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LORD, what have I that I may offer Thee?
Look, Lord, I pray Thee and see.—

What is it thou hast got?
Nay, child, what is it thou hast not?
Thou hast all gifts that I have given to thee:
Offer them all to Me,
The great ones and the small,
I will accept them one and all.—

I have a will, good Lord, but it is marred;
A heart both crushed and hard:
Not such as these the gift
Clean-handed, lovely saints uplift.—

Nay, child, but wilt thou judge for Me?
I crave not thine but thee.—

Ah, Lord, who lovest me!
Such as I have now give I Thee.—C. ROSSETTI.

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The Hittite Inscriptions.

IN REPLY TO PROFESSOR HOMMEL.

BY PROFESSOR P. JENSEN, PH.D., MARBURG.

IN the May number of THE EXPOSITORY TIMES Professor Hommel criticises my last book, *Hittiter und Armenier*, without—strangely enough—saying a single word about the new arguments contained in my article in the April number of this same magazine. For his criticism I tender him my sincerest thanks, for, in seeking to represent my conclusions as weak or unfounded, he shows how little assailable they are in the main.

While in several passages in the inscriptions he substitutes other place names for the ones adopted by me, he admits—without making this prominent, to be sure—that I have rightly recognized the position of these names in the inscriptions, namely, at the beginning of the latter before a cone or an equivalent group of signs, and in this way he concedes a *chief* basis for my further decipherments. But in thus implicitly admitting that the group just named, like the simple cone, stands for 'king,' and, further, that a certain sign occurring between this group and a place name marks, at least in one instance, a genitive ending, he concedes additional data which were and are of importance for the work of decipherment. Hommel admits, moreover, that in certain inscriptions, e.g. that of Bor,

a transcription and translation of which I attempted in the April number, I have rightly taken a group x-y-z-x as indicating a title. He concedes also that I am right in taking a certain sign to mean 'queen,' and in referring it to a goddess (the great goddess). But this carries with it a further admission. For, according to Hommel, I appear to be right also in reading 'king of *Karkemish*' before this sign in two passages of an inscription of Jerabis. Now, as Hommel also agrees, the expression in question ('king of *Karkemish*') stands in the first of these passages in the nominative, and the sign for the queen-goddess, as he also admits, is followed by an expression in the nominative. But, seeing that the inscription emanates from a king, not from a queen, the 'queen' of the inscription cannot under any circumstances form an apposition to 'king of *Karkemish*,' but must be dependent upon the following nominative expression. The latter in that way will indicate a relation of the king to the 'queen,' as I have maintained, and as has proved of importance for the interpretation of the inscriptions of Bulgarmaden, Bor, and Andaval, and now of the greatest importance for the

interpretation of the Lion inscription of Mar'ash. Once more, this sign for 'queen' appears at Fraktin before a goddess, along with a group which I found also before the queen of the gods at Boghazköi. Before this goddess there stands also, as I finally discovered, a fist. Since now Hommel would surely admit that what stands before the divinities at Boghazköi must be recognized to be their names and titles, he will concede that the fist which has such remarkable prominence in the inscriptions represents the queen of the gods, as I insisted anew in my reply to Sayce, without any protest having been offered by Hommel. And along with the fist as her symbol, he will also, in accordance with my argumentation (*z.c.*), be led to accept of the open hand as the symbol of the king of the gods, and therewith a whole series of other hand hieroglyphs which occur in succession at Jerabis (Inscrip I. and II.), and in the Lion inscription in an order determined by considerations of rank, must be viewed as god hieroglyphs, and the expressions which follow or precede them, as indicating the relation of the author of the inscriptions to them. But in this way Hommel implicitly recognizes that my interpretation of the inscriptions, *e.g.* that of Bor, is correct as far as the general contents are concerned. And yet by his reply he implicitly denies that my deciphering has so much as broken ground. This I call preposterous. And if Hommel was not aware of this self-contradiction, it is evident that, in spite of his declaration at the beginning of his article, he has not yet reached a comprehensive view of the inscriptions.

But, further, if Hommel, not merely on account of my decipherings, but because it is proclaimed by the rocky walls at Boghazköi, must assume with me that a series of hand hieroglyphs are god hieroglyphs, it must strike him as remarkable that there is never found in conjunction with these the sign in which he and Sayce see a variant of the sign for 'god' at Boghazköi, but which I regard as the hieroglyph for 'land.' When, besides, this alleged variant is never exactly the same as the Boghazköi sign for 'god,' sound reason must compel Hommel to separate the one category from the other. But then there remains not a shadow of ground to oppose my reading of the sign as 'land,' 'city,' 'district.' Hommel concedes, further, that this sign is followed on the 'Bowl' inscription by a group, for which the reading

Karkemışh (a city name) is probable, and at Bulgarmaden is both followed and preceded by a group in which he as well as myself sees an expression for 'king'! Still *this* proves little for one who has not yet any idea of the contents of the inscriptions. The oldest form of this sign is a circle, with two parallel strokes drawn through it and having on either side two semi-circles parallel with the circle. One cannot avoid supposing that what this is meant to represent is a city, through which and out of which on two sides a highway leads, and the surrounding district under the city's jurisdiction. That is at least no forced explanation. But Hommel brings against me the Amulet (!) inscription recently published by Hayes Ward. Well, in the first place, I would remark that Hommel is not quite just to me in his translation, according to my deciphering. For it was only in my first work and quite conjecturally and with a query that I equated the word *ar-s* in the inscriptions with the Armenian *arats* = 'shepherd,' but since then I have long abandoned the attempt at an exact interpretation, and consequently in my last book I have left this word quite out of account. Beyond this, I have no essential exception to take to Hommel's rendering, after my decipherment scheme, except that a sign for 'worshipper,' ('servant') (*paštaunēai*) occurring at the end of the inscription is ignored by him. I have to protest, however, against his notion that the translation furnishes a *reductio ad absurdum* of my position. For even if the symbol in the inscription which I formerly took as another expression for 'king' need not have this sense, it expresses a relation between a king or a god and a land, and the 'brave *ar-s* x of Cilicia and *Arz(s)auia* (?)', whose worshipper the possessor of the amulet styles himself, is perhaps the same who is called in Inscrip. III. from Jerabis the king of kings, the Hittite Hercules and god of war, the god who, according to my latest results appears under one name or another in very many amulet inscriptions. The whole inscription, according to my deciphering, runs: 'Of the *ar-s* brave x of Cilicia and *Arz(s)auia* (?) y servant (*or* worshipper).' What y (the serpent above the sign for 'worshipper') signifies I cannot say. But suppose we grant the entire possibility that, as Hayes Ward and Hommel assume, it is a divine symbol, or even that it stands actually for the king of gods, namely, the weather-and-lightning god, representing him perhaps in the latter aspect. Granted, fur-

ther, that it is quite possible that what precedes runs parallel to this, and characterizes the king of the gods and not the god of war (in any case, in the inscription of Ordasu, as I now know, the king of the gods is called the ruler of Melitene, if not perhaps of Hati). Yet these possibilities, as is evident, do not shatter in the least my interpretation of the land hieroglyph. By the way, Hommel will hardly expect us surely to follow him in tracing a connexion between the Greek *δράκων* and the Hittite divine name *Tarkhu*, in which he sees the above-named serpent god. A Hittite word for 'serpent' borrowed by the Greeks! What an idea!

To proceed. I said that Hommel has directly and indirectly accepted of a very large part of my decipherments, e.g. in the case of the inscription of Bor essentially the whole of the commencement. But the king must have named himself, and that, too, somehow at the commencement of the inscriptions. Now, if at the commencement of the inscription of Bor we introduce the interpretations of the signs and sign groups which Hommel directly and indirectly sanctions, then, after deducting relatively frequent or very frequent groups or individual signs which as expressions for personal names cannot come into consideration, there remains for the king's name precisely the group which I claim as indicating this. But now it is just this which stands at the very opening of Jerabis I. before the expression for which Hommel himself concedes to me an interpretation such as 'king of Karkemish,' and in another inscription, that of Ordasu at Melitene-Malatya, likewise before a group for which Hommel implicitly concedes to me the general sense of 'king of the land of so and so.' One must admit that the name is discovered by me at a very appropriate place. Hommel, however, who does not follow out the consequences of his concessions, finds the Ordasu king's name in a different place, namely, nearer the commencement, in a group of three signs. This group he reads *Tarkunazi*, and, as we know only one king of Melitene of this name, he must feel compelled, until decisive grounds forbid it, to assign the inscription to this king's time. Well, I have done exactly the same in assigning it to his successor *Mud(ḫ)alu*. Hommel, by his reading of the name, thus implicitly bears witness to the correctness of my chronology. But the date was to me a weighty argument in

favour of my assumption that the inscription emanated from *Mud(ḫ)alu* of Kommagene, who, Sargon tells us, had temporary possession also of Melitene! Thus against his will Hommel turns out everywhere to be a witness in favour of, instead of against me.

As to what Hommel puts in place of my readings, I may leave the judgment to my readers, contenting myself merely with the following remarks. In inscriptions found to the west of the Taurus in what was demonstrably, at least in later times Cilician territory, x-y-z-x is, as Hommel himself admits, the chief title of the kings, and one of these kings, whose inscription has been preserved in full, calls himself, as Hommel also concedes, king of a-x. And all kings of Cilicia of whom the Greeks speak as *living and reigning* are called *Syennes-is* (where -s is the Gr. ending). Hence it was long ago assumed, although not universally, that Syennesis was the title or reigning name of the kings of Cilicia. Thus, at least very possibly, the first and the last consonant in the title and the last in the name of the capital of the Cilician kings, Tarsus, had the same or a quite similar sound, and therefore I considered myself justified in adopting the reading, 'The *Syennes-is* king of *Tars-us*' for a group x-y-z-x and a following a(+ b) + x 'king' at the beginning of an inscription from what was demonstrably, at least in later times, Cilician territory. This interpretation appeared evident enough, and it has approved itself as such to a great many. But according to Hommel, it is impossible. For—thus he utters his dictum—Syennesis is no title, and—he knows it for certain—in *Syennes-is* the second s is the Cilician nominative ending, nor does he shrink from adducing for comparison the Median (!) *Zualzash*. And this is put forward without a single vestige of proof. Strange that Hommel did not recollect Hittite names like *Tarku-nazi* on the one hand, and *Nḫḫ-ḫ-s* and **ḫ-wḫḫ-ḫ-s* on the other (see my *Hittiter u. Armenier*, p. 225), which, had it been a case of producing evidence in his own favour, he would certainly have cited as decisive against the view stated above! To think that one should still have to argue against such idle fancies!

But now what would Hommel substitute for my readings? Since he discovers in Eastern Asia Minor no kingly title answering to the group x-y-z-x, he goes to Lydia (!), finds there the word *παλμυ-s* = 'king,' prepares this for his purpose by

evolving from it *vialvi* (!), and behold the reading of the Eastern Asia Minor title is discovered! Then for *Tars-* he reads the name *Kavi*, which at least approximately was borne by a small district on the Gulf of Issus, between which and the locality of our inscription Cilicia lay (!), and about whose kings we do *not* know that they ever held sway to the west of the Taurus. In doing this he identifies *ad hoc* the first sign of the group standing for the name of the country with another sign read by me *k(a,o)*, which, as Insc. III. from Jerabis might have sufficed to show him, is totally different from it. But he now offers a still further choice of reading for the kingly title, namely, *Desanda-s*, the name of a Cappadocio-Cilician god (!), and, corresponding to this, for *Tars-us* the reading *Kode* an Egyptian (!) designation (meaning 'circle') for a district in N. Syria, or perhaps N.W. of this, but which we cannot define more specifically! And in this Hommel does not observe that in proposing the Cappadocio-Cilician divine name as title of the king, he must at all events concede the possibility that the latter, as I maintain, is precisely the king of Cilicia. If one of us is right, there can surely be no discussion about which of us it is.

But the worst feature of my deciphering is, in Hommel's estimation, that I contend for the Armenianism of the language. For, to begin with—so he argues—even if I am right in my readings, the words I obtain are comparable also with words from other languages. My *m-s* = 'great' may be placed side by side with the Scythian (Iranian) *maz* with the same meaning; my *s* ('-s) = 'I' with the Vannic *iesh(e)* [in which *-sh(e)* is most probably the ending of the nominative!]; my *mi* [in case my Armenian hypothesis is correct, say *emi*] = 'I am' with the Sumerian *mi* = 'to be' (!), and on the other hand with the Vannic ending (!) *-ubi* for the 1st person sing. of the perfect (!), and, assuming the correctness of Hommel's *m-vi* instead of *m-s* for 'great,' with *-moas* in Cilician names, a termination whose meaning is absolutely unknown to us!

I have already indicated above by parenthetical additions why such comparisons, to put it mildly, are impracticable, with the possible exception of that of *m-s* with the Iranian *maz*. And this particular comparison might be adduced, according to Hommel, in favour of the Indo-Germanic character of the language, and thus, he himself being witness, in my favour—provided, that is to say, such arguments have any weight. But this

they have not. For two languages are not proved to be cognate simply because here and there coincidences between them can be pointed out, and the course of argument by which I sought to establish the Armenianism of the inscriptions consisted not in adducing such isolated coincidences, but in showing that the Hittite words obtained by me, for which I had at least approximately hit the right sense recur in Armenian at least in the majority of instances, with the same or a quite similar sense and in a form *changed in accordance with the established Armenian laws of phonetics*. Only in the case of two words was I unsuccessful, namely, the word *dzar(i)o* for 'king,' and a supposed word *emio* for 'powerful,' or the like. But this apparent obstacle has meanwhile been removed. On the one hand *emio* has to be replaced by *mio* = 'one,' and this corresponds to the Armen. *mi* = the older *mio*, on the other hand by *im(oi)o*, 'my or mine' = Armen. *im(oy)* from the older *im(oi)o*, and finally, in at least four passages where a name of the great goddess is in view, by *Ma-a*, probably = *Ma*, the presumptive name of the great goddess at Komana in Hittite-Cilician territory.—Then as to the Hittite word for 'king,' *dzar(i)o*, this is not indeed directly demonstrable as Armenian, yet indirectly in so far as the inscriptional hieroglyph, *i.e.* the Rebus for 'king,' as exhibited by the oldest inscriptions, those of Hamath, is a tree, and 'tree' in Armenian is *tsar*, probably from an older *dzaro*.

It should be evident that there is a radical difference between being able for a few of my Hittite words to adduce words or mere endings having a partial assonance, drawn from different languages and with different or even unknown meanings, and being able to bring forward a language in which the whole of the Hittite terms recovered by me, as far as their meaning is established, recur with the same or a very similar sense, and in precisely the form which in accordance with the laws of the Armenian language they must assume. Hence I cannot understand Hommel's wonder that in my latest book I have not taken account of the language of the Vannic inscriptions which he thinks [why?] would have been much nearer my purpose. It ought also to be clear that Hommel, instead of having shattered my position by his objections, has given it new strength: *Hommel could not discover any one language which answered so completely to my Hittite as the Armenian.*

But Hommel maintains, to be sure, that I compare things that are not comparable. For instance, I assume for Hittite an original ending *-s* in the nominative singular—which, however, does not appear in the *inscriptions*—and an ending *-m* in the genitive plural, and offer this as evidence of Armenianism. Hommel, on the other hand, remarks—let one listen and wonder!—that in Armenian an ending *-m* in the gen. plur. ‘is no longer discoverable,’ and that ‘Armenian wants all traces of an original nominative in *-s*.’ But no Indo-Germanic scholar doubts in the least that both these endings were once present, any more than he doubts that (contrary to the opinion of Lagarde and after him Hommel) an original *Hatio* = ‘Hittite’ must appear in Armenian as *Hay*, *i.e.* however, = ‘Armenian.’ Hommel cites the name *Tharhath*(!)*ay* occurring in the Armenian literature (from the Syrian *Tar’atē*), and thinks this (!) might have shown me that a Hittite word *Khati* must appear in Armenian as *Hatay*, not *Hay*. Here again he shows that he has not grasped the real point at issue. *Tharhathay* is really a late foreign word in Armenian! Besides, I never asserted that *Hay* goes back to *Khati*, but to *Hatio*.

It is strange that Hommel, instead of playing off all these trivial objections against me, should not have acquainted his readers that also in every other instance where I have compared the Hittite vocabulary with the Armenian, the coincidence is exact: *e.g.* my *mi* (for which *emi* may be read) = ‘I am’ answers to the Armen. *em* from an older **emi*; my *-s* = ‘I’ to Armen. *es* from an older **eso*; my *a-i-s* = ‘this’ to Armen. *ais*; my *-s-t-r* = ‘son’ or ‘child’ to Armen. *ustr* = ‘son’; etc.

What Hommel urges against my attempt to prove the Armenianism of Hittite from the phonetic values of the written symbols, is made up merely of possibilities, inaccuracies, and objections whose feebleness we have already noted above. I do not require to lay stress on this part of my proof so long as the evidence from the language of the inscriptions is unshattered, and in any case that argument cannot have the same weight as this last named. Hence, in the interest of my readers, I decline to give a detailed refutation of the pointless objections of Hommel, merely noting briefly the following points for the benefit of those who are inclined to go thoroughly into the question.—There is no evidence that Armen. *artsiv-artsui* ‘eagle’ is ‘an ancient Iranian loan-word,’ rather does the

form evince it as genuine Armenian as long as data hitherto unknown to us cannot be brought forward to oppose this conclusion.—Hübschmann, who perhaps knows Armenian a little better than Hommel, does not adopt the view that *aman* and *anōth*, ‘jar,’ ‘pitcher,’ are loan-words, and I am not aware how *anōth* must be a loan-word from the Semitic. (Surely Hommel is not thinking of the Assy. *unūti* ‘utensils,’ ‘tools’?)—According to Armenian phonetic laws, *aragil*, ‘stork,’ cannot go back to an original *gharagil* or *varagil*.—Even if, when the Hittite writing was invented, the Armenian *orth*, ‘calf,’ was already represented by *forth*—but not by *porth*,—this would not affect my view that the calf’s head (with the pronunciation *p+a* or *o*) witnesses in favour of the Armenianism of the language, seeing that I assume that this symbol stands for every labial sound (except *m*) with *a* or *o* following.—It is perfectly inexplicable to me how Hommel can see in the sign which I read *t(a)r*, ‘a tied up wine-skin.’—The position of the hand in the hand hieroglyph, which I take to be = *h(a)t*, excludes the possibility that the instrument it holds is ‘a gimlet or a style.’—I am not aware of any reason why the Hittites should not have indicated a worm or a caterpillar(!) by a semicircle.—Accordingly, the assertions Hommel opposes to my combinations are ineffectual. The same, according to what was remarked above, applies to his contention, *e.g.* that for the value *t* of the pointed shoe, instead of appealing to the Armen. *trekh*, ‘peasant’s shoe,’ one might cite the Lesgian *tapi*, etc., ‘shoe,’ and for the value *k* of the ram’s head, instead of the Armen. *khoy*, ‘ram,’ the Lesgian *kheb*, etc., ‘sheep.’

Here is another subject of wonder to me. While Hommel incorrectly asserts that I read a foal’s head as = *Mud(t)allu*, simply on the ground of the Armenian *mtruk* (perhaps from **mudal + uk*), and while he rejects this combination, he forgets to mention that in the closest connexion with this I read a certain group, namely, dog’s head + lion’s claw, indicating the name of the territory over which the above-named king rules, as = *Kom + magh*, and that I combine these phonetic values with the Armenian words *gamphr* = ‘dog’ and *magil* = ‘claw.’ And these three combinations were the most important of all. From considerations which had *nothing* to do with the Armenianism of the language, the conclusion presented itself as very natural that the inscription

of Ordasu emanated from a king *Mud(t)alu* of Kommagene, and that consequently the foal's head, the dog's head, and the lion's claw should receive the above readings. And now the circumstance revealed itself that these readings, so far demanded already, corresponded, respectively, to an Armenian word for 'foal' and to the first parts of Armenian terms for 'dog' and 'claw.' Surely Hommel could have mentioned this.

I might say much more in reply to the incorrect statements and baseless objections of Hommel, but I will touch on only three points: (1) How comes he to assert that a reading *Shilkuaššeme* is impossible for the name of the king in the Assyrian version of the bilingual inscription? The group for the name consists of the sign for *tar*, *kut*, *šil*, *khas*, + that for *ku* or two for *bi*, + one for *u*, + sign for *ash*, + that for *she*, if not that for *mu*, although this is less likely, + sign for *me*. How then is my reading argued to be impossible? How long, truly, unfounded assertions can continue effective! On account of a late Cilician king Tarkondemos, and for no other reason, the name was for long read *Tarkūdimme*, the next to the last sign or rather the two signs next to the last being improperly identified with the sign for *dim*. Against this, following the example of Amiaud, I protested, but long in vain. The Tarkondemos must not be dethroned. 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!

(2) Hommel says that whenever his bird-goddess is named at all, she comes 'almost always' directly after his 'serpent god.' From this remark, as from other circumstances, it would look as if his acquaintance with the texts is very slight indeed. There are in all only two texts to which his remark is applicable. Such is his 'almost always,' which put in this way serves as the ground of an argument! By the way, I may tell Professor Hommel that *I*, as he is doubtless aware, cannot recognize his bird-goddess; on the other hand, I now know that the eagle, the same bird in which Hommel discovers her, certainly accompanies, without however being his symbol, the Hittite king of the gods as well as the god of Tarsus, who is identical with him. This we learn from a recently discovered amulet now in the possession of Hayes Ward, and I might have learned it even from the amulet with

its legends published by Sayce in the *Archæological Journal*, 1887, following p. 348.

(3) Hommel, as remarked above, sees in the serpent upon the cylinder published by Hayes Ward a serpent god, and will have it that the figure in the inscription resembling a serpent is his symbol. This looks very evident to anyone viewing the figure given by Hommel on p. 369 of last month's EXPOSITORY TIMES, and might readily prejudice many a one who does not know the inscriptions against my interpretation 'Cilicia.' But one's judgment will be different when one learns that the figure drawn by Hommel is not identical with the serpent 'hieroglyph' in the inscriptions. *The latter* has exactly the same appearance on the left as on the right.

Finally, I owe my readers a reply to Professor Hommel's charge that I am unjust to Professor Sayce. I asserted that the latter had recovered from the Hittite inscriptions nothing but what any one could have seen who was not quite blind, and that a few important results reached by him hit the mark by a lucky chance. Hommel thinks that in this 'Professor Jensen seeks to depreciate as much as possible any correct results that have been reached by others before him,' and that Sayce's Hittite discoveries were made 'through the intuitive perception of genius.' On this it may be remarked: (a) Sayce formerly assumed rightly that a figure occurring at the beginning of many inscriptions, namely, a human head with an arm pointing to it, signified 'I,' but he afterwards replaced this by the interpretation 'says'; (b) a hieroglyph standing before numerous figures of gods, at Boghazköi in Asia Minor, is explained by Sayce rightly, no doubt,—though the proof for it is not yet forthcoming,—to be the sign for 'god'; (c) because in the 'Tarkondemos' inscription he *erroneously* (and in this I followed him till recently) saw in a cone a sign for 'king,' which he *erroneously* (and here again I followed him till recently) identified with a similar sign in the inscriptions, he *rightly* interpreted this latter sign as that for 'king'; (d) because in a Hittite sign in the same inscription he *erroneously* saw the second part of the supposed territorial name *Ermē* of the Assyrian legend, he *rightly* read this sign *me*. This is all that Sayce has discovered. I do not know why under these circumstances I was not justified in speaking as I did, and at all events I am unable to recognize in such discoveries 'the intuitive perception of genius.'

I am far indeed from regarding myself as infallible. Rather am I firmly convinced—and every day contributes to force this upon my attention—that my deciphering is in many points of detail merely ephemeral, and that in course of time much of it will crumble away. But that is not only excusable, it is a matter of course. I am but human. If one has the courage to think and to form combinations, he must frequently and in many ways fall into error. But when Hommel applies to what he cannot accept of the results I have reached by a severe process of thought the name ‘absurdities,’ he presents them in a light in which it seems to me they do not deserve to appear, even if Hommel’s own views were correct. I believe, however, that the author of *Ancient Hebrew Tradition* and other works, not to speak of the critique with which I am dealing, is hardly the man to judge of what is absurd, and has no right to speak of the conclusions of his colleagues as ‘absurdities.’ ‘He who sits in a glass house ought not to throw stones.’

I have replied to Professor Hommel more fully than I am accustomed to do on other occasions when I am wantonly assailed by him or others. Silence is in such cases generally the most effective defence. But as I am concerned that in England as well as in Germany the true state of this important question should be learned once for all, I have felt compelled to make an exception in this

instance, and have gladly availed myself of the kind permission of the editor of THE EXPOSITORY TIMES to express my views on Professor Hommel’s article.

Having reached in my deciphering a point from which I daily gain deeper insight into the inscriptions, it strikes me as almost comical that even yet I should have to fight for my life. I can only wish that Hommel may continue to follow his own method of decipherment, disdaining to the uttermost my results, and reaching such conclusions, e.g. as that a sign, which occurs some dozen and a half times in the Lion inscription, instead of being a ‘word-closer’ (*Wortbeschiesser*), or the like, stands for ‘son.’ That is the surest and the shortest way to discover that when in the main points he abandons my methods he will find himself on a dead track; and as Hommel professes to have at heart only the victory of the truth, he will thus be brought to see and to confess that the inscriptions in the main can be deciphered only in the way in which I have done it.

I may say beforehand that I do not intend to notice any rejoinder Professor Hommel may choose to make. The above must suffice by way of answer to anything he may still bring forward against me, unless, instead of mere baseless assertions, he should adduce substantial arguments, showing that he at least knows the inscriptions, which as yet he does not.

At the Literary Table.

THE BOOKS OF THE MONTH.

THE QUEST OF FAITH. BY T. B. SAUNDERS. (*Blackwood*, 8vo, pp. 191. 7s. 6d.)

NOT everyone who set out in quest of the Holy Grail found it. Mr. Saunders sets out in quest of Faith and finds it not. What he finds is that religion is a product of the human spirit. The loftier minds, the men of genius, have attained a loftier view than is granted to common humanity. And that loftier view is heaven. For a moment Mr. Saunders seemed to pause, content with ‘an additional factor,’ willing to describe it as ‘Fate, Unknown Power, God, or whatever other term.’ But even that was dismissed as needless. The human spirit alone engenders the tendency towards

higher moral and social relations, which we call religion. But, in truth, Mr. Saunders does not set out in quest of Faith. He sets out to criticise all the recent popular books that handle matters of faith, and to show them unable to establish it. He criticises cleverly, and for the most part convincingly. But his own position is the most open to criticism of them all.

RECENT ARCHÆOLOGY AND THE BIBLE. BY THE REV. THOMAS NICOL, D.D. (*Blackwood*, 8vo, pp. xii, 333. 9s. net.)

There are few things more urgently needed at present than a survey of recent Biblical Archæ-