

## RESPONSE TO ARNO EGBERTS

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'Sprachwissenschaftlich korrekte Wiedergabe solcher Eigennamen dürfen wir in fremder Sprache und Schrift niemals erwarten...'.<sup>1</sup>

*Introduction*

The quotation with which Arno Egberts heads his paper<sup>2</sup> wonderfully epitomises its content and condescending tone. Dr. Knobloch, from whose criticism of the Soviet scholar Jernstedt the quotation is taken, had no doubt that Jernstedt was wrong. Apparently it never occurred to Knobloch to question his own certainty that it was absurd to suppose that there could be a substantial number of Egyptian loan words in Greek or to investigate the intellectual atmosphere in which, for instance, Adolf Erman as the leading German Egyptologist of the late 19th and early 20th centuries had proclaimed, in the *Beiträge zur Kunde der indogermanischen Sprachen*, that he could not find a single sure Egyptian etymology in Greek.<sup>3</sup>

Knobloch's combination of condensation and damning resembles the tone of the attack made by Sir Eric Thompson on Jernstedt's contemporary at Moscow and Leningrad, Y.V. Knorosov, who incidentally was also trained as an Egyptologist. Sir Eric, then the doyen of Mayan studies, denounced Knorosov's new approach to the decipherment of Maya hieroglyphs in scathing terms. The modern Mayanist Michael Coe has summarised Thompson's denunciation as follows:

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<sup>1</sup>Eduard Meyer, *Geschichte des Altertums*, Stuttgart and Berlin: Cotta, 4 vols. 1928-39. II.1. p. 547 n. 4.

<sup>2</sup>A. Egberts, 1997, 'Consonants in collision: Neith and Athena reconsidered' (this volume).

<sup>3</sup>A. Bezenberger, 'Aus einem Brief des Herrn Dr. Adolf Erman', *Beiträge zur Kunde der Indogermanischen Sprachen*, 7 (1883): 96.

'The great Mayanist had spoken: Knorosov's methodology was not even worth a sentence, and his so called decipherment was a Marxist hoax and propaganda ploy.'<sup>4</sup>

Almost 50 years later, we know today that Eric Thompson's approach was a dead end, while Knorosov was on the right track leading to the decipherment of the Maya script.<sup>5</sup>

There are at least three parallels between the early reception of Knorosov's work and that of Jernstedt. In the first place, both took place at the height of the Cold War when, in particular, there was outrage in the West against Stalinist cultural excesses, such as the claims that Russians had invented everything beneficial to humankind, and the vicious repression of Mendelian genetics and geneticists by Lysenko and his political apparatus. Second and even more important, was the Western scholars' instinctive and unthinking rejection of fundamental challenges to conventional wisdom and the structures of disciplinary authority. Finally, there seems to have been hostility to the fact that both Jernstedt and Knorosov used common sense and analogies in their work. Where Thompson believed that the Mayas had a mystical cult of time, Knorosov assumed that the Maya script was structurally similar to other hieroglyphic systems and that one should approach the system by learning existing Maya languages, something that Thompson considered beneath him. Similarly where Hellenists and Egyptologists saw their fields of study as fundamentally divided by a chasm separating east from west, Jernstedt saw Egyptian and Greek as neighbouring languages and therefore believed that there might be borrowing between them, especially when so many Greek words were unexplained in terms of Indo-European.<sup>6</sup>

It is interesting that during the 1940s and early 1950s, a particularly grim period in Russian history, there should have been so many original linguists. Michael Astour in prison camps and internal exile was preparing work that later emerged in his wonderfully rich work *Hellenosemitica*.<sup>7</sup> In the same period, Vladimir Illich-Svitych, Aharon Dologopolsky, and Vitaly Shevoroshkin were beginning studies that have led to the creation or rather revival of the 'Nostratic theory' and other macro-linguistic hypotheses, linking language families that have been considered categorically distinct by scholars working along more conventional lines.<sup>8</sup>

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<sup>4</sup>Michael D. Coe, *Breaking the Maya Code*, London: Thames and Hudson, 1992, p. 153.

<sup>5</sup>Coe, 1992, p. 275.

<sup>6</sup>P.V. Jernstedt, *Egipetskie zaimstvovaniya v grecheskom yazyke Moskva*, Leningrad: Akademiya Nauk, 1953; P.V. Jernstedt, 'Iz oblasti drevneyshikh egyptizmov grecheskogo yazyka', *Palestinskiy sbornik* 83 (1954): 29-40.

<sup>7</sup>Astour, M., *Hellenosemitica: An ethnic and cultural study in West Semitic impact on Mycenaean Greece*, rev. ed. Leiden: Brill, 1967.

<sup>8</sup>For their work and its background see Philip E. Ross, 'Hard Words,' *Scientific*

Only Knorosov's work has been universally acknowledged today, that of all the others is still controversial. Nevertheless, there is no doubt that all of these scholars pioneered what are now seen as the most exciting areas of historical linguistics. Why should there have been such scholarly brilliance in a period of extraordinary hardship and repression? Many factors were clearly involved. All the men were strictly trained in conventional historical linguistics, on the other hand, in the 'heat' of the Cold War they were encouraged to break away from 'bourgeois' theories. Paradoxically, Stalin's pamphlet *Marxism and Problems of Linguistics* (first published in 1950), which was largely an attack on Nikolai Yakovlevich Marr's super-political linguistics, gave linguists more leeway in which to operate.<sup>9</sup> Indeed, historical linguistics seems to have provided a refuge from the horrors and terrors of Stalin's last years.

Obviously, the fact that Knorosov was right and the other scholars were at least extremely interesting, does not make either qualities true of Jernstedt. It does, however, rule out Egberts' confidence that anything written in Russia in the 1950s is *ipso facto* valueless.

It is true that Jernstedt is not often referred to today and that the call made in 1962 by the Romanian scholar Constantin Daniel to follow the lead of 'the eminent Soviet scholar' went unheeded.<sup>10</sup> Nevertheless, Dr. Egberts is mistaken when he writes that

'even the whirlwind caused by *Black Athena* could not blow it [the dust covering Jernstedt's work] away.'<sup>11</sup>

I have just seen an application by a classicist to the Canada Council asking, among other requests, for funds to translate sections of Jernstedt's work into English.

Although I was not aware of Jernstedt's work when I began my own studies which led to *Black Athena I* and *II*, Dr. Egberts is quite right to see a connection between us. For instance, I can see nothing wrong with the two instances cited by Egberts, where Jernstedt and I arrived independently at the Egyptian etymologies *mtr* 'witness, bear witness', for the Greek *mártur-* 'witness, bear witness', and *k/h3rt* or Late Egyptian *hr̄it* 'widow' and the Greek *khērā*, 'widow.' This is not merely because the Indo-European etymologies for these words are weak, but also because I see nothing extraordinary in there having been considerable borrowing

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*American*. April 1991: 138-147, and Robert Wright, 'Quest for the mother tongue', *The Atlantic*, April 1991: 39-68.

<sup>9</sup>See the English edition brought out in Peking: Foreign Languages Press 1954. For a description of its impact see Wright, p. 48.

<sup>10</sup>Daniel, C., 'Des emprunts égyptiens dans le grec ancien,' *Studia et Acta Orientalia Bucarest*, 4 (1962): 13-23, pp. 14-15.

<sup>11</sup>Egberts, *o.c.*.

between two languages, spoken by people who were in more or less close contact for over two thousand years.<sup>12</sup> In the enlarged text of my Leiden 1996 talk as published in the present volume,<sup>13</sup> I have argued that the Greek language should not be seen as the result of a linguistic 'shift' in which 'Pre-Hellenes' gave up their language for an Indo-European dialect, but as one of linguistic 'contact' in which an Indo-European speaking population borrowed heavily from nearby influential languages, notably those of Egypt and the Levant.

Dr. Egberts remains a faithful follower of the 19th century *Junggrammatiker*, and their slogan of the *Ausnahmslosigkeit der Lautgesetze* which he cites with approval. In the printed version of my talk, I have expressed my views on the limitations of the complete phonetic regularity in which the *Junggrammatiker* believed, as well as on their inability to handle semantics with the same 'scientific' rigour. Above all, I should like to emphasise that the *Junggrammatiker* were almost exclusively concerned with genetic relationships between languages and paid virtually no attention to language contact, especially that between unrelated languages. As the quotation from Eduard Meyer, with which I head this response, makes very clear, scholars have long known that the situation is still more uncertain when it comes to proper names and that, as he puts it:

'We should never expect the renderings of such proper nouns in foreign languages and scripts to be correct from the point of linguistic science.'<sup>14</sup>

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<sup>12</sup>Chantraine dismisses Frisk's derivation of *mártus* (and hence *márturos*) from a hypothetical \**már-tu-* 'testimony.' Chantraine simply sees it as cognate with the much less precise Sanskrit *smárati* 'remember.' As the Egyptologist Wilfred Assmann has acknowledged, my Egyptian etymology for *márturos* would place it in a cluster of other Greek legal terms with similar possible Egyptian etymologies: Wilfred Assmann 'Diskussionen', in: *Anfänge politischen Denkens in der Antike: Die nah-östlichen Kulturen und die Griechen*, ed. Kurt Raaflaub, München: Oldenbourg, 1993, p. 400. In fact, 'widow' is also essentially a legal term.

Chantraine states that it is impossible to link *khērā* to the general Indo-European word for 'widow' Skt. *vidhāva*, cf. the Latin *uidua* and the Old English *witwe*. He derives it simply from a general stem *khē-*, *kha-* vaguely meaning 'privation'. The Egyptian etymology is more precise in both its semantics and its phonetics.

<sup>13</sup>M. Bernal, 1997, 'Responses to Black Athena: General and linguistic issues' (this volume).

<sup>14</sup>My translation. For a recent discussion of the enormous difficulties involved see: Ulrich Obst, 'Eigennamen in und zwischen den Sprachen', *Beiträge zur Namensforschung*, 31 (1996), *Wissenschaftliches Kolloquium in Verbindung mit der Jahrestagung der Gesellschaft für Namenkunde e.v. zu Ehren des 65. Geburtstages von Herrn Prof.Dr. h.c. Ernst Eichler*, pp. 2-16.

With this caveat in mind, we should look at the crux of Dr. Egberts' argument — his conviction that my proposal concerning the derivation of the name Athena derives from the Egyptian *Ht Nt* is, as he says, 'preposterous'. Before doing this, however, we should consider the frequency of 'preposterous' derivations of proper nouns in general and place names in particular. The strange phonetic connections are only made visible because the semantic links are certain. For instance the Celtic town names rendered by Ptolemy in the second century CE as *Eborakon* and *Sorviadunum* turned into York and Salisbury a thousand years later. Even more striking is the market town established by the Knights Templar in the early 13th century and named by them after Baghdad which by Tudor times had become *Baldock*.<sup>15</sup> Closer to the region and periods with which we are concerned, the city of Byblos was called *Kbn* or *Kpn* in Egyptian and *Byblos* in Greek. Because we know the Semitic original *Gublum/a*, it is clear that not only the city but the very different city names *Kbn* and *Byblos* are related.<sup>16</sup>

The difference between *Ht Nt* and *Athena* is, if anything, rather less than these examples. What is more, as I have tried to show in my Leiden 1996 presentation, the semantic ties between the Egyptian and Greek divine/ place-name are as strong as those establishing the etymologies of the English city names. Dr. Egberts seems to agree when he writes:

'Bernal's explanation of 'Athena' need not be impeded by semantic considerations'.<sup>17</sup>

The only points I should like to add to his summary of the semantic associations, are firstly, that *Ht Nt* did not merely mean 'House or Temple of Neit' but it was also used as the religious name for the city of Saïs itself. Secondly I should like to draw attention to the strong iconographic links between the two goddesses, stretching back to the very beginning of the second millennium.<sup>18</sup>

I have set out the phonetic arguments for the equation in the enlarged version of my talk,<sup>19</sup> so that I should only like to make two or three points

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<sup>15</sup>All of these can be found in Eilart Ekwall, *The concise Oxford dictionary of English place names*, 3rd ed., Oxford: Clarendon Press, 1947.

<sup>16</sup>For an explanation of the shift from Gubla to Byblos see William Albright, 'Some oriental glosses on the Homeric problem,' *American Journal of Archaeology*, 54 (1950): 162-176, p. 165 and *Black Athena I*, pp. 56-58.

<sup>17</sup>Egberts, *o.c.*

<sup>18</sup>Henri Gauthier, *Dictionnaire des noms géographiques contenus dans les textes hiéroglyphiques*, 7 vols. Cairo: L'Institut Français d'Archéologie Orientale, 1925-1931, IV, p. 88.

<sup>19</sup>M. Bernal, 'Responses to *Black Athena*: General and linguistic issues' (this volume).

here. Firstly, for reasons that I cannot follow, Dr. Egberts supposes that my argument is weakened by the fact that around 2000 BCE, when I propose that the loan was made, it is uncertain whether

- *Ht* and *Nt* were regarded as separate words forming a syntactic unit, or
- whether *Ht Nt* was already a composite name or morphological unit.

If the former was the case, this would help the argument I made in my paper concerning the lengthening of the hypothetical prothetic vowel before *Nt*. But in any event I cannot see how this uncertainty would affect the likelihood of borrowing.

Secondly, Dr. Egberts points out that the final dental of *Nt* ‘was exceptionally retained in this divine name,’ incidentally illustrating Eduard Meyer’s point that the rigidity of the *Junggrammatiker* is particularly inappropriate for proper nouns. However, I argue that the final *-t* was not dropped in Egyptian but in Greek at a point at which final stops were no longer tolerated. Dr. Egberts dismisses this claim on the basis of the Greek transcription of the Egyptian divine name *Thoth* with a final dental. Here, he fails to see that foreign words introduced into Greek after the final stops were dropped, would not be affected by a previous phonological shift.

Dr. Egberts’ final objection is based on the fact that the final *-t* in *ht* had disappeared in most contexts not merely in Coptic but in the Late Egyptian of the second half of the second millennium. This, however, does not affect my etymology because, I propose — on grounds given in my main paper in this volume — that *Ht Nt* was introduced into the Aegean at the very beginning of the second millennium when the final *-t* was almost certainly still pronounced in Egyptian. The only apparent counter-example is the personal name found in Linear B, *Aikupitiyo* derived from *Ht k3 Pth* ‘House of the *ka* of Ptah’, i.e. Memphis, hence *Aiguptios* ‘Egyptian’ which is apparently equally old. I would explain the dropping of the final *-t* here as the result of the awkward consonantal succession t-k-p.<sup>20</sup>

### *From Pr ḥn to Parthénos*

Dr. Egberts accepts the semantic relationship between *Ht Nt* and *Athena/ai* but denies the possibility of a phonetic one. In the case of my deriving *Parthénos* from *Pr ḥn*, he takes the opposite tack accepting the possibility of a phonetic parallel but denying any semantic connection. He claims that *Pr ḥn* as the name of the temple in Saïs was particularly inappropriate as

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<sup>20</sup>Interestingly, there would seem to be a parallel to *Aikupitiyo* ‘Egyptian’ in another personal name attested in the genitive in Linear B, *Kamatijojo*, which could plausibly be derived from the Egyptian *Kmt* ‘Egypt, Egyptians.’ If this derivation is correct, it would mean that in this case the final *-t* was preserved.

an etymon for *parthénos* for two reasons, firstly because there is only one, late attestation and secondly because the temple was associated with Osiris. The first argument weakens the association but given the fact that the Athenian Parthenon is a temple, and the massive and intricate connections between *th n* both as faience and as Eastern Libya, with Neith and Athena, the damage it inflicts on it is not severe.

Egberts' second objection is based on three misleading oversimplifications. Firstly, we cannot be sure that this temple was the only *Pr thn*. The idea of 'House of Brilliance' is an attractive one especially in regions of North West Egypt, close to the *Thnw* people. Secondly, even if this is the only identification with *Pr thn*, Osiris was portrayed as having an extraordinary array of differing and sometimes contradictory aspects. His association with fertility and his posthumous virility by no means negate his role as *Wnn Nfrw*, 'the beautiful young being,' the passive victim of Seth and in some identifications such as that of Bata in *The Story of the Two Brothers*, Osiris was portrayed as an innocent and pure young man.<sup>21</sup> Thirdly, the Greek word *parthénos* conveys youth and beauty, usually but not necessarily, associated with girls and virginity. I repeat, the phonetic fit, the lack of Indo-European alternatives and the tight semantic connections between Neith and *thn* 'faience,' divine eyes, the *Thnw* people and olive oil and those between Athena as *parthénos* with grey-blue, terrifying eyes, Libya and olives do not make the etymology certain but merely very plausible, especially since they are mutually reinforcing. The etymologies of both Athena from *Ht Nt* and *parthénos* from *Pr thn* should be seen in the light of the close cultural contacts between speakers of Ancient Egyptian and Greek for more than two millennia. In this context we should pay attention to Walter Burkert's wise admonition:

'In any case, the kind of minimalism that rejects all connections with Semitic [I would add Egyptian — Martin Bernal] that are not crystal clear, remains, on the whole, the most unlikely of possible hypotheses.'<sup>22</sup>

Finally Dr. Egberts urges me to give up what he sees as 'this nonsense' and instead, follow my grandfather and mother in writing my memoirs. I will do this happily, if and when I reach the age of 80. Until then, I think I have better things to do.

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<sup>21</sup>See Susan Tower Hollis, *The Ancient Egyptian 'Tale of Two Brothers'*, Norman: University of Oklahoma, 1990.

<sup>22</sup>W. Burkert, *The orientalizing revolution: Near Eastern Influence on Greek culture in the early archaic age*, Cambridge, Mass. and London, Harvard University Press, 1992, p. 40.