multitude of airily constructed data sets up something as a fact which is purely a product of his own imagination, and because I quite rightly deny its reality, declares that my 'poverty as an Assyriologist' is demonstrated. Any one who has followed my previous explanations will perhaps understand why this reproach coming from this quarter does not move me, any more than the reproach that I betray my 'complete ignorance of the history of the Greek language' because I regard as ridiculous the affirming of a connexion between the Greek δράκων, δράκοντος, and the Hittite *Tarkhu*, etc. But I am anxious that one should learn here what means Hommel employs to put his opponent in the wrong.

I now address to Professor Hommel quite formally the request either to declare here in brief and straightforward fashion, and without any superfluous circumbendibus, that, as I asserted, the phonetic value *t(d)im* for the sign *MU* cannot be demonstrated from any Assyrian text, or else to adduce unambiguous evidence for it, and, as he cannot do this last, to confess that in an unheard of fashion he has groundlessly insulted me. I surely do not exaggerate my colleague's feeling of honour when I assume that he will accede to my proposal. But, in the interest of the readers as well as the Editor of THE EXPOSITORY TIMES, I address to him the urgent request in future to adhere to the point so that this unedifying performance of ours may come to an end. We have already sufficiently abused their patience. Therefore in future let him give us fair and objective arguments *sine ira et studio*, such as he wishes I employed.

# At the Literary Table.

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