

BLACK ATHENA FADES AWAY. A CONSIDERATION OF  
MARTIN BERNAL'S LINGUISTIC ARGUMENTS<sup>1</sup>

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SUMMARY

*Black Athena, vol. III The Linguistic Evidence* came out in 2006 to very little fanfare. Unsurprisingly so; the book which professes to give readers the demonstration of the contentious Afrocentric etymologies with which Bernal had scandalized most Classicists and Indo-Europeanists, among countless other derivations, intermingles Chinese comparanda with the marshaling of Egyptian and West Semitic data in such a tightly opaque manner that the wood cannot easily be seen for the trees. The present piece aims to disentangle the various strata of Bernal's argumentation, analysing individual etymologies and the macrolinguistic speculations seeking to diminish the autonomy of Proto-Indo-European.

KEYWORDS

Afrocentrism, Greco-Egyptian contacts, lexicography

RESUMEN

*Black Athena, vol. III The Linguistic Evidence* fue publicado en 2006 con muy poco ruido. Lo cual no es sorprendente: el libro que pretende ofrecer a los lectores pruebas que sustenten las polémicas etimologías afrocentricas con las cuales Bernal había escandalizado a la mayor parte de filólogos clásicos y especialistas en indoeuropeo, entre otras innúmeras derivaciones, mezcla comparaciones a partir del chino con datos procedentes del egipcio y del semítico occidental de una forma tan irremisiblemente opaca que los árboles no permiten ver el bosque. El presente artículo se propone desenredar la maraña que forman los varios estratos en la argumentación de Bernal, analizando etimologías individuales y aquellas especulaciones a nivel macrolingüístico cuyo propósito es disminuir la autonomía del proto-indoeuropeo.

PALABRAS CLAVE

Afrocentrismo, contactos greco-egipcios, lexicografía

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<sup>1</sup> Since the present disquisition bridges three disciplines, namely Classics, Indo-European linguistics, and Near Eastern and Oriental studies (Egyptology, Assyriology, Semitics), none of the practitioners of which can be expected to be familiar with the standard abbreviations used outside of their own field, the author has asked, and was courteously given, permission to depart from the bibliographical norms of this journal. With a view to the immediate identification

The writing and assessment of comparative works of scholarship is a complex task that requires many skills. Key among them is the aptitude to see the data for what they are; for if you cannot tell a howler from a secure fact at the brick and mortar level, or discriminate between discredited fancies and ideas that have withstood the tests of time and new information, then you have no business to make pronouncements on the relationship between two civilizations. It is all too easy to make snap judgements about factual errors in books concerned with cross-cultural comparisons. But to criticize their worth it is desirable to have an extensive knowledge of *ratio et res ipsa* plus the state of opinion before the author came on the scene<sup>2</sup>. This is a very tall order with Martin Bernal's *Black Athena*, for it claims to combine linguistic and philological erudition in Greek, the Semitic and African languages, and Indo-European, with a broad marshalling of cultural change, archaeology, mythography, and chronography. Of necessity, I will focus on the former aspects, for the work stands and falls on its capacity to provide evidence for the alleged Afroasiatic impact on the Greek language.

It behooves every writer who transgresses a *consensus omnium* to provide the relevant apparatus of proof and meet conceivable strictures with erudition and logic. It is not enough for the author of a non-fiction book that 'works' (p. 6) to apply to it a veneer of scholarship by compiling a Harvard-type bibliography that supplements one or two hundred pages of primary and secondary references. His discussion must reflect the scholarly era in which we live, since nothing ages more quickly than linguistic knowledge, notably in the case of Egyptian and the Semitic languages. The arguments deployed by a radical revisionist must exhibit rational restraint too: a writer who allows himself a highly permissive set of rules yet juggles *termini technici* or broad concepts borrowed from social sciences is in no position automatically to claim the higher ground. Finally, anyone who seeks to rewrite history should not need to destroy a whole discipline in order to feel vindicated; all

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of the secondary literature cited, acronyms for the title of periodicals have been banned; every book comes equipped with its place(s) and publisher(s), abbreviated if cumbersome; the full particulars of every article, including the pagination, are furnished (except for reviews); and as few shorthands of any kind as possible have been used.

<sup>2</sup> My review of *Black Athena, Volume III The Linguistic Evidence* (New Brunswick, N.J., Rutgers University Press, 2006), in *Classical Review* 63, 2013, 142-4, thus pilloried the global unsoundness of his information; a thorough rebuttal appears in my *Aristarchus anti-barbarus. Pseudologies mésopotamiennes, bibliques, classiques* (Amsterdam, Hakkert, 2012), VIII-XV, XLVI-XLVIII, 1-35, 182-7, 194-205, as a counterfoil to the complacency of *Black Athena Comes of Age* (note 5), 11-64, 65-98. The present paper, which tries hard neither to duplicate what I said in those occasions nor to rely too much on my book, presents in an integrated narrative the final state of my reflections. I take for granted W. Slack, *White Athena. The Afrocentrist Theft of Greek Civilization* (New York, etc, iUniverse, 2006), and E. D. Francis, "The Impact of Non-Indo-European Languages on Greek and Mycenaean", in E. C. Polomé - W. Winter (edd.), *Reconstructing Languages and Cultures* (Berlin-New York, Mouton de Gruyter, 1992), 469-506.

the more so when he blunders far oftener than his peers. If *Black Athena* is ever to leave a mark outside of Afrocentric circles in the twenty-first century, it has thus to demonstrate scholarly capacity on all three levels: control of the evidence, theoretical soundness, accuracy. Otherwise, the series will be compelled to join in the grave with J. M. Allegro's *The Sacred Mushroom and the Cross* and G. Semerano's *Le origini della cultura europea*<sup>3</sup>. The debate stirred by Bernal is in any case moribund<sup>4</sup>; it was not revived by W. van Binsbergen's reprint of the ill-fated *Black Athena: Ten Years Later* (1996) with fresh matter by himself and Bernal<sup>5</sup>.

Beyond the languages he deals with, the comparative scholar must be equipped with tireless industry and nearly boundless curiosity, as he will inevitably be called upon to qualify his conclusions and weigh their worth. He also needs a sure instinct to know when it is best to hammer away a point and when to tread lightly. Bernal begs to differ. To present the reader with 'competitive plausibilities', he relies on lexical verisimilitude<sup>6</sup>, viz. on consonantal resemblances reinforced by semantic matches between a Greek word and its putative source(s) in Egyptian or West Semitic. The considerable ingenuity these comparisons exhibit is not matched by their theoretical foundations in macrolinguistics<sup>7</sup>. First of all, these foundations

<sup>3</sup> T. Jacobsen, "Mr. Allegro Among the Mushrooms", *Union Seminary Quarterly Review* 26, 1971, 235-46 at 237 sqq., showed how much nonsense informs his Sumerian doctrine (or lack thereof); as regards Semerano, his huge volumes, seemingly reviewed only in Italian journals and by non-linguists, were left virtually untouched by classicists, Indo-Europeanists, and Semitic scholars abroad (*Aristarchus antibarbarus*, 203-4 note 17). Bernal remained unaware of both writers, cf. also *infra*, note 181. The one he most resembles, however, is Simon Davis (see H. A. Hoffner, *Journal of Near Eastern Studies* 31, 1972, 35-7, especially 36-7).

<sup>4</sup> *BA III* provoked no scholarly ripple at all: very few classicists mention it and nobody wrote a review — a fact Bernal's supporters are loath to admit in print. Apart from Bernal, the only exception known to me is the Latinist turned paragon of Africana studies P. D. Rankine in his "Black Apollo? Martin Bernal's *Black Athena* (...), volume iii, and Why Race Still Matters": D. Orrells, G. K. Bhambra, and T. Roynon (edd.), *African Athena. New Agendas* (Oxford, Oxford University Press, 2011), 40-54 at 42 note 8.

<sup>5</sup> *Black Athena Comes of Age. Towards a Constructive Re-assessment* (Berlin, Lit, 2011). No similar generosity was extended to those participants in the original volume who were critical of Bernal (J. Blok, A. Egberts), which is revelatory of conspicuous bias.

<sup>6</sup> Compare Semerano, who castigates «l'insolente verbosità formalizzatrice che nella così detta nuova linguistica spesso aduggia il senso comune» (*Origini...*, *Il Dizionario etimologici* [Florence, Olschki, 1994], 1 *Dizionario della lingua greca*, XVIII), with M. R. Lefkowitz, "Black Athena: The Sequel (Part 1)", *International Journal of the Classical Tradition*, Spring 2003, 598-603 at 602.

<sup>7</sup> The net gains of which are poor, as he readily admits: «I have looked for possible Afroasiatic influences on Greek at four levels: phonology, morphology, syntax, and lexicon. The first two attempts were largely unsuccessful» (p. 584). His ignorance of comparative grammar includes Schwyzler; H. Rix, *Historische Grammatik des Griechischen* (1976); M. Mayrhofer's *Lautlehre (Segmentale Phonologie des Indogermanischen)*, in *Indogermanische Grammatik* (Heidelberg, Winter, 1986), I, 75-216. He is also unaware that, *contra* his view of the Egyptian impact on Indo-European and Greek, there is evidence in the

appear subsidiary to the core of the series. From the start, *Black Athena* was meant to address the cultural relationships of Greece and Egypt in the third and (mostly) second millennium B.C. *BA I-II* have accordingly subsumed under Afroasiatic that part of the Greek lexicon which cannot be explained by standard Indo-European or Proto-Indo-European processes. The series is not directly concerned with the influences of the Afroasiatic phylum on Proto-Indo-European phonology and morphosyntax which Bernal now prides himself on having discovered, since these chronologically predate both his chosen period and the emergence of Greece as a regional power by several thousand years. Such influences are therefore useless to demonstrate that Greece or the Greek language was indebted to Africa. The appeal to Nostratic, which launches Bernal's construct (chapter 2, pp. 39-58), entails too much ad hoc pleading to be convincing. Endorsing the methods of J. Greenberg, viz. mass or multilateral comparison<sup>8</sup>, Bernal proceeds from the most remote, then swiftly moves downward until PIE, and Greek, are lumped together under a merely hypothetical line of descent. There are good reasons mainstream linguists avoid going back so far in time (Pre-Proto-Indo-European is a 'pre-morphosyntactic era' with 'half-cognitive, half-grammatical categories': N. E. Collinge, "Before PIE: Motives for Thematism", *Folia Linguistica Historica* 16, 1995, 3-27 at 3-4), and «the Nostratic hypothesis is highly controversial and has very few supporters among specialists in Afroasiatic languages» (Z. Frajzyngier and E. Shay, in their edited volume *The Afroasiatic Languages* [Cambridge, Cambridge University Press, 2012], 4). Another major issue is Bernal's lack of linguistic control over the evidence, for *BA III*, pp. 24-164, rests on a trinity of handbooks, two of which belong to the Russian school of linguistics, with a little Diakonoff and early Dolgopolsky on the side<sup>9</sup>. As a consequence, he

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domain of basic vocabulary for a IE stratum in *Egyptian* (F. Kammerzell, *Panther, Löwe und Sprachentwicklung in Neolithikum* [Göttingen, s.ed., 1994], 37-58).

<sup>8</sup> It remains premature to tell whether or not, and in what measure, he was right (A. M. Ramer, "Tonkawa and Zuni: Two Test Cases for the Greenberg Classification", *International Journal of American Linguistics* 62, 1996, 264-88 at 285-6; etc), but notice the skepticism of the best scholar of writing as to his methods and achievement (P. T. Daniels, *Language* 80, 2004, 889-90).

<sup>9</sup> He knows Nostratic through A. R. Bomhard - J. C. Kerns, *The Nostratic Macrofamily. A Study in Distant Family Relationship* (Berlin-New York, De Gruyter, 1994) (to sample the degree of discrepancy between experts, compare A. Dolgopolsky's lexicon [pdf, 2008] with Bomhard's *Reconstructing Proto-Nostratic. Comparative Phonology, Morphology, and Vocabulary* [Leiden, Brill, 2008], vol. II); Afroasiatic through V. E. Orel - O. V. Stolbova, *Hamito-Semitic Etymological Dictionary. Materials for a Reconstruction* (Leiden-New York-Köln, Brill, 1995), with a little input from C. E. Ehret, *Reconstructing Proto-Afroasiatic (Proto-Afrasian). Vowels, Tone, Consonants, and Vocabulary, Linguistics* (Berkeley-Los Angeles, University of Los Angeles Press, 1995) (one will understand best the shortcomings of Orel-Stolbova who has read their introduction, pp. IX-XXVII, especially XIII-XV, in which they state that «many of our reconstructions are based on scarce lexical ma-

gulped down the last dregs of imaginative linguists, misprints included. Thus the statement on p. 92 «likely the Kartvelian (Georgian) family, a “sister” of Euroasiatic and Hurrian as well as the apparent isolate Hattic, greatly influenced Hittite and even provides it with its name. (The Hittites called themselves Nes and their language Nesili)» will not delude anyone with a modicum of Caucasian linguistics<sup>10</sup>. The core of the matter is the (Proto-) Anatolian connections of the Indo-European loans in Kartvelian; if they are minimal (as seems to be the case<sup>11</sup>), then Kartvelian cannot belong to Proto-Indo-Hittite. On the Afroasiatic level, we lack reconstructions of the various subfamilies; so what *BA III* does is merely pick out convenient pieces and equate them to similar Greek or (P)IE data with no understanding of the system. Bernal’s wish to marginalize the Indo-European family of languages

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terial (...) This may diminish or deteriorate the credibility of certain reconstructions for lack of additional data. However, we prefer to adduce this kind of material as well, hoping that in the future it will be partly supported by new discoveries and partly discarded. At the present stage, it is obviously preferable to create an extensive data base open to a profound critical study» [p. XV]; and Indo-European through T. V. Gamkrelidze - V. V. Ivanov, *Indo-European and the Indo-Europeans. A Reconstruction and Historical Analysis of a Proto-Language and a Proto-Culture* (Berlin-New York, De Gruyter, 1995), 2 vol. (Russian ed., 1984; a bad book: *Aristarchus antibarbarus*, 200-1 note 14), without so much as a glance at J. P. Mallory - D. Q. Adams (edd.), *Encyclopedia of Indo-European Culture* (London-Chicago, Fitzroy Dearborn, 1997). Bernal’s justification for relying on this trinity would seem to be that ‘their reconstructions are similar but not identical’ (p. XVIII).

<sup>10</sup> See G. A. Klimov, *Einführung in die kaukasische Sprachwissenschaft* (Hamburg, Buske, 1994), 297: «allein schon die Tatsache, daß die kaukasischen Sprachen historisch an der Peripherie des ältesten Kulturraums in Vorderasien, v.a. der hethitischen und urartäischen Zivilisation angesiedelt sind, fordert zu einer Suche nach Indizien für ehemalige Kontakte mit alten indogermanischen, semitischen und den sog. ‘asianischen’ Sprachen heraus, worunter man die Vielfalt sonstiger Sprachen im vorderasiatischen Bereich faßt, deren genetische Einordnung unklar ist. Auch wenn die entsprechenden Untersuchungen hier noch nicht über ein Anfangs-stadium hinausgekommen sind, so lassen sich doch bereits heute einige Dutzend altvorderasiatischer Glossen postulieren, die das kartvelische und teilweise auch das naxischdagestanische Sprachgebiet erfassen, das westkaukasische Gebiet hingegen offenbar ausklammern». Cf. also V. A. Chirikba, “The Problem of the Caucasian Sprachbund”, in P. Muysken (ed.), *From Linguistic Areas to Areal Linguistics* (Amsterdam-Philadelphia, Benjamins, 2008), 25-93 at 79-83, most particularly 80-1, and T. Smitherman, “On Ancient Kartvelian-Indo-European Lexical Contacts and their Consequences for Proto-Indo-European”, in coll., *The Sound of Indo-European. Phonetics, Phonemics, and Morphophonemics* (Copenhagen, Museum Tusulanum, 2012), 501-22 at 506, 518.

<sup>11</sup> Klimov, *Etymological Dictionary of the Kartvelian Languages* (Berlin-New York, Mouton de Gruyter, 1998), cites one case (\**wel-*, p. 51), while H. Fähnrich, *Kartwelisches etymologisches Wörterbuch* (Leiden-Boston, Brill, 2007), downplays the number of «[die] Wurzelmorpheme, das deutlich die Frage nach den Beziehungen zwischen den Kartwelsprachen und den indoeuropäischen Sprachen aufwirft» (pp. 46-7 ad \**ban*), cf. the nod of G. Hewitt, *Bulletin of the School of Oriental and African Studies, University of London* 71, 2008, 373-5 at 374. The list of IE borrowings in Georgian drawn by G. Djahukian, “Notes on Some Lexical Correspondences between Armenian and the Kartvelian Languages”, *Iran & the Caucasus* 7, 2003, 191-4, is similarly unimpressive (p. 192).

is conspicuous throughout; it is rooted in the disregard for American, English, French, and German-speaking scholarship posterior to Porkorny's dictionary to which we owe the suppression in *BA III* of masterpieces of such enduring relevance as R. Schmitt, *Dichtung und Dichtersprache in indogermanischer Zeit* (1967), and C. Watkins, *How To Kill A Dragon. Aspects of Indo-European Poetics* (1995), or its shabby treatment of Benveniste's synthesis of IE institutions (*infra*, note 64). Another proof that Bernal does not have a great understanding of the discipline comes from his subtraction of (Proto-)Anatolian from Proto-Indo-European, an old, respectable view, albeit a minority one; however, the latest research on the innovations shared by the Indo-European languages versus Anatolian tends to suggest that Hittite belongs to Proto-Indo-European<sup>12</sup>. Too bad Bernal makes a parody of a debate in which material culture and linguistic paleontology must go hand in hand (T. P. J. van den Hout, *Journal of Near Eastern Studies* 63, 2004, 229). Defences of PIH, particularly the wide-ranging P. W. Brosman, "Evidence in Support of Proto-Indo-Hittite", *Folia Linguistica Historica* 23, 2002, 1-21, cf. his "The *nt*-Participles and the Verbal Adjectives in *\*-to-*", *Indogermanische Forschungen* 115, 2010, 22-34 at 23<sup>13</sup>, did not make it onto his radar screen, and I fail to grasp how

<sup>12</sup> A. Kloekhorst, *Etymological Dictionary of the Hittite Inherited Lexicon* (Leiden-Boston, Brill, 2008), 7-11, cf. 237-9 (s.v. *\*ekku-*), 300-1 (s.v. *ḫarra-<sup>i</sup>*), 578, 581-2 (s.v. *mimma-<sup>i</sup>*), 691, and 903-4. «Each and every example (...) shows that the non-Anatolian IE languages have commonly undergone an innovation where Anatolian has preserved the original situation. This can only lead to one conclusion, namely that the non-Anatolian IE languages still formed one language community (at least close enough for innovations to reach all speakers) at the moment that the Anatolian branch split off. In other words, each of these examples is conclusive evidence that the Anatolian branch was the first one to split off from the mother language. (...) I think that the term Proto-Indo-European is still adequate as long as we keep in mind that the Anatolian branch may have preserved an original situation that has undergone innovations or losses in the other IE languages (but likewise the Anatolian branch may have innovated or lost an original situation that is still present in the other IE languages, of course). So, the times of a solely Graeco-Indic reconstruction of PIE are definitely over: we should always take the Anatolian material into account and keep in mind the possibility that the non-Anatolian IE languages have commonly undergone an innovation where Anatolian preserves the original, PIE situation» (pp. 10-1). Some Tocharian evidence points to the same conclusion, cf. R. Kim, "To Drink' in Anatolian, Tocharian, and Proto-Indo-European", *Historische Sprachforschungen* 113, 2000, 151-70 at 164-5, and A. Nikolaev, "Indo-European *\*dem(h<sub>2</sub>)-* 'to build' and its derivatives", *ibid.* 123, 2010, 57-96 at 58-9; this confirmation from another language that diverged early from PIE is crucial.

<sup>13</sup> Alone in the IE languages, Hittite has an indifference to voice, the *nt*-participle being either passive (with transitive verbs) or active (with intransitive ones). This «is attested also in Luwian (...) and in Hieroglyphic Luwian. Since in each case it possessed a passive meaning when belonging to transitive verbs (...), it seems clear that the Hittite usage was inherited from Proto-Anatolian. That the contrasting Indo-European usage was also a common inheritance seems equally safe to say, for it or vestiges of it occurred in every traditional Indo-European dialect (...). Presumably one should conclude that one usage or the other prevailed in Proto-Indo-Hittite and was inherited by both Indo-European and Anatolian, where it was retained



the partisan stamp of his scholarship could be missed here. The temptation to castigate all of this loosely-woven patchwork as a dilettante concoction is high but shall be resisted.

The second half of *BA III* surveys in a series of thematic chapters (pp. 165–582) the Egyptian or West Semitic etymologies of Greek lexemes either void of obvious (Proto-)Indo-European origins or whose (P)IE pedigree found no favor in the eyes of Bernal. In gathering his information, he seems to have had no recourse to a large variety of sources; method as well as convenience separate him from lexicographers of Greek and of any of the Near Eastern languages. Though he can cite the whole output of a few linguists, Ehret, Hodge, Szemerényi, he displays no familiarity with PIE studies, Assyriology, Greek and Semitic scholarship, whether monographs<sup>14</sup> or papers, apart from run-of-the-mill lexica for the two classical languages (the semantism and attestations of Greek words derive from LSJ<sup>9</sup>, without its (*Revised*) *Supplement*, their IE roots, from Pokorny as updated by Frisk and Chantraine<sup>15</sup>; Latin words and etymologies come from Ernout-Meillet<sup>4</sup>) and spotty mentions of antiquated or inadequate Semitic dictionaries (Erman-Grapow for Egyptian; Klein, Gesenius as revised by Brown-Driver-

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unchanged on one side and fundamentally altered on the other. Such a conclusion seems clearly preferable to the only apparent alternative that the meanings of the participles of transitive verbs inherited from Proto-Indo-Hittite had somehow come to be active in some cases and passive in others and that they then evolved in different directions in Indo-European and Anatolian» (Brosman, 23; read, e.g., J. Kurylowicz, *The Inflectional Categories of Indo-European* [Heidelberg, Winter, 1964], 167 § 12, on participle vs. verbal adjective). Though this is an attractive view (witness O. Szemerényi, *Introduction to Indo-European Linguistics* [Oxford-New York, Oxford University Press, 1996], 318), I find it more likely to hold that PIE lacked inherent orientation, on the strength of the Anatolian evidence and of C. Thimmabrey, “Attributives Partzip Präsens im Mittelhochdeutschen”, *Beiträge zur Geschichte der deutschen Sprache und Literatur* 112, 1990, 371–403.

<sup>14</sup> To the lists in *Aristarchus antibarbarus*, XV–XVI, 9–11, 13–4, 18–9, 20, 23–4, add two classics. F. L. Benz, *Personal Names in the Phoenician and Punic Inscriptions* (Rome, Biblical Institute, 1972), especially the catalogue, pp. 53–198, would have increased significantly Bernal’s pool of comparanda (so the Semitic *ʿrb* discussed in *BA III*, 104, after *BA II*, 629 note 11, appears in CIS 6025b1 *ʿRBY* [Benz, 384]; the name Μέγαρα [*BA III*, 509] evokes *MGR*, ‘to destroy, throw over?’ [Benz, 339–40], since its most famous bearer was slain by her husband in a Hera-sent bout of madness; etc), whereas S. A. Kaufman, *The Akkadian Influences on Aramaic* (Chicago-London, University of Chicago Press, 1974), contains a concise etymological dictionary, pp. 30–114. No wonder Bernal missed such resources despite their revealing title; he ignores the entire output of E. Lipiński, perhaps the most distinguished Semitist alive, author of the standard *Semitic Languages. Outlines of a Comparative Grammar* (Leuven, Peeters, 1997) (750 pages!).

<sup>15</sup> Swept under the rug are the Estienne-Dindorf *Thesaurus Graecae Linguae* and the ongoing *Diccionario griego-español*; the *Lexikon des frühgriechischen Epos*; and F. Aura Jorro, *Diccionario micénico* (Madrid, Instituto de Filología, 1985–1993), 2 vol. (*DMic.*). The consequences of the exclusive use of Frisk and Chantraine are baleful: PIE reconstructions appear only very sporadically; laryngeals play a part when Bernal needs them, not wherever they should appear; and the common IE cognates look back to the sixties at best.

Briggs, for Hebrew; Muss-Arnolt for Akkadian; Lewy for Canaanite) and grammars (Harris [1939] for Phoenician, Gardiner<sup>3</sup> [1957] for Egyptian). The only standard tools used in *BA III* are, in increasing order of frequency: Černý's Coptic dictionary (sure etymologies only); the Leskos' lexicon, whose shortened version (*A Dictionary of Late Egyptian*, Fall River, MA, s. ed., 2002-2004, 2 vol.) appears only once, p. 652 note 54; Hoch's *Semitic Words in Egyptian Texts of the New Kingdom and Third Intermediate Period* (rich and up-to-date, but often perverse; yet Bernal relies on it blindly); Vycichl's *Dictionnaire étymologique de la langue copte*, which he loves to chide; and the first two volumes of Takács' very tentative *Etymological Dictionary of Egyptian*, his favorite. So *BA III* leaves unchecked all of its quotations from Akkadian<sup>16</sup>, Arabic<sup>17</sup>, Aramaic and Hebrew<sup>18</sup>, Canaanite<sup>19</sup>,

<sup>16</sup> W. von Soden, *Akkadisches Handwörterbuch*, Wiesbaden, Harrassowitz, 3 vol., 1965-1981 (*AHw.*; Old Babylonian or Akkadian spelling); *The Assyrian Dictionary of the Oriental Institute of the University of Chicago* (Chicago-Gluckstadt, 1956-2010), 21 vol. in 26 tomes (*CAD*; Middle Assyrian spelling, used hereafter). J. A. Black - A. R. George - N. Postgate, *A Concise Dictionary of Akkadian*, 'Second (corrected) printing', Wiesbaden, Harrassowitz, 2000 (*CDA*; lemmas as in *AHw.*), is preferable by far to T. R. Kämmerer - D. Schwiderski, *Deutsche-Akkadisches Wörterbuch* (Münster, Ugarit, 1998), whereas H. Tawil, *An Akkadian Lexical Companion for Biblical Hebrew* (Jersey City, Ktav, 2009), 1-436, best serves as a supplement to the *HALOT* (note 18). It may be good, finally, to look at the lexica of recent editions: the one in J.-M. Durand, *Les documents épistolaires du palais de Mari* (Paris, Cerf, 2000), III, 537-606, collects many new senses; exemplary too is B. Kienast - K. Volk, *Die Sumerischen und Akkadischen Briefen die III. Jahrtausends aus der Zeit vor der III. Dynastie von Ur* (Stuttgart, Steiner, 1995), 213-94. In a move that speaks volumes about his grasp of the language, Bernal has 25 mentions of W. Muss-Arnolt's *Concise Dictionary of the Assyrian Language*, 1905, 2 vol., whose obsolescence is betrayed even at the most casual perusal by its arrangement according to the Hebraic alphabet; why he supplies only one citation for the *CAD* (p. 620 note 36) and I. J. Gelb's *Glossary of Old Akkadian* (1957; p. 616 note 187) I fail to guess.

<sup>17</sup> A useful tool is M. R. Zammit, *A Comparative Lexical Study of Qur'anic Arabic* (Leiden-Boston-Köln, Brill, 2002), 67-446 (it lists in abbreviated form all Semitic cognates). For vernacular, the ambitious F. Corriente, *A Dictionary of Andalusí Arabic* (Leiden-New York-Köln, Brill, 1997), proves to be a helpful supplement to the far larger Wehr, which I shall mention in due time.

<sup>18</sup> Of the Biblical lexica, J. J. Stamm's revision of L. Koehler - W. Baumgartner's *Hebräisches und Aramäisches Lexikon zum Alten Testament* (Leiden, Brill, 1967-1996), 6 vol., or its translation directed by M. E. J. Richardson, *The Hebrew and Aramaic Lexicon of the Old Testament*, *ibid.*, (1994-2000), 5 vol. (*HALOT*), are highly regarded, with the *Theological Dictionary of the Old Testament*, 15 vol. so far, on the side. D. J. A. Clines, *The Dictionary of Classical Hebrew* (Sheffield, Academic Press / Phoenix Press, 1993-2011), 8 vol., is a great tool albeit a tough one. An elegant alternative for Bernal would have been to equip each Hebrew or Aramaic word with the list of its attestations in Scripture, as did J. Yahuda in his stimulating, if wrong-headed, *Hebrew is Greek* (Oxford, Becket, 1982).

<sup>19</sup> Viz. Old Canaanite (glosses in the Amarna letters and documents); Phoenician/Punic; Ammonite, Moabite, Edomite; and epigraphic Hebrew: basic is now I. K. H. Halayqa, *A Comparative Lexicon of Ugaritic and Canaanite* (Münster, Ugarit, 2008). The vintage classic H. Lewy, *Die semitischen Fremdwörter im Griechischen* (Berlin, Gärtner, 1895), should no longer serve as the source of such words.



Kartvelian<sup>20</sup>, Phoenician<sup>21</sup>, Sumerian<sup>22</sup>, Syriac<sup>23</sup>, Ugaritic<sup>24</sup>, plus the lone African, Caucasian, or Slavic vocable which pops up sporadically third-hand and is never sourced — a fruitful occasion for howlers of all sizes (*infra*, note 33). Thus it turns out that *tolì* is not so much Baltic (p. 269) as Lithuanian (E. Fraenkel, *Litauisches Etymologisches Wörterbuch* [Heidelberg / Göttingen, Winter / Vandenhöck & Ruprecht, 1965], II, 1105), and that the Albanian for ‘dream’ (also: ‘sleep’) is *ëndërr*, *andërr* (Orel, *Albanian Etymological Dictionary* [Leiden-Boston-Köln, Brill, 1998], 92), not ‘*ädërë* and *ëndërrë*’ (*BA III*, p. 273). Even Bernal’s pet languages, Chinese and Egyptian, hardly fare better. The relevance of the former to the *Black Athena* project is controversial<sup>25</sup>, so the Chinese of *BA III* should have been

<sup>20</sup> Klimov (rich but ill-edited, posthumous publication entailing many absurd or erroneous glosses; looks backwards to Gamkrelidze-Ivanov for the historical divergence and spread of Kartvelian, pp. IX-XII); Fähnrich and S. Sardshweladze, *Etymologisches Wörterbuch der Kartwel-Sprache*, Leiden, Brill, 1995<sup>2</sup> (the rival reconstruction, far less indo-europeanist), reworked as Fähnrich-Sarjveladze, *Etymological Dictionary of the Kartvelian Languages* (‘Second Revised and Supplemented Edition’), (Tbilisi, Tbilisi Sulxhan-Saba & Orbeliani State University Press, 2000) (in Georgian), then as Fähnrich, *Kartwelisches etymologisches Wörterbuch*. — Pace Bernal, p. 603 note 48, these tools muster hardly any Akkadian loan (none for Fähnrich; 3 for Klimov: pp. 3 [*\*arwa-*], 4 [*\*as,ir-*], 251 [*\*šwid-*]).

<sup>21</sup> C.-F. Jean - J. Hoftijzer, *Dictionnaire des inscriptions sémitiques de l'Ouest* (Leiden, Brill, 1965), revised as Hoftijzer and K. Jongeling, *Dictionary of the North-West Semitic Inscriptions* (Leiden-New York, *ibid.*, 1995), 2 vol.; C. R. Krahmalkov, *Phoenician-Punic Dictionary* (Leuven, Peeters, 2000) (entirely new but bibliographically poor, replete with non-mainline glosses, and error-prone).

<sup>22</sup> The only complete tool, A. Deimel, *Šumerisches Lexikon*, II. Teil *Vollständige Ideogram-Sammlung* (Rome, Pontificium Institutum Biblicum, 1928-1933), 4 vol., has been out of date since the fifties; its competitor, the online *Pennsylvania Sumerian Dictionary* (olim Philadelphia, 1984-1998, 4 parts), progresses at a snail’s pace and still covers too few letters. The standard lists of values are R. Borger, *Assyrisch-babylonische Zeichenliste* (Kevelaer etc, Butzon & Bercker etc, 1988<sup>4</sup> [1978]), 225-300, and C. Mittermayer, *Altbabylonische Zeichenliste der sumerisch-literarischen Texte* (Fribourg / Göttingen, Academic Press / Vandenhöck & Ruprecht, 2006), 249-78, yet we miss a comprehensive work like C. Rüster and E. Neu, *Hethitischen Zeichenlexikon*, (Wiesbaden, Harrassowitz, 1989).

<sup>23</sup> R. Payne Smith, *Thesaurus Syriacus*, Oxford, Clarendon Press, 1879-1901, 2 vol.; J. Payne Smyth, *A Compendious Syriac Dictionary. Founded upon the Thesaurus Syriacus*, *ibid.* (1903); M. Sokoloff, *A Syriac Lexicon. A Translation from the Latin. Correction, Expansion, and Update of C. Brockelmann’s Lexicon Syriacum* (Winona Lake / Piscataway, N.J., Eisenbrauns / Gorgias, 2009).

<sup>24</sup> G. del Olmo Lete and J. Sanmartín, *Diccionario de la lengua ugaritica* (Sabadell, AUSA, 1996-2000), revised as *A Dictionary of the Ugaritic Language in the Alphabetic Tradition* (Leiden-Boston, Brill, 2004), 2 vol.; J. Tropper, *Kleines Wörterbuch des Ugaritischen* (Wiesbaden, Harrassowitz, 2008) (to be avoided: H. Gzella, *Journal of the American Oriental Society* 128, 2008, 351-3).

<sup>25</sup> Here is his justification : «the parallel between China, Korea, and Japan on the one hand, and Egypt, the Levant, and Greece on the other seems a useful one, but in a different way from that suggested by Jasanoff and Nussbaum. As I see it, in both cases, the ultimate recipient culture initially used a language that was unrelated or, at most, only distantly related to those of the outside sources. In both cases, we have substantial archaeological, iconographic,

impeccable. Yet his mistakes range from careless mention of the original in a translation (on p. 8, Bernal equates ‘western practices or techniques’ to *yong*, instead of *wei yong*, in the late Qing formula ‘Chinese learning for essence / substance, Western learning for function / utility / practical application’<sup>26</sup>) to blunders in pinyin that create a superficial layer of rubbish uncreditable to any Sinologist. *Pace* p. 101, the word for ‘(mountain) goat’, the beast whose arrival after winter heralds the beginning of spring (*Metropolitan Museum of Art Bulletin* 53, 1995-1996, 74-5), is *yang*, not *jie*: *Chinese Characters Dictionary with English Examples* (Shanghai, Shanghai Jiaotong Daxue Chubanshe, 2002), 1249; A. Schuessler, *ABC Etymological Dictionary of Old Chinese* (Honolulu, University of Hawai‘i Press, 2007), 557, s.v. *yáng*<sub>1</sub>. Despite pp. 103-4, ‘(honey) bee’ is *mì* (*CCDEE*, p. 722; Schuessler, 383 s.v. *mì*<sub>6</sub>). Against p. 502, ‘intestine, bowel’ = *cháng* (*CCDEE*, p. 110; Schuessler, 202, s.v. n°6), not *gāng*, perhaps a slip due to the following *gǎng*, ‘harbor, creek’.

Now for Bernal’s Semitic. He patronized skeptics for their inability to assess his Egyptian etymologies, yet his lexicography is mostly one of decoys (*CR* 63, 143-4), alongside poor morphology and syntax. For the latter, consider on p. 414 «*irr* ‘evildoer’, *irrt* ‘work’ provide a reasonable etymology for the Homeric hapax εἴρερον ‘slavery’. Chantraine denies any link with the Latin *servus* and is skeptical of all other proposals». Appearances are deceiving, for it has been concealed to the reader that he is not dealing with nouns. These comparanda are forms of the verb *iri*, ‘to make, do/act’ (J. Winand, *Temps et aspect en égyptien. Une approche sémantique* [Leiden-Boston, Brill, 2006], 128-9): *irr* means ‘he who does’<sup>27</sup>, viz. evil or good according to the context despite Faulkner, p. 27 bottom, cf. the Tale of the eloquent peasant, B 1.224, on the negative side, Sinuhe, B 263 *irr hm=k mri.t=f*, ‘may your Majesty do/act as he wishes’, on the positive one; as for *irrt*, it is passive participle of *iri* and signifies ‘that which is done’, ‘what is done’, compare *iry.t*, ‘that which was done’, ‘what has been done’. It is hard to see how the substantive εἴρερος (for *Odyssey* 8.529 — in an interpolated simile: H.

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and historical evidence of contact between the two regions. Both Japanese and Greek cultures retained and developed their distinctiveness. Nevertheless, both were saturated with elements from the older, more elaborate, continental civilizations. In both cases, the basic structure of their original languages remained, but more than 50 percent of the lexicon came from elsewhere» (D. C. Moore (ed.), *Black Athena Writes Back. Martin Bernal Responds to His Critics* [Durham-London, Duke University Press, 2001], 116). The amount is correct, yet linguists are leery of such unrefined figures, cf. M. Irwin, *Loanwords in Japanese* (Amsterdam-Philadelphia, Benjamins, 2011), 14-21, notably 17 table 1.4. I will return to this point.

<sup>26</sup> Full interpretation in W. Xianming, *Zhongguo jindai shehui wenhua shi xulun* (Tianjin, Renmin Chubanshe, 2005), 258-77.

<sup>27</sup> D. Jones, *An Index of Ancient Egyptian Titles, Epithets and Phrases of the Old Kingdom, Volume I* (Oxford, Archeopress, 2000), 338-42, canvasses with commentary no less than 26 compound titles in *irr* (n°s 1248 to 1273).

van Thiel, *Odysseen* [Basel, Schwabe, 1988], 117? — has the accusative) may have evolved out of an intrinsically neutral Egyptian verb, *nomen actionis*, or participle: εἴρεος is clearly understood, within a context of war loss, after εἴρειν, ‘to arrange in a row, bind, fasten together’ (cf. φθινύθουσι 530). The sketchiness of Bernal’s lexicography strikes the eye on every page; for example, on 185, the Latin *penates* ‘may come from *p3 ntr*, “the god”’: better here, ‘the divine figure / statue’ (N. Grimal, *Les termes de la propagande royale égyptienne de la XIX<sup>e</sup> Dynastie à la conquête d’Alexandre* [Paris, Imprimerie nationale, 1986], 287-8), as in Wenamun, 1.39 (H. Goedicke, *The Report of Wenamun* [Baltimore, Johns Hopkins University Press, 1975], 54; B. U. Schipper, *Die Erzählung des Wenamun* [Fribourg / Göttingen, Academic Press / Vandenhöck & Ruprecht, 2005], 57, 263-4). Bernal refers twenty or so times (out of thousands words) to the semantically solid yet antiquated Erman-Grapow<sup>28</sup>. Coupled with his disregard for its separate series (*Belegstellen*), this means that Egyptian roots and lexemes are cut off from their attestations, a wholly anomalous procedure in Egyptology. We need an explanation. From the odd reference to Gardiner’s listings in his *Egyptian Grammar*, I surmise that Bernal must have worked in desultory fashion with those resources available to him according to opportunity and familiarity<sup>29</sup>, instead of starting from scratch by digging his way through the indexes to Erman-Grapow before consulting more recent literature (like J. P. Allen’s glossary and list of grammatical forms in *The Inflection of the Verb in the Pyramid Texts* [Malibu, Undena, 1984], II, 541-601, 602-58, or P. Wilson, *A Ptolemaic Lexikon. A Lexicographical Study of the Texts in the Temple of Edfu* [Leuven, Peeters, 1997]; cf. D. Meeks, “Dictionnaires

<sup>28</sup> The Berlin *Wörterbuch* (1925-1931, 5 + 2 vol., reimpr. 1971; *Belegstellen*, 1935-1953, 5 vol.) includes nothing later than the early 1900s, thus misses all of the Coffin and Pyramid Texts (a corpus in constant expansion: C. Berger-el Naggar - J. Leclant - B. Mathieu - I. Pierre-Croissiau, *Les textes de la pyramide de Pépy I<sup>er</sup>* [Cairo, IFAO, 2001], I, 9-11); one will find them in R. Hannig’s vast thesaurus: *Ägyptisches Wörterbuch*, Mainz, Zabern, I (Old Kingdom), 2003, II (Middle Egyptian), 2006, 2 vol. The reliable *Concise Dictionary of Middle Egyptian* by Gardiner’s longtime assistant, R. O. Faulkner (Oxford, Griffith Institute, 1962), features very lightly in *BA III* yet silently informs many of its glosses (e.g. p. 439: “*sb* itself is ‘to glorify [a god], beautify [a tomb]’ ~ Faulkner, 210 ‘spiritualize dec’d’, ‘glorify god’, ‘beautify tomb’; etc). It was never meant to be used as an all-sufficient tool, à la Bernal.

<sup>29</sup> This is confirmed by Bernal’s memoir, *Geography of a Life (sine loco)*, XLibris, 2012), 412-3. After a chance encounter in a bookshop, he bought copies of Černý and Faulkner in which he ‘began to look for and find plausible Egyptian etymologies of Greek words’; this initial phase of his research he describes as follows: «when Leslie and Patrick [his newborn son] returned from hospital, I spent many nights half asleep, waiting for the next cry. This state, which [sic] resembles the ‘incubation’ practised in Egyptian and Greek medicine, the ‘half sleep’ when dreams or ideas come to you. In my case, these often took the form of etymologies. Many of these disappeared as I woke up. Most of those I could remember seemed absurd in the light of day. Some, however, remained plausible, increasing my confidence that I was on the right track». The doubts of Slack as to his linguistic expertise, pp. 2-3, are thus vindicated.

et lexicographie de l'égyptien ancien: méthodes et résultats", *Bibliotheca Orientalis* 56, 1999, 569-94).

Once he had enough appealing words in his files, Bernal never backtracked to check them out or look at them in actual texts, otherwise one would not face an average of two or three howlers a page. This explodes his oft-repeated claims to profound Auseinandersetzung with the sources<sup>30</sup>; it also comes a long way towards explaining the abnormally numerous *voces nihili* that mar his Near Eastern isoglosses. To my samples in CR 63, 142-3, *Aristarchus antibarbarus*, 13-6, 19-20, 200-1, add pp. 68-9 «*qrsll/n* "small bones" occurs in Akkadian and Canaanite» (Akk. *qarnu*, Ugaritic and Phoenician *qrn*, Hebrew *qeren* mean 'horn': S. Levin, *Semitic and Indo-European. The Principal Etymologies* [Amsterdam, Benjamins, 1995], 29-35); p. 158, where καί is taken out of *kyy*, 'other', a blatant misreading of Gardiner, p. 78 § 98 (*ky*, plur. *ky/úwy*); and p. 321, in which a *lim* qua 'many' is bestowed on Akkadian (*vere* LIM, *līmu*, 'thousand') whereas the Ugaritic for 'people' appears as *l'm* instead of *lim* as in *līmu*, Neo-Akkadian *li'mu*, Hebrew *l'ōm*, Eblaite *li-im* (Del Olmo Lete-Sanmartín, I, 487-8; A. F. Rainey, *Canaanite in the Amarna Tablets...* [Leiden, Brill, 1996], I, 190). Awkward thumbing explains the unreliability of his glosses of comparanda<sup>31</sup>; and even if Bernal has them right, he all too soon muddles them<sup>32</sup>. Let four instances suffice. First, Akkadian *pilaqu* (sic p. 104; actually *pilakku*) is 'spindle', not 'ax' (Francis, "Impact...", 487; M. M. Fritz, "...und weinten um Tammuz". *Die Götter Dumuzi-Ama'ušumgal'anna und Damu* [Münster, Ugarit, 2003], 319 note 1315; CAD P [2005], 371-3). Second, *pace* p. 314, where it sources σκυλάειν, σκυλεύειν, σκύλλειν, συλάειν, plus the noun σκύλα, the Semitic √SLH does not convey the notion of 'cast out, send away', with sole respect to the Arabic verb *salaha* which Bernal wrongly defines as 'flay an animal,

<sup>30</sup> «It should be clear to any reader that my books are based on modern scholarship. The ideas and information I use do not always come from the champions of conventional wisdom, but very few of the historical hypotheses put forward in BA are original. The series' originality comes from bringing together and making central information that has previously been scattered and peripheral» (*Black Athena Writes Back*, 109; that is to say, he freely retains whatever suits best his purposes); «language, or the relationships between Greek on the one hand and Egyptian and West Semitic on the other, is my strongest suit» (*Geography of a Life*, 438).

<sup>31</sup> Unlike mainline linguists, who try hard to ascertain the quality of their data, using the best lexica (e.g. F. Bader, "Liage, peausserie et poètes-chanteurs", in F. Létoublon (ed.), *La langue et les textes en grec ancien* [Amsterdam, Gieben, 1992], 105-19 at 116 sqq.).

<sup>32</sup> Akkadian *marru*, 'hoe' with an 'extended meaning "channel, canal"' in Hittite (p. 343), is false: it means 'shovel, spade' and (GŠ/URUDU)<sup>32</sup>MAR is the emblem of <sup>d</sup>AMAR.UTU (CAD M Part 1 [1977], 289c; W. Sommerfeld, *Der Aufstieg Marduks. Die Stellung Marduks in der babylonischen Religion des zweiten Jahrtausends v. Chr.* [Kevelaer etc, Butzon & Bercker etc, 1982], 55; J. Peterson, *Godlists from Old Babylonian Nippur in the University Museum, Philadelphia* [Münster, Ugarit, 2009], 32<sup>97</sup>, 99<sup>15</sup>), see further Rüter-Neu, *Hethitisches Zeichenlexikon*, n°191 p. 183, W. Schramm, *Akkadische Logogramme* (Göttingen, Universitätsverlag, 2010), 17, 99.

strip the bark from a tree' (for *salaha* H. Wehr, *Arabisches Wörterbuch für die Schriftsprache der Gegenwart*<sup>5</sup> [Wiesbaden, Harrassowitz, 1985], 586 / *salaha* Wehr and J. M. Colwan, *A Dictionary of Modern Written Arabic*<sup>3</sup> [Ithaca, Spoken Language Services Inc., 1976], 421 = 'to pull off, strip off', 'skin, flay', 'end, terminate, conclude, bring to a close', 'peel [skin, from sunburn]', 'to be stripped off, to be shed [skin, slough]', from  $\sqrt{SLH}$ , not  $\sqrt{SLH}$ : read R. C. Steiner, "On the Dating of Hebrew Sound Changes ( $*H > H$  and  $*\dot{G} > \dot{c}$ ) and Greek Translations (2 Esdras and Judith)", *Journal of Biblical Literature* 124, 2005, 229-67 at 230-1). Cf. Del Olmo Lete-Sanmartín, II, 816; Halayqa, 323; Zammit, 225-6, with Corriente, 257-8; Wehr, 585-6; and Wehr-Colwan, 420-1, for the Arabic words formed on  $\sqrt{SLH}$  'to arm' (*salaha* in the second, fifth, and six conjugations). Thus the Semitic root *SLH* is actually 'to send away, let go, stretch, extend, throw, accompany': so Benz, 416; Hoftijzer-Jongeling, II, 1136-41; Krahmalkov, 461-2 s.v. *SLH* I; F. L. Hossfeld, F. van der Velden, and U. Dahmen, 'πῶψ *sālah*', in G. J. Botterweck and H. Ringgren (edd.), *Theological Dictionary of the Old Testament*, XV (Grand Rapids-Cambridge, Eerdmans, 2006), 49-73 at 51 sqq.; Sokoloff, *A Dictionary of Jewish Palestinian Aramaic of the Byzantine Period* (Ramat Gan, Bar Ilan University Press, 1990), 551-2 ('to send'). Third, an Amharic form is mistranslated on 269: *mārra* signifies 'to lead', 'leader', not 'to distribute or allot land'<sup>33</sup>. Fourth, Linear B *me-ri-te-wo* can in no way be 'weavers' (plural !) on the sheer strength of Egyptian *mrt* (p. 408), for *me-ri(-to)* = μέλι(τος) (*DMic.*, II, 438-439). So *me-ri-te-wo* : *melitēwos* conceals \*μελιτήφων, nominative \*μελιτεύς, 'bee-keeper' (*DMic.*, II, 440; J. Fischer, "Nahrungsmittel in den Linear B-Texten", *Chiron* 33, 2003, 175-194 at 182, 182-3 note 35; A. Bartoněk, *Handbuch des mykenischen Griechisch* [Heidelberg, Winter, 2003], 280, 367). On top of all its false glosses, *BA III* blindly endorses the obsolete Semitic parallels of its secondary sources. For instance, on p. 391, *belu* is supposed to be Akkadian for 'weapon' (cf. *AHw.*, I, 120 s.v. *bēlu* II), but this reading, though it dies hard (e.g. E. Pentiuć, *West Semitic Vocabulary in the Akkadian Texts from Emar* [Winona Lake, Eisenbrauns, 2001], 37 s.v. *be-el*), is now discarded for the usual *tillu*, a weapon (see *CAD* T [2006], 411 B), following E. Frahm, *Einleitung in die Sanherib-Inschriften* (Vienna, Institut für Orientalistik, 1997), 105 at

<sup>33</sup> So W. Leslau, *English-Amharic Context Dictionary* (Wiesbaden, Harrassowitz, 1973), 703, 704; T. L. Kane, *Amharic-English Dictionary* (*ibid.*, 1990), I, 168-170. 'Allocated land' corresponds to ሞሪተ, *marrit* (Kane, 170) as distinct from ሞሪ, *mārra*, cf. ሞሪተ, *māret*, 'earth, ground, soil, terrain, land; lot or plot (of ground), tract (of land), territory; floor, bottom (of a body of water)' (Kane, 184), and የሞሪተ, *yāmāret*, 'terrestrial', as acknowledged by Bernal («the Amharic *mārra* and *marrit* "distribution of land by the government"»). 'Allocated land' for *mārra* is only referenced in G. T. Getahun, *Advanced Amharic Lexicon. A Supplement to Concise Amharic-English Dictionaries* (Münster, Lit, 2003), 25, yet after 'led, gave guidance' and 'being quartered or billeted'.



T. VI 55. Finally, Bernal prints way too much gibberish. He forgets that assyriologists mark vocalic contraction in Akkadian by a circumflex (e.g. *birtu* ‘fortress’, not *birtu* p. 391, from Muss-Arnolt’s *bīrtu* [I, 196]; *līliātu* / *līlātu*, ‘evening’, becomes *līlatu* p. 46 and is mistranslated ‘night’ next to two such cognates; the garment used as bed covering is *lītu*, *lētu*, *liṭtu*, not ‘*litu* or *letu*’ [p. 332]; *lāqah* is not Akkadian *pace* p. 333, cf. *leqû*, *laqû*, *laqā’u*, ‘to take over / away / off’), and *s*, *ṣ* or *š* sound the same for him. So on p. 183 «a Latin loan from Semitic is *summa*. (...) It would seem much more plausible to derive it from the Afroasiatic and Semitic root \**sam* “high” attested in Semitic in the Ugaritic *smm*, the Phoenician *smm* and the Hebrew *šamayim* “heavens”»<sup>34</sup>; yet, all three words have an initial *š*, as at *BA III*, 492 (cf. my note 168), whereas *smm* is ‘perfume’ in Ugaritic (Del Olmo Lete-Sanmartín, II, 763; Halayqa, 298).

Combination of these defects proves lethal, witness on p. 403 «the root *sbl* appears in the Akkadian *šūbultu* “to cause to hang down,” the Arabic *sabala* with the same meaning plus “to let fall”, the Ugaritic *sblt* and the Hebrew *šibōlet* “flowing stream, ear of grain”<sup>35</sup>. Saying shiboleth / siboleth was, of course, the test of dialect. The sense of hanging down would fit the meaning of ἀπαλιεύς (3) “fisherman using line”. The prothetic vowel would shield the double consonant». The tendency of wild etymologists to juggle substantives (or quasi-substantives such as infinitives) and ignore, or misinterpret as such, all verbal forms, is on display here. Bernal has been misled by F. Brown - S. R. Driver - C. A. Briggs, *A Hebrew and English Lexicon of the Old Testament* (Oxford, Clarendon Press, 1907), 987, s.v. שָׁבַל «√ of foll.; cf. Ar. سَبَلَ IV. *cause to hang down*, سَبَلَةٌ *flowing dress*; As(syrian) *šūbultu*, *šunbultu* (Meissner), Ar. سَبَلٌ, سَبَلَةٌ (Ba<sup>NSB207</sup>), Eth. ስብል, Aram. שְׁבַלְתָּא <Syriac> مَحْكَلَا, all *ear of grain*». The Semitic √*SBL* / *ŠBL*, ‘to carry a load, be laden’<sup>36</sup>, actually produced in Akkadian the

<sup>34</sup> *Summus* descends from \*(h)*up-m(H)o-*, ‘highest’ (M. de Vaan, *Etymological Dictionary of Latin and the Other Italic Languages* [Leiden-Boston, Brill, 2008], 600). Cf. A. L. Sihler, *New Comparative Grammar of Greek and Latin* (New York-Oxford, Oxford University Press, 1995), 207; F. Villar, “The Celtic Language of the Iberian Peninsula”, in P. Baldi and P. U. Dini (edd.), *Studies in Baltic and Indo-European Linguistics in Honor of William R. Schmalstieg* (Amsterdam, Benjamins, 2004), 243-73 at 260.

<sup>35</sup> ‘Stream’: Semitic *tblt*, G. A. Rendsburg, “More on Hebrew *šibōlet*”, *Journal of Semitic Studies* 32, 1987, 255-8. Cf. Lipiński, *Studies in Aramaic Inscriptions and Onomastics II* (Leuven, Leuven University Press, 1994), 108-9, *Outline...*, § 13.7, p. 128; R. S. Hendel, “Sibilians and *šibōlet* (Judges 12 :6)”, *Bulletin of the American Schools of Oriental Research* 301, 1996, 69-75 at 70.

<sup>36</sup> M. Held, “The Root *ZBL* / *SBL* in Akkadian, Ugaritic and Biblical Hebrew”, *Journal of the American Oriental Society* 88, 1968, 90-6 at 92-6; E. C. B. MacLaurin, “Beelzeboul”, *Nouum Testamentum* 20, 1978, 156-60 at 156 note 2, 157; P. Bordreuil, “*Mizzēbbul lō*: à propos de Psaume 49:15”, in L. Eslinger - G. Taylor (edd.), *Ascribe to the Lord. Biblical and Other Studies in Memory of Peter C. Craigie* (Sheffield, Academic Press, 1988), 93-8 at 93-6; Hoftijzer-Jongeling, II, 7745, *sbl* I (‘to carry’, ‘to support, to sustain, to provide for’); Del Olmo

substantives *šūb/pultu* ~ *šūb/piltu*, ‘consignment, shipment, gift’ (*CAD* Š part 3 [1992], 188-90; *AHw.*, III, 1258-9), and *šub/pultu* ~ *šub/piltu*, ‘ear, spike of corn / barley’ (*CAD* Š.3, 187-8; *AHw.*, III, 1258) with no trace whatsoever of a verb, let alone a causative one<sup>37</sup>; in Ugaritic, the noun *šblt*, ‘ear, spike of corn’ (C. Cohen and H. R. Cohen, *Biblical Hapax Legomena in the Light of Akkadian and Ugaritic* [Missoula, Scholars Press, 1978], 112-3; Del Olmo Lete-Sanmartín, II, 805); and in Arabic, the noun *sabal*, collective for ‘ears of corn’, and the verb *sabala*, ‘to drag’ (verbatim: ‘to let hang down, let fall, drop’), after Wehr, 550 = Wehr-Colwan, 396. For the sake of precision, I shall mention the Canaanite and Akkadian words meaning ‘ill / sick, illness’, ‘burden, burdened’ (Halayqa, 294-5), and the Jewish Aramaic *šabaltā/šubaltā*, Syriac *šebbalā, šebbaltā*, all of which signify ‘ear of corn’ (see Sokoloff, 538; Payne Smith, *Thesaurus Syriacus*, II, coll. 4033-4, 4232-3; and Sokoloff, *Syriac Lexicon*, 1504, 1580 — Hebrew / Aramaic has its own term for ‘barley’: Sokoloff, 572). *√SBL / ŠBL* is thus very far from cohering with the notion of (a fisherman’s!) hanging line. Furthermore, that spelling *תְּבִלָּה*, *šibbōlet* (18x), or *תְּבִלָּה*, *šibbōlet* (once: Judges 12 :6), was ‘a test of dialect’ (viz. another *furtum* from Brown-Driver-Briggs: p. 987 s.v. תְּבִלָּה) according to the *communis opinio* exemplified in, say, Lipiński, *Studies II*, 107-9, remains unsettled; those who find here a matter of literacy or cultural level may very well be right (e.g., R. S. Kawashima, *Biblical Narrative and the Death of the Rhapsode* [Bloomington, Indiana University Press, 2004], 36). Worse, this issue has nothing to do with Bernal’s proposed etymon; it merely evidences his fondness for obfuscatory comments. Last but not least, Chantraine dismissed too eagerly *ἀσπαλιεύς* as a derivative of *ἄσπαλος* influenced by *ἀλιεύς* (so J.-L. Perpillou, *Les substantifs grecs en -εύς* [Paris, Klincksieck, 1973], §§ 337-8, pp. 305-7), yet Bernal did not demur: ‘*DELG* strangely doubts the connection of *ἀσπαλιεύς* and *ἄσπαλος*’ Beekes (note 43), I, 153. Cf. *DELG*, 1377 (*Supplément*).

Bernal combines this abysmal grasp of Egyptian and Semitic with other blinkers. Just as he remained unaware of all post-Chantraine studies in Greek etymology, he paid little or no attention to Anatolian<sup>38</sup>, Indo-

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Lete-Sanmartín, II, 998-9, *zbl* III. *Šunbultu* in Akkadian is a mere variant by nasalization to which no weight whatsoever should be attached (and also appears in Arabic); on the interchange between *S* and *Š* in Semitic around the ear / spike of corn, help can be sought in H. Odeberg, *Das sogenannte hebräische Henochbuch (3 Henoch). Nach dem von H. O. vorgelegten Material zum erstenmal ins Deutsche übersetzt* (Königsberg-Bonn, Hanstein, 1984), 59-60.

<sup>37</sup> The most that exists is the noun *šupālu*, ‘depression, low lying area’; the verb *šapālu*, ‘to be(come) deep, low’ (*CAD* Š.1, 422-7; J. Bottéro, *Textes culinaires mésopotamiens* [Winona Lake, Eisenbrauns, 1995], 53 at 60: ‘plonger (dans le bouillon)’); and its by-form *šuppulā*, ‘lowering (of price)’ (Durand, *Les documents épistolaires du palais de Mari* [Paris, Cerf, 1998], II, 93 note a).

<sup>38</sup> J. Friedrich - A. Kammenhuber, *Hethitisches Wörterbuch* (Heidelberg, Winter,

Iranian<sup>39</sup>, and Tocharian<sup>40</sup>, unless they were quoted in Pokorny, Frisk, or Chantraine, to the extent that he chides them for mentioning these terms (so p. 24: «Pokorny derives this root from (...) such far-fetched forms as the Tocharian *ānt* “plain”, cf. 339 «Chantraine then turns to even less-likely Indo-European etymologies» or 387 «all they can find is the Old Persian *rūda*, Ossetic *rūd* “intestines!”»). It is no more invalid to deploy such forms in defence of a (P)IE etymon than to marshal the Icelandic *lái*, the German *Leid*, and the French *laid* to secure the IE *leig-* / *leik-*, ‘dürftig, elend; Krankheit, schlechtes Ergehen’ (Pokorny, I, 667)<sup>41</sup>. Too bad for Bernal the Anatolian languages, though often tantalizing<sup>42</sup>, seem to hold the key to many etymological enigmas in the Greek lexicon<sup>43</sup>! Bettering Pokorny is

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1975-2010), 2 vol. and a half (A-E; H); J. Tischler, *Hethitisches etymologisches Glossar* (Innsbruck, Institut für Sprachwissenschaft, 1977-), 3 vol. and a half (A-U); J. Puhvel, *Hittite Etymological Dictionary* (Berlin-New York, De Gruyter, 1984-), 8 vol. (A-Pa; HED); H. G. Güterbock, Hoffner, van den Hout (edd.), *The Hittite Dictionary of the Oriental Institute of the University of Chicago* (Chicago, Oriental Institute, 1980-), 3 vol. (L-P; Š); Tischler, *Hethitisches Handwörterbuch. Mit dem Wortschatz der Nachbarsprachen* (Innsbruck, I.f.S., 2001); and Kloekhorst.

<sup>39</sup> Mayrhofer, *Etymologisches Wörterbuch des Altindoarischen* (Heidelberg, Winter, 1992-1996), 3 vol.; Bernal barely draws upon his great *Kurzgefaßtes etymologisches Wörterbuch des Altindisches* (*ibid.*, 1956-1980). See also H. W. Bailey, *Dictionary of Khotan Saka* (Cambridge, Cambridge University Press, 1979) (exhausts Iranian and the IE, reference-free); Schmitt (ed.), *Compendium Linguarum Iranicarum* (Wiesbaden, Reichert, 1989); and J. Cheung, *Etymological Dictionary of the Iranian Verb* (Leiden-Boston, Brill, 2007).

<sup>40</sup> A. J. van Windekens, *Le tocharien confronté avec les autres langues indo-européennes* (Leuven, s. ed., 1976-1982), 3 vol., ought not to be used (so, e.g., Winter, *Language* 57, 1981, 935-41); standard are now Adams, *A Dictionary of Tocharian B* (Amsterdam, Rodopi, 1999, reissued 2007), despite its numerous flaws (see Winter and X. Tremblay in *Aristarchus antibarbarus*, LXXIX), and the up-to-date lexicon of the verbs offered by M. Malzahn, *The Tocharian Verbal System* (Leiden-Boston, Brill, 2010), 517-1000.

<sup>41</sup> G. Dumézil, *Loki* (‘Nouvelle édition refondue’, Paris, Flammarion, 1986), 97-8 for the Icelandic, cf. 97 note 2 for the other cognates. To the stores of Orel, *A Handbook of Germanic Etymology* (Leiden-Boston, Brill, 2003), 233-4 s.v. *\*laiþaz*, add J. De Vries, *Altnordisches etymologisches Wörterbuch* (‘Zweite verbesserte Auflage’, Leiden, Brill, 1977), 344 s.v. *laiþi-gaR*.

<sup>42</sup> Cf. the rare *nakkuš*, ‘damage’, ‘loss’, ‘fault’ (Tischler, II, 262-3; ‘Schaden’ idem, *Hethitisches Handwörterbuch*, 110). E. Laroche, “Hittite *nakkuš* — *nakkušiš*”, in Hoffner – G. M. Beckman (edd.), *Kaniššuwār. A Tribute to Hans G. Güterbock on his Seventy-Fifth Birthday* (Chicago, Oriental Institute, 1986), 137-40 at 139-40, speculates that «en restituant à l’anatolien le groupe de *nakkuš* et ses dérivés, on récupère le représentant de la racine indo-européenne *\*nek-* ‘périr, faire périr.’ A cette racine appartient le vieux terme juridique latin *noxa*, pratiquement synonyme du hittite *nakkuš*: en grec, le dérivé *nekus* ‘cadavre’ et en latin *nocuus* ‘nuisible’ ont le même thème d’adjectif en *-u-* que le hitt. *nakkuš(i) < \*nok-u-s(i)*», which is less strained (for the objections of HED, VII, 56, may be overcome: Rix (dir.), *Lexikon der indogermanischen Verben. Die Wurzeln und ihre Primärstammbildungen*<sup>2</sup> [Wiesbaden, Reichert, 2001; LIV<sup>2</sup>], 451-2) than it is incapable of being proved or disproved (Kloekhorst, 595).

<sup>43</sup> *Aristarchus antibarbarus*, XI bottom, 182 note 1, 183 *ibid.*; all the cases in van Windekens, “Graeco-Hittitica”, *Zeitschrift für vergleichende Sprachforschung* 100, 1987,

no easy task for someone loath to look at this evidence, and an impossible one when you cannot pick the wheat from the chaff. Consider αἰών, pp. 371-2: Bernal agrees with Onians against Chantraine on ‘spinal chord’ being its primary shade of meaning, but this is wrong (J. N. Bremmer, *The Early Greek Concept of the Soul* [Princeton, University Press, 1983], 15-6, H. M. Keizer, *Life-Time-Entirety. A Study of ΑΙΩΝ* [diss. Amsterdam, 1999], 15-23; T. Meissner, *S-Stem Nouns and Adjectives in Greek and Proto-Indo-European. A Diachronic Study in Word Formation* [Oxford, Oxford University Press, 2006], 150-1, has the IE cognates, cf. my note 140). Καθαρός too screams for attention: *BA III*, 254-5, piles up remote Egyptian data instead of the Sanskrit *śithirah*, *śrathayati*, < PIE \**kʷrth<sub>2</sub>ros* (possible despite Beekes, I, 615, cf. F. De Decker, “Evidence for Laryngeal Aspiration in Greek? Part I: The ‘Recent’ Evidence”, *Indogermanische Forschungen* 116, 2011, 87-109 at 91-4). No less remarkable is Bernal’s mistreatment of ἀίτας (p. 212): a tendentious summary of the *DELG* entry, which lets us ignore what escaped Chantraine’s notice, viz. that this word turns out in Alcaeus, fr. 346, 2 Voigt καὶ δ’ ἄερρε κυλίχνας μεγάλαις, ἄϊτα, ποικίλαις, where it can hardly signify much more than ἑταῖρος (bibliography in G. Liberman, *Alcée* [‘C.U.F.’, Paris, Belles Lettres, 1999], II, 240 note 290), goads the reader into accepting an Egyptian linkage with the relatively infrequent *id*, ‘male boy, youth’ (Erman-Grapow, I, 151, *Belegstellen*, I, 27; Hannig, I, 240, II.1, 451; Jones, *Index*, 346-7 n<sup>o</sup>s1287-92). However, the IE cognates (Latin *auēō*, Sanskrit *āvati*, with Mayrhofer, *Et. Wört. d. Altindoarischen*, I, 133, s.v. *avasá-*, *Oxford Latin Dictionary* [Clarendon Press, 1968-1982], 210, on their values; read further E.-M. Hamm, *Grammatik zu Sappho und Alkaios* [Berlin, Akademie, 1957], 64) strongly favor a mainstream reconstruction, from PIE \**√h<sub>2</sub>eu-eh<sub>1</sub>-*, ‘genießen’, ‘to enjoy, consume’ (*LIV*<sup>2</sup>, 274, De Vaan, 65; Beekes, I, 45, is antiquated).

It follows that virtually no argument detailed in the second part of *BA III* ever carries any conviction, whether Vycichl gets flogged for being too cautious in his search for Afroasiatic materials behind Coptic (list at *Aristarchus antibarbarus*, 10; commentary on select examples, 27-9) or the specific objections of Bernal’s *bêtes noires* Jasanoff and Nussbaum to his etymological grid are dismissed<sup>44</sup>. As expected, positive evidence against

307-12, look plausible to me. The, otherwise weak and narrow-minded (O. B. Simkin, *Classical Review* 61, 2011, 1-3; M. Beckwith, *Classical World* 105, 2012, 558-60), *Etymological Dictionary of Greek* by R. Beekes - L. van Beek (Leiden, Brill, 2010), 2 vol. (hereafter: Beekes), rightly exhibits a passionate concern for Hittite, Luwian, etc.

<sup>44</sup> J. H. Jasanoff - A. Nussbaum, “Word Games”, in Lefkowitz - G. M. Rogers (edd.), *Black Athena Revisited* (Chapel Hill-London, University of North Carolina Press, 1996), 177-205. The evidence is too recalcitrant for them to be rebuked; thus R. D. Griffith, “Criteria for Evaluating Hypothetical Egyptian Loan-Words in Greek: The Case of Αἴγυπτος”, *Illinois Classical Studies* 22, 1997, 1-6, is reduced to invoking ‘aesthetic caprice’ to claim that «the

their (P)IE defence of Greek words he deems Semitic is extremely limited in quantity; as his parallels are seldom of such a nature to command assent, all he does is dodge and muddle the issues at stake. Accordingly, Bernal never meets their challenge to him to watch beyond the surface of things<sup>45</sup>. That decent (PIE) etyma are shunned in favor of extravagant or merely specious speculations comes as no surprise<sup>46</sup>. The sheer naivety of his derivations is more interesting to pursue. Ὀβελός ~ Dorian and Arcadian ὀδελός descend

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moral of this story is that Greek accommodated loan-words to its own native forms in ways rich in *Volkspoesie*» (p. 6), cf. a few lines earlier «we will never know whether the identity of the initial syllable of Αἴγυπτος with the αἶγ- root is a coincidence or — as I think more likely — a case of popular etymology».

<sup>45</sup> ‘Word Games’, 200: «there is a certain sameness to all of Bernal’s etymologies. In each case, a Greek word is said to “come from” an Egyptian or Semitic expression to which it bears some real or fancied similarity of meaning and a vague, often extremely tenuous, phonetic resemblance. No effort is made to go beyond the realm of appearances; known and inferable facts about the history of individual forms are systematically ignored, misrepresented, or suppressed. Above all, there is a thoroughgoing contempt for phonetic consistency (...). To be sure, an excuse is offered for the confusion; the inconsistencies that we observe in the treatment of foreign sounds, Bernal tells us, are due to differences in the date at which individual words were borrowed. But he made no effort to substantiate this claim by arguing, for example, that Greek words which exhibit the “early” treatment of Eg. *ʒ* also consistently show the “early”, and never the “late” or “middle” treatment of the similarly variable sounds *h*, *i* and *l*. In fact, it is quite clear that no such regularities exist; the hypothesis of relatively early versus relatively late borrowing is simply another wild card (...). Rendsburg’s “An Etymological Response”, *Arethusa* Special Issue. *The Challenge of Black Athena*, 1989, 67-82, was obviously too sympathetic.

<sup>46</sup> He thus explains θάλασσα from *tʒš*, ‘boundary, limit’ (p. 197; rather \*τὰ ἀλ-ασσα < \*sal-a<sub>1</sub>q<sup>m</sup>l<sub>1</sub>, ‘salt water’, after van Windekens’ *Dictionnaire étymologique complémentaire* [Leuven, Peeters, 1986], 98-9? ‘typically Pre-Greek’ Beekes, I, 530); (φ)ἄναξ, from \*nh dt, ‘may he live forever’ (259-60: on \*nh J. Kahl, *Frühägyptisches Wörterbuch*, I [Wiesbaden, Harrassowitz, 2002], 82-5; Hannig, I, 272-7; II. 1, 508-34), though the Greek word clearly points to Anatolia (*Aristarchus antibarbarus*, XII; T. G. Palaima, “Wanaks and Related Power Terms in Mycenaean and Later Greek”, in S. Deger-Jalkotzy - I. S. Lemos (edd.), *Ancient Greece. From the Mycenaean Palaces to the Age of Homer* [Edinburgh, University Press, 2006], 53-71 at 53-8), which puts to rest both its stamp as a substrate (Beekes, I, 98-9) and any linkage with the Hebrew \*anāqim (S. B. Noegel, “The Aegean Ogygos of Boeotia and the Biblical Og of Bashan: Reflections of the Same Myth”, *Zeitschrift für die Alttestamentliche Wissenschaft* 110, 1998, 411-26 at 420); δοῦλος, from the Afroasiatic \*vdal-, ‘to be weak, tired’, and a Semitic (?) \*vDL(L), ‘dependent’ (pp. 326-8; better PIE \*vdes-, ‘enemy’, with Mallory-Adams, *Encyclopedia*, 169, *Introduction*, 269? the semantic shift may have been exaggerated by Chantraine after Benveniste [*infra*, note 64]; Beekes, I, 350, is grossly ignorant); κῦδος, κῦδρός, etc. from the Semitic √QDŠ, ‘holy, sacred, cultic’ (W. Kornfeld, in id. and Ringgren, ‘שרך *qds*’, *TDOT*, XII [Grand Rapids-Cambridge, Eerdmans, 2003], 521-45 at 521-6; Hoftijzer-Jongeling, II, 993-7), despite the IE connection stabilized by M. Meier-Brügger, “À propos de la partie étymologique du *Dictionnaire* de Chantraine”, in Létoublon, *La langue et les textes en grec ancien*, 267-72 at 269, read further Beekes, I, 796-7 (*BA III*, 336-7); and finally the doublet Ζεὺς ~ Διός, from *nsu*, a title of the Egyptian god Amun to whom Zeus was identified by some fifth-century Greeks (pp. 478-9; *contra*, Δι(φ)ώνη and the IE cognates of Διός Bernal so crassly misconstrued that it borders on the grotesque: *Aristarchus antibarbarus*, XIII-XIV; Beekes, I, 338, 498-9).



from *wb3* / *wḏ*<sup>c</sup> (pp. 373-4), sparing us the labiovelar, but where does the /l/ hail from? Better PIE *\*√h<sub>3</sub>eg<sup>u</sup>-*, ‘to fry’ (R. Plath, “Zur Etymologie von homerisch ὄβελός”, *Historische Sprachforschungen* 105, 1992, 243-59 at 251 sqq.), than Pre-Greek (Beekes, II, 1043); on *\*og<sup>u</sup>elós* cf. Plath, 256 «denn das Griechische hat in anderen Fällen öfters in den anderen indogermanischen Einzelsprachen nur schlecht oder gar nicht bezeugte Lexeme weitergeführt und gelegentlich sogar durch Derivation auf dem nominalen wie auf dem verbalen Sektor zu einem umfangreichen System ausgebaut». Θάνατος and θνήσκειν perpetuate the Egyptian *tni* < *tni*, ‘to be weak of old age’, ‘(mark) of old age’, by an euphemism of the same kind as the one assumed by Chantraine to drag in the IE (p. 409); that the verb *tni* is euphemistic for death is a guess<sup>47</sup>, unlike the PIE *\*√d<sup>h</sup>enh<sub>2</sub>-* (*LIV*<sup>2</sup>, 144-5; *contra*, Beekes, II, 533-4). Bernal tars us with the same brush p. 302: ὕβρις adapts *wr-ib*, ‘of great / wide heart’, either in good part (‘brave, magnanimous’) or negatively (‘insolent’ Ptahhotep, 8.10), never mind that the expected idea of power is recovered through PIE *\*√h<sub>2</sub>ek<sup>’</sup>-*, ‘to be(come) sharp, pointed’ (D. S. Wodtko, B. Irslinger, and C. Schneider, *Nomina in indogermanischen Lexikon* [Heidelberg, Winter, 2008], 287-300), cf. Nikolaev, “Die Etymologie von altgriechischem ὕβρις”, *Glotta* 80, 2004, 211-30 at 211-21, criticized by Beekes, II, 1525. How may *wr-ib*<sup>48</sup> persuade, though, when the state of the Egyptian morphology and phonology at each historical stage when borrowing is supposed by Bernal to have taken place, never receives any attention in *BA III* (S. Patri, ‘La perception des consonnes hittites dans les langues étrangères au XIIIe siècle’, *Zeitschrift für Assyriologie und vorderasiatische Archäologie* 99, 2009, 87-126 at 93-6, 99-113)? Nonetheless Bernal asserts bluntly that the New Kingdom was crucial, with the Peripheral Akkadian of the royal letters acting as a buffer. He even equates a Greek word to an Akkadian one he suspects of being a loanword from Egyptian so that he can reconstruct this ‘etymon’. On 219-223, he hypothesizes the serial borrowing *p3 sr* > Boğazköy Akkadian *pa-ši-ia-ra* > Linear B *qa/pa<sub>2</sub>-si-re-u*, Cypriot *pa-si-le-wo(-se)* (~ *pa-si-le-o(-se)*): attestations in M. Egetmeyer, *Le dialecte grec ancien de Chypre* [Berlin-New York, De Gruyter, 2010], I, § 527, pp. 417-8)

<sup>47</sup> Did it replace *mwt*, ‘to die’, as did *šm*, ‘to go’ (J. Zandee, *Death as an Enemy According to Ancient Egyptian Conceptions* [Leiden, Brill, 1960], 54-5)? There is a use of *tni* for ‘to be miserable’: Goedicke, “The Letter to the Dead, Nag<sup>c</sup> Ed-Deir N 3500”, *Journal of Egyptian Archaeology* 58, 1972, 95-8 at 96. Compare G. Lambin, *La chanson grecque dans l’Antiquité* (Paris, CNRS, 1992), 106-25 with the endnotes at 411-8, for death qua object of songs and the various euphemisms used to broach it.

<sup>48</sup> Bernal is far from suspecting that *ib* qua ‘(physical) heart’, in competition with *h3ty*, has been doubted by J. H. Walker, *Studies in Ancient Egyptian Anatomical Terminology* (Warminster, Aris & Phillips, 1996), 169-71; on *ib* qua ‘self, psyche’, idem, 172-5. It might turn out that he was right: the P. Zagreb 601, c. 27.13, says *iw ib n nk<sup>c</sup> h3tiw*, ‘oh, you who take the *ib*-heart and tear out the *h3tj*-hearts’ (I. Urañić, “Book of the Dead Papyrus Zagreb 601”, *Studien zur Altägyptischen Kultur* 33, 2005, 357-71 at 365).

> βασιλεύς (not a title: Perpillou, § 452, pp. 392-3), a monstrosity (*CR* 63, 143; *Aristarchus antibarbarus*, 31-2). He could have made the Akkadian *pa-ši-ia-ra* the name of one of the Egyptian senders of this Boğazköy letter, on the strength of the equivalence \**P3-sirw*: **ΠΕCΙΟΥΡ**, ‘the (ΠΕ) eunuch (**CΙΟΥΡ**: M. Fieger - S. Hodel-Hoenes, *Der Einzug in Ägypten. Ein Beitrag zur Alttestamentlichen Josefsgeschichte* [Bern-Oxford, Lang, 2007], 86-7)’: Ψιοῦρις (F. Preisigke, *Namenbuch enthaltend alle... Menschnamen, soweit sie in die griechischen Urkunden Ägyptens sich vorfinden* [Heidelberg, s.ed., 1922], col. 493), probably ‘LePrince’ since Pharaonic Egypt never had eunuchs, but no! Bernal links **ΠΕCΙΟΥΡ**... to ψιλός (pp. 223-4). Never did he ask himself whether it was standard practice, for a diplomatic letter in Akkadian, to be addressed by nameless ‘Grandees’ (LU<sup>MES</sup>.GAL<sup>MES</sup>) and *p3 sr*, ‘the official’ (!). This highly codified genre always names or identifies the correspondents<sup>49</sup>, so Bernal cannot be right. He must not have read E. Edel at all (references to his works at *CR* 63, adding “Zwei Originalbriefe der Königsmutter Tūja in Keilschrift”, *Studien zur Altägyptischen Kultur* 1, 1974, 105-46 at 131-2; also Vycichl, 185-6 s.v. **CΙΟΥΡ**). The mutual influences of Egyptian and Akkadian require care, and not haste, if they are to be broached in a study of the Levantine impact on the Greek language<sup>50</sup>.

A further defect in *BA III* is its indigent acquaintance with facts of civilization. Egyptian titlature suffers, either because Bernal misrepresents data at the brick-and-mortar level, e.g. *nsu(t)*, ‘king’, and *kn(i)* (his *qni*), ‘effective, brave, valiant’ (p. 386)<sup>51</sup>, or because he roots vastly inflated

<sup>49</sup> W. L. Moran, *The Amarna Letters* (Baltimore-London, Johns Hopkins, 1992), XXII-XXIII; J. Mynarova, *Language of Amarna - Language of Diplomacy. Perspectives on the Amarna Letters* (Prague, Czech Institute of Egyptology, 2007), 126-30. «The primary identification of both correspondents must be purely functional, providing the respective names and the functions of the two persons. Only when it is assured that the message will be delivered to the right person and the addressee will certainly identify the identity of the sender, the second level of identification containing the social status of both correspondents (...) can follow» (p. 126). For Peripheral Akkadian titles, cf. W. Heimpel, *Letters to the King of Mari* (Winona Lake, Eisenbrauns, 2003), 571-604 *passim*.

<sup>50</sup> A. Militarev, “Akkadian-Egyptian Lexical Matches”, in C. L. Miller (ed.), *Studies in Semitic and Afroasiatic Linguistics Presented to Gene B. Gragg* (Chicago, Oriental Institute, 2007), 155-63 at 155-60, marshals Akkadian borrowings in Egyptian; Z. Cochavi-Rainey, “Egyptian Influence in the Akkadian Texts Written by Egyptian Scribes in the Fourteenth and Thirteenth Centuries B. C. E.”, *Journal of Near Eastern Studies* 49, 1990, 57-65, does the reverse and insists on Egyptianisms, especially in the Boğazköy letters; A. Loprieno, “On an Egyptianism in Akkadian”, *Orientalia Lovaniensia Periodica* 27, 1996, 23-7, demonstrates the construction *ana* plus infinitive and suffix or dependent genitive to be the calque of the idiom *rd(i)* + prospective *sdm=f*.

<sup>51</sup> Grimal, *Termes*, 125-8 (§ II 7:1), 708-9 note 843; D. M. Doxey, *Egyptian Non-Royal Epithets in the Middle Kingdom. A Social and Historical Analysis* (Leiden, Brill, 1998), 110-4 and 74, 124, 254; A. R. Schulman, “Some Observations on the Military Background of the Amarna Period”, *Journal of the American Research Center in Egypt* 3, 1964, 51-69 at

claims in unstable grounds. The title *idnw*, when used separatim, means the ‘deputy’ of a higher up (then the nature of the function is normally clear in the mind of the writer as determined by the context), whereas it specifies, in compounds, the holder of the inferior, or medium-level, charge that immediately follows<sup>52</sup>. *BA III*, p. 88, states that *idnw* inspired the West Semitic ‘lord’<sup>53</sup>, Ugaritic ‘*adn*, Phoenician ‘*dn* (Krahmalkov, *A Phoenician-Punic Grammar* [Leiden-Boston-Köln, Brill, 2001], 135, 136; idem, *Phoenician-Punic Dictionary*, 34-8), Punic *dn* (Krahmalkov, *A Phoenician-Punic Grammar*, § 4d, 36-7; Hoftijzer-Jongeling, I, 15-7), and Hebrew ‘*ādôn* / ‘*ādhôn*, with the terse comment that ‘this terminology reflects the difference of power between the two regions’. The idea belongs to S. Yelvin and W. F. Albright and, while interesting, lacks conclusive evidence (O. Eissfeldt, וַדָּן ‘*ādḥôn*; וְדָן ‘*dhōnāi*’, *Theological Dictionary of the Old Testament*, I [‘Revised edition’, Grand Rapids, Eerdmans, 1974], 59-73 at 59-60). The material civilization of Egypt is not handled better. The standard measure of grain *ḥzr* becomes a nonsensical *ḥar* p. 218; p. 227, \**p3 th* translated ‘the beer jug’ is equated to the sign W22 (ḥ), for which such a gloss obtains (Gardiner, p. 530), yet a vessel for drinking *th* does not seem to exist; p. 368, a verb *mgzr* declared to mean ‘broil or grill’ and etymologize μαγειρεύειν, μάγειρος has been misread from Hoch, 170-1 n°227 — it is actually *mgr*, a late hapax whose value remains elusive (Lesko-Switalski Lesko<sup>2</sup>, I, 212, endorses Hoch, but cf. Takács, III, 679-80.2-3); worse, the derivation of the Greek words cannot be accepted (Takács, 678-9 N.B 3)<sup>54</sup>.

57 note 39 (*kni*).

<sup>52</sup> Erman-Grapow, I, 154, *Belegstellen*, I, 28; Lesko-Switalski Lesko<sup>2</sup>, I, 54 (‘deputy, lieutenant, adjutant, lieutenant commander’); Hannig, II.1, 455-6; E. F. Morris, *The Architecture of Imperialism. Military Bases and the Evolution of Foreign Policy in Egypt’s New Kingdom* (Leiden-Boston, Brill, 2005), 491-5.

<sup>53</sup> The Ugaritic ‘*adn* signifies ‘father’ too; this dual meaning (Del Olmo Lete-Sanmartín, I, 18-9; J. Huehnergard, *The Ugaritic Vocabulary in Syllabic Transcription* [Atlanta, Scholars Press, 1987], 48, 104, who posits /\**adānu*l, ‘lord; father, head of the family’; A. Rahmouni, *Divine Epithets in the Ugaritic Alphabetic Texts* [Leiden-Boston, Brill, 2008], 26 note 4; M. S. Smith - W. T. Pitard, *The Ugaritic Baal Cycle Volume II. Introduction with Text, Translation and Commentary of KTU/CAT 1.3-1.4* [Leiden, Brill, 2009], 291) remains vexing even when one considers that social structures can be shaped (or mirrored) in familial ones, cf. H. J. Marsman, *Women in Ugarit and Israel. Their Social and Religious Position in the Context of the Ancient Near East* (Leiden, Brill, 2003), 133. Tropper’s *Ugaritische Grammatik* (Münster, Ugarit, 2000), eschews this duality of values in its 1056 pages: the word, which he never discusses, is rendered everywhere ‘Herr’ (viz. its sole meaning in prose); contrast D. Sivan, *A Grammar of the Ugaritic Language* (‘Second Impression with Corrections’, Leiden-Boston-Köln, Brill, 2001), 68, 73, P. Bordreuil and D. Pardee, *A Manual of Ugaritic* (Winona Lake, Eisenbrauns, 2009), I, 295 (glossary), and read further Halayqa, 36.

<sup>54</sup> Far more natural verbs for ‘to b(r)oil, cook’ are *p/psi*, cf. the substantive *p/psw* (Hannig, I, 489; Takács, II, 114-5), and *wdd* (Takács, II, 101; Hannig, II.1, 757), cf. *wdp.w*, ‘cook’ but much oftener ‘butler, attendant, steward’ (Jones, 406-7 n°1494-7; Kahl, *Frühägyptisches Wörterbuch*, I, 127; Hannig, I, 391-2, II.1, 750-4; L. D. Morenz, “Zu einem scheinbar enigma-

The history of ideas fares badly too, not so much because a correspondence more striking than most of its kind has been missed<sup>55</sup> than because *BA III* conflates the Greek gods with their Egyptian or West Semitic counterpart with little regard for phonetics (and, *pace* p. 254, Β/βράκχος cannot stem from בְּרָכִי: Steiner, “On the Rise and Fall of Canaanite Religion at Baalbek: A Tale of Five Toponyms”, *Journal of Biblical Literature* 128, 2009, 507-25 at 520 note 72). The history of techniques gets equally short shrift. P. 100, Bernal borrows from Gamkrelidze-Ivanov (I, 616-7) a PIE root *\*r(e)udh-*, ‘red, red metal, copper’, and contends that, if the Hebrew «*kōp(p)er* “red” also meant “copper,” Kypros / Cyprus seems to be a West Semitic name for the island famous for its copper, rather than the toponym originating the metal name found in the Latin *cuprum* etc». Now this is a laryngealist root, *\*h<sub>1</sub>/Hreudh-* (Mallory-Adams, *Encyclopedia*, 379; D. Stifter, “Study in Red”, *Die Sprache* 40, 1998, 202-23 at 206-7; B. A. Olsen, “Another Account of the Latin Adjectives in *-idus*”, *Historische Sprachforschung* 116, 2003, 234-75 at 258-9; *The Oxford Introduction to Proto-Indo-European and the Proto-Indo-European World* [Oxford-New York, Oxford University Press, 2006], 332; Wodtko-Irslinger-Schneider, 580-5); *pace* Gamkrelidze-Ivanov, ‘copper’ seems a mere semantic extension thus should not be assigned to the PIE (*Encyclopedia*, 379-80); and Bernal ignores that the best work on the ancient names of Cyprus rules out a Semitic connection with this metal (Palaima, *The Triple Invention of Writing in Cyprus and Written Sources for Cypriote History* [Nicosia, A. G. Leventis Foundation, 2005], 21 with 47 note 36; Egetmeyer, I, 24, 243)<sup>56</sup>. Finer points have this lover of generalities who cannot tell when they are warranted (compare pp. 263-4, on φιλοσοφία versus Egyptian teachings, with B. Alster, *Wisdom of Ancient Sumer* [Bethesda, CDL, 2005], 18-24; he shuns the Greek Romance, despite G. A. A. Kortekaas, *Commentary on the Historia Apollonii Regis Tyri* [Leiden-

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tischen Epitheton eines Meisterschlachters aus dem späten Alten Reich”, *Journal of Egyptian Archaeology* 84, 1998, 195-6). I shall cite Schulman, “The Royal Butler Ramessesemperrē”, *Journal of the American Research Center in Egypt* 13, 1976, 117-30 at 127 note 10, for the concurrence between the readings *wdp.w* and *wb3* (especially in the title *w...nsw*, ‘Royal Butler’).

<sup>55</sup> For C. A. Faraone - E. Teeter, “Egyptian Maat and Hesiodic Metis”, *Mnemosyne* 57, 2004, 177-208, ‘Maat was in some way or another a source or inspiration for Hesiodic Metis’ (p. 202). This is far sounder than *m3<sup>c</sup>.t* (Ma<sup>c</sup>at) > μοῖρα (*BA III*, 269-70).

<sup>56</sup> Cf. also O. Poltera, *Le langage de Simonide. Étude sur la tradition poétique et son renouvellement* (Bern, Lang, 1997), 264, and the forgotten etymological discussion of Κύπρος by S. Paliga, “Metals, Words and Gods. Early Knowledge of Metallurgical Skills in Europe, and Reflections in Terminology”, *Linguistica* 33 (*Bojan Cop septuagenario in honorem oblata*), 1993, 157-76 at 163-5. Neither his hypothetical root *\*koP-* / *\*kuP-*, ‘small, little’, which would reflect an archaic, pre-Indo-European substratum, nor his attempt to link it to the island carry much weight, but his interesting effort shows that Semitic origins are not our only way out («the P(ersonal) N(ame) Kýpros cannot be any longer considered enigmatic as long as many other parallels can be quoted », p. 164).

Boston, Brill, 2007], 428, or S. Panayotakis, *The Story of Apollonius, King of Tyre. A Commentary* [Berlin-Boston, De Gruyter, 2012], 365-6; and so on) rather at sea. He uses Karum Kanesh as a blanket toponym (pp. 91, 114): at Kaneš, the lower city was a commercial hub, whence the short-hand *kārum* Kaneš to distinguish it from the city mound (C. Michel, *La correspondance des marchands de Kanish* [Paris, Cerf, 2001], 25-30; K. R. Veenhof, in idem and J. Eidem, *Mesopotamia. The Old Assyrian Period* [Fribourg / Göttingen, Academic Press / Vandenhöck & Ruprecht, 2008], 41-6). Bernal's dating of *kārum* Kaneš II is misguided (p. 601 note 3, with a shadowy reference to 'Manning *et al.* (2001)') and he makes a travesty of the linguistic situation by speaking of the 'early Hittite names' attested there (p. 91: these Anatolian names are not only Hittite, viz. Indo-European, but Hattic, Luwian, and Hurrian).

Finally, Bernal's shaky acquaintance with the Greek literature pervades almost every page. Perhaps the most glaring instance appears on p. 474: broaching the topic of the birth of Apollo, he discusses what should have been introduced as the Pindaric hymn Θήβαις εἰς Δία (fr. 29-35 Snell-Maehler) and states that «Pindar enlarged the story» (told in the Homeric Hymn to Apollo) «with the claim that Delos was only the “mortal” name of the island, the immortal one was Ἀστερία “star”». However Ἀστερία does not appear in this composition; all fr. 33c says, in an elaborate set of appositions (P. Hummel, *La syntaxe de Pindare* [Paris, Peeters, 1993], 364), is that mortals call the island Delos discrepantly from 'blessed Olympians': ἄν τε βροτοί | Δᾶλον κικλήσκουσιν, μάκαρες δ' ἐν Ὀλύμπῳ (33c, 4-5). Pindar merely has Asteria once as the «sister of Leto, daughter of Koios and Phoebe; a Titan pursued by Zeus and turned into the island of Delos» (W. J. Slater, *Lexicon to Pindar* [Berlin, De Gruyter, 1967], 76). Bernal must not like the Theban poet, for on another occasion he neglects to source his Ἄμμων Ὀλύμπου δέσποτα (fr. 36), from which an identification of Zeus with the Egyptian Am(m)on would stem, instead of the cultic name it can only be. Discarding all such quirks, we must now pause to consider the ways the all-important early Greek poetry features in *BA III*. For it is no mean achievement of Bernal to etymologize numerous Greek words, toponyms, and gods first to be found in the Homeric epics without ever paying heed to formulaicity, oral composition, and other concepts and bibliographical resources familiar to Homerists, nor taking into account the characteristics of epic style. In the case of the Homeric *glossa*, this comes tantamount to renouncing verisimilitude. Bernal does not disappoint when he is faced with γλαυκῶπις, pp. 578-9. He paraphrases it without further ado 'pale and brilliant eyes', transferring the rival interpretation, from γλαυῖξ (which he does not mention as the equally possible alternative it is), to the goddess Neith from which name Athena in his view ultimately derives. However (*Aristarchus antibarbarus*, p. XII), γλαυκῶπις < γλαυκός is at least as



likely to be an early re-interpretation of this archaic lexeme ossified in the formula and no longer clear to the poet, than its original shade of meaning<sup>57</sup>. For γλαυκός cf. of late C. L. Wilkinson, *The Lyric of Ibycus* (Berlin-Boston, De Gruyter, 2013), 247-8. Apart from twelve (!) lines on the Greek article in Homeric and early Greek (pp. 162-3, drawing on Meillet's *Aperçu* instead of Chantraine's *Grammaire homérique*), everywhere in *BA III* the reader remains in the dark as to the uniqueness of the Homeric diction. Not only is it an artistic medium (*Kunstsprache*), quite unlike everything else in the Greek literature; it shows to a high degree the influence of Mesopotamian and Levantine narrative traditions. When at last this became inescapable, in the sixties, so great was the zest for this dependence of Homer or his tradition on the Near East that Ugaritic poetry was identified as the model for a majority of the stylistic devices the *Iliad* and the *Odyssey* rely on<sup>58</sup>. That there exists an intricate intertwining of Hellenic, if not Indo-European, motifs and Near Eastern themes in the monumental epics, was eventually recognized, and the so-called Levantine roots of the Homeric technique fell

<sup>57</sup> This undermines S. Deacy - A. Villing, "What Was the Colour of Athena's Aegis?", *Journal of Hellenic Studies* 129, 2009, 111-29 at 121-4 («it was coloured in ways that denoted Athena's protective and inherently ambivalent, and potentially destructive, power», p. 125). Nor is γλαυκώπις hers exclusively, for it applies to Hera in the anonymous *Palatine Anthology* 9.189.1 ἔλθετε πρὸς τέμενος γλαυκώπιδος ἀγλαῶν Ἥρας, naturally enough (D. L. Page, *Further Greek Epigrams* [Cambridge, Cambridge University Press, 1981], 338: «the transference of γλαυκώπις from Athena to Hera is not more remarkable than the transference of βοῶπις from Hera to Artemis in Bacchylides 11.99, or to Amphitrite in 17.110, or to Harmonia in Pind. Pyth. 3.91; it is less surprising than the transference of Ἀργειφόντης from Hermes to Apollo in Sophocles fr. 1024»). In any case, K. Tümpel, "Lesbiaka", *Philologus* 50, 1891, 566-8 at 567 note 11, has a pointed explanation. So beware, like Bernal, of taking too rigid a new of the ritual epithets in early poetry and religion: S. P. Morris, *Daidalos and the Origin of Greek Art* (Princeton, University Press, 1995), 52-3, 56-7.

<sup>58</sup> C. H. Gordon, *Before the Bible. The Common Background of Greek and Hebrew Civilizations* (New York, Harper & Row, 1962), 128-205; A. F. Campbell, "Homer and Ugaritic Literature", *Abr-Nahrain: An Annual* 5, 1965, 29-56; P. Walcot, "The Comparative Study of Ugaritic and Greek Literatures I-III", *Ugarit-Forschungen* 1, 1969, 111-8, 2, 1970, 273-5, and 4, 1972, 129-32; M. Maróth, "Epischer Stil im Ugaritischen und im Griechischen", *Acta Antiqua Academiae Scientiarum Hungaricae* 23, 1975, 65-76. They were arguing against the influential contentions of T. B. L. Webster, *From Mycenae to Homer* (London, Methuen, 1958), 64-90, for whom «the manner of Eastern poetry has many parallels in Homer, and this comparison may lead to useful inferences about the manner and performance of Mycenaean poetry and about its preservation in Homer. By itself however this would show that Mycenaean poetry was a local variant of Eastern Mediterranean poetry (just as we have seen that Mycenaean civilization and Mycenaean art is a local variant of Eastern Mediterranean civilization and art), but not that Mycenaean poets borrowed Eastern themes» (67). We are now in a far better position to assess the situation; the contacts between the Greeks and people from the Levant in the Proto-Geometric period had the former rediscover the cultural climate of exchange of the Mycenaean age that they remembered from their oral poetry (S.-T. Teodorsson, "Eastern Literacy, Greek Alphabet, and Homer", *Mnemosyne* 59, 2006, 161-87 at 166-9; this might well have led to the creation and writing down of the Homeric epics: idem, 172-5).

into disregard<sup>59</sup>. No matter the stance of Bernal with respect to the Graeco-Egyptian acculturation, it is cheap of him to confine to oblivion the strong Mesopotamian flavor of the *Iliad* and *Odyssey* whenever he deals with epic myths; heroic personal names, such as Ἀχιλλεύς (Perpillou, 241 note 8; H. von Kamptz, *Homerische Personennamen* [Göttingen, Vandenhöck & Ruprecht, 1982], 348), Μελάμπους, Πηλεΰς (Perpillou, § 204, pp. 183-4; Von Kamptz, 300-1); the origins of Zeus, Apollo, Athena, Demeter, etc; and ethnics, toponyms, or astynyms for which Homer is our oldest testimony. Intertextuality along well-trodden paths is not indifferent to the core of these instances; yet *BA III* tramples the principles of sound interpretation of the epics in order to treat snippets from them as open to whatever fancy floats Bernal's boat<sup>60</sup>. *Nihil vitiosus*.

Lefkowitz quipped that «Bernal is concerned with making particular arguments, not with constructing a coherent hypothesis» (“Black Athena: The Sequel”, 602). My reader has now reason to suspect that she was just being kind. As the book operates in the realm of possibilities and chance, it hardly bothers to build a case for its Afroasiatic derivations whenever the evidence is in short supply. Time and again the reader faces propositions asserted *ex cathedra* of the form ‘X admits of no IE etymology, yet Semitic preserves Y or Z’, viz. absurdities qualified by philological leaps of faith. Thus «*bāyay*, *bî* are particles of entreaty in Arabic and Hebrew, used in formulae of address to superiors. Βαίος [sic] (5) is ‘small, without importance, mean and humble’. It has no Indo-European etymology» (p. 409) with ‘*baios*, an obscure form of humble address’ on 413. Only through ignorance of sociolinguistics can someone see an address in this adjective used in the neuter singular with an adverbial value, *paulisper*, from Hesiod on (*Works and Days*, 418), or in the nominal group χρόνον βαιόν (see E. Dickey,

<sup>59</sup> See B. Louden, *The Iliad. Structure, Myth, and Meaning* (Baltimore, Johns Hopkins University Press, 2006), 5-9. Louden is the main contemporary protagonist of the elucidation of the Homeric poems through the Hebrew Bible and Ugaritic epics, yet even he does not revive the claims of Gordon or Maróth that the style of the latter compositions served as the main inspiration for Homer. The bibliographies on the Homeric indebtedness to the Near Eastern epos compiled by J. L. Ready, “Zeus, Ancient Near Eastern Notions of Divine Incomparability, and Similes in the Homeric Epics”, *Classical Antiquity* 31, 2012, 56-91 at 58 notes 8-10, are a boon; J. M. Foley (ed.), *A Companion to Ancient Epic* (Malden, MA, Blackwell, 2005), 215-63 and 277-87, provides their context.

<sup>60</sup> This recipe for disaster originates in a vastly skewed notion of the achievements of Greek scholars with respect to *Hellenorientalia* and their impact on Homer. In its narrative of the post-1945 situation, *BA I*, 400-38, remains mum on the progressive discovery of the links between the *Iliad* and the epic of Gilgamesh by Homerists, as if the contemporary scholarly blackout on the part played by the Phoenicians during the Greek protohistory was not justified, after the fifties and the sixties, by the evidence for such links with the Mesopotamian corpus (*Aristarchus antibarbarus*, 185 note 3, has more details). Bernal's misshaped handling of Homer in *BA III* was consequently to be expected; it could only have been avoided had his Greek scholarship ripened between 1987 and 2006.

*Greek Forms of Address. From Herodotus to Lucian* [Oxford, Clarendon Press, 1996], 4-19, cf. 247-8). What is more, βασιός, though deprived of IE connections (Prellwitz, Boisacq are antiquated), does not miss cognates, per F. A. Wood, "Etymologische Miscellen", *Zeitschrift für vergleichende Sprachforschung auf dem Gebiete der Indogermanischen Sprachen* 45, 1912, 61-71 at 64. Quacks of the ilk of I. Van Sertima claimed that 'Black' Africans discovered America before Columbus, only to be met with scorn once it was demonstrated that these Afrocentrists falsified the record to extinguish the achievements of Native Americans<sup>61</sup>; Bernal blunts the edge of the linguistic data, yet protests that his appeals to a paradigmatic shift within the research on Graeco-Semitic relationships fell on deaf ears. Who of the two is more guilty here, pray tell?

The will to operate within a framework far broader than the ones experts of Greek or Indo-European normally use<sup>62</sup> counts for little without preparation (the patient collation of data), flexibility (their painstaking elaboration), and sound common sense (the final decision, with an eye on the most recent scholarship available<sup>63</sup>, between what looks almost certain, what

<sup>61</sup> B. Ortiz de Montellano - G. Haslip-Viera - W. Barbour, "They Were NOT Here before Columbus: Afrocentric Hyperdiffusionism in the 1990s", *Ethnohistory* 44, 1997, 199-235, offers a comprehensive hecatomb, cf. p. 218 «it is quite clear from the foregoing that claims of an African presence in pre-Columbian America are purely speculative, rigidly diffusionist, and have no foundation in the artifactual, physical, and historical evidence. Nevertheless, the Afrocentric position is routinely articulated in a very forceful manner with few if any caveats». Their "Robbing Native American Cultures: Van Sertima and the Olmecs", *Current Anthropology* 38, 1997, 419-41, is much more constructive than Bernal's emotional reply ("On 'Robbing Native American Cultures'", *ibid.* 39, 1998, 512-4); cf. also Ortiz de Montellano, "Black Warrior Dynasts: l'afrocentrisme et le Nouveau Monde", in F.-X. Fauvelle-Aymar - J.-P. Chrétien - C.-H. Perrot (edd.), *Afrocentrismes. L'histoire des Africains entre Égypte et Amérique* (Paris, Karthala, 2000), 249-73. The level of Van Sertima's dealings with Egypt is equally atrocious, if not worse: Slack, 215-20, 224-6.

<sup>62</sup> It is easy to abuse the comparative method to try and reconstruct a common IE inheritance from previously unexplained puzzles of a mythographic or philosophical character in two cognate languages. Let mentions of B. Sergent, *Athéna et la grande déesse indienne* (Paris, Belles Lettres, 2008) («ce que je reproche à Bernard Sergent, c'est d'avoir ratissé large sans se préoccuper de l'histoire des traditions si disparates qu'il sollicite. La comparaison doit avant tout rapprocher des traditions homogènes» E. Pirart, *La naissance d'Indra. Approche comparative des mythes de l'Inde ancienne* [Paris, L'Harmattan, 2010], 63; cf. note 153), and of the needlessly long-winded A. Pinchard, *Les langues de sagesse dans la Grèce et l'Inde anciennes* (Geneva, Droz, 2009) (647 p.!), and D. Frame, *Hippota Nestor* (Washington-Cambridge, Mass, Center for Hellenic Studies, 2009) (922 p.!!), both of which violate Wortphilologie while going overboard in their analogical comparative dealings, suffice as evidence that this pitfall of the game is gaining popularity. While they do not invalidate the investigation of IE poetics as a whole, such attempts put it beyond any doubt that the range of PIE is so massive as to prove hard to control at the brick-and-mortar level. A far larger range yet, *more Bernalio*, is clearly unmanageable.

<sup>63</sup> I cannot postpone any longer the exposition of one of the most irritating features of *BA III*: Bernal has linked together the three tomes of the series in an undesirable way by

seems likely, what is merely possible, and what one would like to squeeze out of the evidence). Otherwise results, meager as they are, are achieved at the expense of historical probability and comparative linguistics. Instead of leading the reader by the hand along those very guidelines towards what he feels is the truth, Bernal prefers to give him an impatient shove. The obvious poverty of the Indo-European pedigree of the Greek words he preyed upon, being copied (not altogether correctly) from the outdated Pokorny, Frisk, or Chantraine, leaves the unwary user in no doubt that the best course of action is the Semitic connection put forward so confidently. This shifts the onus of the proof from the author to the critic in a move which subverts scholarly standards of decency, while the brisk pace of the book makes it sure that Semiticless readers will not pause and peak too closely at the credentials of its Egyptian or Levantine etyma. The monograph which Bernal advertizes as a linguistic revolution thus sinks each time one of the three things that follow happens: either it is discovered that the author's Near Eastern materials are ghost words (forged by him, miscopied from a source, misprinted in the typescript, etc) or inspection demonstrates that he has abused them (by editing their translation, by pressing them too far, by fabricating unwarranted or conjectural semantic links, and so forth) or a technically defensible PIE etymon exists that Bernal took no notice of owing to his strained lexicographical basis. The conspicuous marks of too-lengthy pregnancy in *BA III* confirm this bleak view: the bibliography,

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referring us back to *BA I* or *BA II* as if science had not moved on in the two decades that elapsed since these books appeared. Thus all *BA III* has to say on the Ionians, viz. 'the form *iwntyw* provides a plausible origin for Ἴωνες' (p. 211), comes with note 6 on p. 628 'see Vol. 1, 83, and Vol. 2, 129'; his only discussion is in *BA I*, and it magisterially disregarded modern scholarship, not citing one study. *Iwn.tiw*, plural *Iwn.tyw* (the scriptural context in J. Vercoutter, *L'Égypte et le monde égéen préhellénique. Étude critique des sources égyptiennes (du début de la XVIII<sup>e</sup> à la fin de la XIX<sup>e</sup> Dynastie)* [Cairo, IFAO, 1956], 79-81), literally 'bowman' because it was felt to be an Asiatic emblem (T. Säve-Söderbergh, in *Lexikon der Ägyptologie*, I [Wiesbaden, Harrassowitz, 1975], coll. 845-6), means 'tribesman' (Faulkner), 'nomad', 'beduin', 'Asiatic foreigner', 'troglodyte', cf. C. Leitz (ed.), *Lexikon der ägyptischen Götter und Götterbezeichnungen*, I (Leuven, Peeters, 2002), 193, or Hannig, I, 60, II.1, 145. The true equation behind Ἴωνες, Ἰάωνες is with the Assyro-Babylonian and Aramaic *Yāw(a)naya* / *Yām(a)naya*, cf. Demotic *winn* (R. Rollinger, "Zur Bezeichnung von 'Griechen' in Keilschrifttexten", *Revue d'Assyriologie* 91, 1997, 167-72, building on J. A. Brinkman, "The Akkadian Words for 'Ionia' and 'Ionians'", in R. F. Sutton (ed.), *Daidalikon. Studies in Memory of R. F. Schoder* [Wauconda, Bolchazy-Carducci, 1989], 53-71), 'Ionians', viz. 'Greeks, Greek speakers': Brinkman, 54-6; Rollinger, "The Ancient Greeks and the Impact of the Ancient Near East. Textual Evidence and Historical Perspectives (ca. 750-650 BC)", in R. M. Whiting (ed.), *Mythology and Mythologies. Methodological Approaches to Intercultural Influences* (Helsinki, Neo-Assyrian Text Corpus Project, 2001), 233-64 at 237-9; O. Casabonne - J. De Vos, "Chypre, Rhodes et l'Anatolie méridionale: la question ionienne", *Res Antiquae* 2, 2005, 83-102 at 84-7 (probably the best brief summary); and N. Luraghi, "Traders, Pirates, Warriors: The Proto-History of Greek Mercenary Soldiers in the Eastern Mediterranean", *Phoenix* 60, 2006, 21-47 at 30-3.

pp. 741-95, contains titles up to 2003, in tiny numbers, but scholarship and textual findings from the nineties are called 'recent' in the notes or the text; the book includes whole chunks of *Black Athena Writes Back* lifted verbatim, a fact that invites caution, if not dismay; the sequel of at least one major study to which Bernal pays lip-service, went unnoticed although it came out early enough (Levin, *Semitic and Indo-European. Volume II Comparative Morphology, Syntax and Phonetic* [2002]; note 180); lexica, handbooks, and monographs crop up in a thoroughly erratic way, with no rationale behind their appearance at some point and their absence everywhere else<sup>64</sup>; and the fanciful, disappointingly thin indices surpass the endnotes (pp. 587-694) in inadequacy<sup>65</sup>. Needless to say, the amount of rubbish in these sections is staggering; Bernal could not even be bothered to transcribe titles accurately (p. 753: 'Elementis Alexandrini Opera', instead of *Clementis Alexandrini Opera*; p. 787, within the same entry: 'annotionibus', 'Leipsig'; etc). As the method behind the work is sheer madness; as the learning of the author falls short of the lowest conceivable standards; and as his critical

<sup>64</sup> Note 171. E. Benveniste, *Le vocabulaire des institutions indo-européennes* (Paris, Minuit, 1969, 2 vol. = *Indo-European Language and Society*, London, Faber & Faber, 1973), is referred to in connection with the IE divide δόμος versus *domus* (309, with 645 note 37); about the IE roots *\*k<sup>w</sup>i* or *k<sup>w</sup>eī* (actually, *\*k<sup>w</sup>ei-*, *\*k<sup>w</sup>ēi-*; 242 with 634 note 187); and for the IE *king* (619 note 13) — not a large harvest for such a foundational work. To take one case-in-point, *BA III*, 326-7, would have been less bad, apropos of δοῦλος and its putative origins in Asia Minor, for exploiting Benveniste (1969, I, 357-9 = 1973, 291-2; R. Rosól, *Frühe semitische Lehnwörter im Griechischen* [Frankfurt am Main, Lang, 2013], p. 169). Similarly, why was R. Edwards' epoch-making *Kadmos the Phoenician. A Study in Greek Legends and the Mycenaean Age* (Amsterdam, Hakkert, 1979), never used in *BA III* once its delayed publication is mentioned (p. 4, with a misspelling)? The answer is: Bernal builds on Astour's demonstration that the contents of the myths of Kadmos are thoroughly West Semitic, yet it has been exploded in *Kadmos*, 139-61; thus the assertions of *BA III*, 466-7, on Kadmos and Europa qua the morning and evening stars, had to suppress Edwards, 142-6, or be ruled out. Finally, it is infuriating that Van Windekens' *Dictionnaire* was only used four times (with respect to αὐτός [p. 159, cf. note 18], ἀνάξ [637 note 99], νός [649 note 39], πέπλος [686 note 60]), and not systematically when Bernal tackles one of his words, like αἶμα (p. 5 ~ *BA III*, 329); Ἀφροδίτη (p. 29 ~ *BA III*, 234-7; far less foolishly than Bernal's *pr wzdyt*, 'temple-city' of *Wadjayat*, βοῦτο / ποῦτο, Βουτώ, M. L. West, "The Name of Aphrodite", *Glotta* 76, 2000, 134-8, reconstructs *\*Aprod/dīt*, from the Semitic √PRD / PRD, and seeks a likely Canaanite value of this root, which he finds in the Hebrew *prāzīt*, female dweller of a hamlet; so Ἀφροδίτη = 'She of the Villages', cf. Beekes, I, 179); or γυμνός (W., 59-60 ~ *BA III*, 387).

<sup>65</sup> Scattered between 712-40 and 797-807, owing to the intrusion of the bibliography in between, are a copious index of Greek words (713 sqq.), then what Bernal terms one of 'letter correspondences' (731 sqq.), and a listing of modern names and themes; the former does not replace the exhaustive Semitic index without which this maze of a book can hardly be used by students of the Near East, while the general index should have been far more comprehensive. Lacking too is an *index locorum*. The endnotes are many yet meager and seldom extend beyond the immediate needs of the main text, whose source(s) they barely manage to spell out, plus the odd qualification or ancillary matter; the reader with no other resource will starve on them. Finally, to make the work less forbidding, a glossary (pp. 695-711) explains, with Bernal's usual standards of inaccuracy, most technicalities and linguistic vocables.



faculty comes close to zero, the conclusion arrived at in *Black Athena Revisited* must be rehearsed: the ill-prepared Bernal overtaxed himself<sup>66</sup>. The staunchest proof of this verdict lies in his assimilation of the situation of Greek vis-à-vis Egyptian / Semitic, with that of Japanese vis-à-vis Chinese.

Both the Greeks and the Japanese borrowed their scriptural vehicle from an older, more advanced and culturally sophisticated language to which their own was totally unrelated; the analogy breaks down there, for differences nigh impossible to bridge appear so soon as one considers the particulars. «Historically, the Japanese were initiated into the art of writing by the Chinese and through the Chinese language. First using the Chinese language for purposes of written communication, they gradually developed ways of writing Japanese with Chinese characters. Thus, since the introduction of writing into Japan in the third or fourth century of our era, Chinese has played a vital role in the linguistic economy of Japan. Together with the script many Chinese words were borrowed into Japanese, the net result being that almost half of the morpheme inventory of modern Japanese is of Chinese origin. This Sino-Japanese stratum of the Japanese language serves a function similar to that of the stratum of Greek and Latin based words in European languages (...)»<sup>67</sup>. A complication<sup>68</sup> unmentioned by Bernal intervenes here,

<sup>66</sup> So, e.g., J. E. Coleman, on p. 294 «there is a positive side to Bernal's work, despite his many errors, the flaws in his methods, and the failure of his arguments to convince», and M. Liverani, on 421 «I take it as given that it is filled with too many logical and methodological inconsistencies, historical and philological mistakes, and documentary and bibliographical omissions to discuss here in detail». It should not be countered that «scholars belong to guilds held together by common opinions, attitudes, and methods. As a rule, innovation is welcome only when it is confined to surface details and does not modify the structure as a whole. For this reason, new interpretations of a problematic word or verse may be applauded by the very academicians who will stop at nothing to discredit a breakthrough destined to touch off a major reappraisal of the entire field», since such sociology of knowledge is typically brandished by interlopers (Gordon, *Forgotten Scripts. Their Ongoing Discovery and Decipherment* [‘Revised and Enlarged Edition’, New York, Basic Books, 1982], 35-6). I can almost predict Bernal's apology: «there are always those who believe exactly what they want to believe, facts notwithstanding. It is a mistake to take obscurantists too seriously, no matter how learned they happen to be» (Gordon, 130, predicated of the refusal of Egyptologists to accept Champollion's decipherment in the first decades after his death).

<sup>67</sup> F. Coulmas, “Language Adaptation in Meiji Japan”, in B. Weinstein (ed.), *Language Policy and Political Development* (Norwood, N.J., Ablex, 1990), 69-86 at 78. Courageous readers will benefit from M. H. Miyake, *Old Japanese. A Phonetic Reconstruction* (London, RoutledgeCurzon, 2003), 5-41, primarily 9-28; for most purposes, B. Frellesvig, *A History of the Japanese Language* (Cambridge, Cambridge University Press, 2010), 11-4, or L. J. Love-day, *Language Contact in Japan. A Socio-Linguistic History* (New York-Oxford, Clarendon Press, 1996, reprinted 2003), 26-43, offer enough on the introduction of (Chinese) writing.

<sup>68</sup> Historically-minded narrative in K. Heffernan, “The Gradual Rise of Bilingualism and the Use of Chinese as a Diglossic Language”, *Journal of East Asian Linguistics* 16, 2007, 61-86 at 64: «the gradual rise of bilingualism and the use of Chinese as a diglossic language during the Nara and early Heian periods (ca. 700-1000) was a result of the massive borrowing of cultural, religious, and political knowledge from China. At the time, the Japanese did not

namely that «the actual relation between E(arly) M(iddle) C(hinese) and O(ld) J(apanese) is anything but direct, as Chinese came to Japan by way of the Korean peninsula<sup>69</sup> and was very likely based on a different stage, if not variety, of Chinese; and finally, EMC is itself a reconstruct, i.e. hypothetical<sup>70</sup>» (Frellesvig, 30-1)<sup>71</sup>. The production of dictionaries and lexicographical works

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have their own writing system and therefore used Chinese as the medium for the exchange of knowledge. The reduction in bilingualism during the Heian period was a consequence of the Japanese gradually adapting Chinese writing to their own language, leading to a variety of native writing styles. Furthermore, civil war in China and court struggles in Japan greatly impeded cultural exchange. The result was a long period in which the Japanese did not travel to China, with a few exceptional cases of Buddhist monks. The consequence of this was the gradual decline in the level of oral fluency of Chinese at the Japanese court although Chinese remained the prestige language. On the other hand, the use of Chinese to recite the Buddhist scriptures helped spread a ritualistic version of the Chinese language from the nobility to the general populace».

<sup>69</sup> Japanese and Korean have strikingly similar typological and genetic profiles, cf. J. J. Song, *The Korean Language. Structure, Use and Context* (London-New York, Routledge, 2005), 17-8, or K.-M. Lee - S. R. Ramsey, *A History of the Korean Language* (Cambridge, Cambridge University Press, 2001), 26-30, 41-4, for a brief initiation, with further details in H.-M. Son, *The Korean Language* (*ibid.*, 1999), 18-36; as an example of case study, read Z. Song, "The Abstract Nominalizers in Korean and Japanese", *Linguistic Journal of Korea* 6, 1981, 157-93. Now, the inclusion of Korean and particularly Japanese in the Altaic hypothesis is a gambit (its Altmeister N. Poppe cautiously included the former in his *Introduction to Altaic Linguistics* [Wiesbaden, Harrassowitz, 1965], §§ 3.14 and 4.4, pp. 137-9, 163-4, but shunned the latter; it was reinstalled by R. A. Miller, *Japanese and the Other Altaic Languages* [Chicago-London, University of Chicago Press, 1971], pp. 48-292, in defiance of the popular notion that Japanese is an isolate). This enlargement of proto-Altaic remains disputed, cf. the discrepant views of S. Georg, P. A. Michalove, A. M. Ramer, and P. J. Sidwell, "Telling General Linguists about Altaic", *Journal of Linguistics* 35, 1998, 65-98 at 65-79, and of I. de Rachewiltz - V. Rybatzki, *Introduction to Altaic Philology. Turkic, Mongolian, Manchu* (Leiden-Boston, Brill, 2010), 348-55, especially 349-52; yet it is accepted as fact in the highly individual *Etymological Dictionary of the Altaic Languages* by S. Starostin and others (Leiden-Boston, Brill, 2003), 3 vol., which intermingles Korean-Japanese with the certain Altaic groups (Turko-Mongolian, Manchu-Tungusic: justifications at I, 230-4; most of the Korean-Japanese data glossed are otherwise unavailable to non-native speakers).

<sup>70</sup> On methodology, E. G. Pulleyblank, "How Do We Reconstruct Old Chinese?", *Journal of the American Oriental Society* 112, 1992, 365-82, has been epoch-making (read 365-77 for the critique of Karlgren's and his followers' Middle Chinese reconstructions); cf., since then, W. South Coblin, "The Chieh-yunn System and the Current State of Chinese Historical Phonology", *ibid.* 123, 2003, 377-83, notably 379-81, and D.-B. Hsu, "The Syllable in Old Chinese: Sub-syllabic Processes, Syllable Structure, and the Status of Medial Glides", *Journal of East Asian Linguistics* 18, 2009, 361-95 at 365-70. On EMC, the language used between the third and the seventh centuries AD, consult, e.g., A. Chan, "Early Middle Chinese Towards a New Paradigm", *T'oung Pao* 90, 2004, 122-62 (discrepant from Karlgren and Pulleyblank; gives a new reconstruction of the finals at 147-56), or Schuessler, *Minimal Old Chinese and Later Han Chinese. A Companion to Grammata Serica Recensa* (Honolulu, University of Hawai'i Press, 2009), 5-10.

<sup>71</sup> An attempt to shake off the Chinese (and Korean) spells on Japanese, by looking at Old Javanese, is made in A. Kumar, *Globalizing the Prehistory of Japan. Language, Genes, and Civilization* (London-New York, Routledge, 2009), 104-27, especially 111-9.

we are dependent upon for the oldest strata of Chinese began as early as the Han Dynasty and its achievement of unprecedented prosperity and stability without which neither education nor scriptural studies could really be expected to bloom. On the other hand, Early Middle Chinese rests on an unique source from the Sui Dynasty, Lu Fayan's dictionary of *Cut Rhymes* (*Qieyun*: 切韻, 切韻), completed in 601 AD; the testimonial significance of this composition raises thorny problems<sup>72</sup> even without broaching any of the comparative issues which bear on Old Japanese. Not only is EMC thus distant; a vital point to digest is that, during the centuries following the adoption of Chinese writing, the Japanese could choose one of three media for composing any text<sup>73</sup>: Chinese proper (*kanbun*, *jun kanbun*: official pieces, religious and cultic texts, academic works); the Chinese syllabic transcription of Japanese (mostly poetry); and the Chinese encoding of Japanese. By ca. 900 AD, *jun kanbun* steadily declined; the trend was for people to use that third medium and write Chinese with more and more infiltrators from their indigenous grammar and vocabulary. This uncouth *hentai kanbun*, verbatim 'variant Chinese-writing' (*kanbun* covers all compositions couched [=bun] in characters with a surface appearance of Chinese [=kan, the Han Dynasty]), eventually yielded to *wakan konkōtai*, the Sino-Japanese mixed prose style. *Hentai kanbun* is best epitomized in the 940 AD *gunki*, or war narrative, *Shōmonki* (Chronicle of Masakado, per the Japanese title); read G. Stramigioli, "Preliminary Notes on *Masakadoki* and the Tairo no Masakado Story", *Monumenta Nipponica* 28, 1973, 261-93 (264-6 on the style), and T. B. Hare, "Reading Kano no Chōmei", *Harvard Journal of Asiatic Studies* 49, 1989, 173-228 at 173-4 (Chōmei wrote in 1212 AD *Hōjōki*, a literary testament under the guise of a religious confession that is widely recognized as the first masterpiece of *wakan konkōtai*). So I invite the reader to decide whether the Japanese side of Bernal's analogy<sup>74</sup> qua a «saturat[ion]

<sup>72</sup> Miyake, *Old Japanese*, 93-5, repackages older views and shows that the Middle Chinese of *Qieyun* is only an approximation of the language then in use. On the scriptural and linguistic culture as reflected in the progress of lexicography, a most convenient reference (it eschews all kinds of annotation) is H. Yong - J. Peng, *Chinese Lexicography. A History from 1046 BC to AD 1911* (Oxford-New York, Oxford University Press, 2008); read, e.g., 15-43, for the origins, and 134-52, for some theoretical issues.

<sup>73</sup> For practical illustrations of this state of affairs and the ways early Japanese writers modified Chinese — the best I can do here due to my plain man's approach —, recourse will be had to J. R. Bentley, *A Descriptive Grammar of Early Old Japanese* (Leiden-Boston, Brill, 2001), 8-10; survey of various styles of *hentai kanbun* at 11-8. Cf. A. E. Gnanadesikan, *The Writing Revolution. Cuneiform to the Internet* (Oxford, Wiley-Blackwell, 2009), 113-115.

<sup>74</sup> I confine to a note another issue which contributes to rob the Sino-Japanese analogy of its value: whereas Chinese can be encoded with relatively straightforward ease thanks to the Mandarin romanization, the transliteration of Japanese, particularly nouns common and proper, is so historically conditioned, riddled with patterns of multiple renderings for many signs, and partial to idiosyncrasies of pronunciation, as to discourage one of looking for 'correct' scriptural representations in the Latin script. See L. Butler, "Language Change and

with elements from the older, more elaborate, continental civilization» at the levels of writing and vocabulary encapsulates the core of the facts correctly, if obviously far too briefly, or was meant from the start to deceive.

Let us now look at the, more familiar, adoption of the West Semitic alphabet by the Greeks and their earliest use of the writing technology. The first scribes, in the early eighth or very late ninth century, never took over the Phoenician language nor used it ‘for purposes of written communication’, to borrow Coulmas’ expression; they created an alphabet out of the Phoenician syllabary under conditions that still remain somewhat opaque, then put the newly minted graphemes to use by writing down their own language<sup>75</sup>, possibly to preserve the Homeric poems owing to their monumentality (so H. T. Wade-Gery, A. Snodgrass, Powell), or, and rather more probably, to assist their memory in their day-to-day dealings (both Ruijgh<sup>76</sup> and Johnston toyed with a purely commercial goal, either accountancy or ownership marking, which draws help from the one-word, incomplete as to its case-

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‘Proper’ Transliterations in Premodern Japanese”, *Japanese Language and Literature* 36, 2002, 27-44, who quips at 36 that ‘for those working in premodern Japanese languages, the challenges never seem to end’. To analogize the putative dynamic of Egyptian and West Semitic borrowings in Greek with the inflation of Chinese in Japanese is thus to explain *obscurum per obscurius*.

<sup>75</sup> Though it remains customary to nod to the classic L. H. Jeffery, *The Local Scripts of Archaic Greece. A Study of the Origin of the Greek Alphabet and its Development from the Eighth to the Fifth Centuries B.C.* (‘Revised Edition with Supplement by A. W. Johnston’, Oxford, Clarendon Press, 1990 [1961]), 1-42, the best expositions are those of B. B. Powell, *Homer and the Origin of the Greek Alphabet* (Cambridge, Cambridge University Press, 1991), 5-67, especially 20-48, and R. D. Woodard, *Greek Writing from Cnossos to Homer. A Linguistic Interpretation of the Origin of the Greek Alphabet and the Continuity of Ancient Greek Literacy* (New York-Oxford, Oxford University Press, 1997), 133-204, particularly 133-46 (the remainder of the chapter strongly assails Powell on the ‘additional letters’, the sibilants, the adapter’s phonetics, and even the place of creation, Cyprus rather than Euboea); C. J. Ruijgh, “La date de la création de l’alphabet grec et celle de l’épopée homérique”, *Bibliotheca Orientalis* 54, 1997, 533-603, is superb too but for the chronology (following note). On a briefer scale, the two antagonistic papers of Ruijgh, *Mnemosyne* 51, 1998, 658-87, and S. R. Slings, *ibid.*, 641-57, make for fruitful reading. As to the names of the letters, all previous scholarship has been bettered, with the very latest data marshaled, by A. Willi, “Cow, Houses, Hooks: The Graeco-Semitic Letter Names as a Chapter in the History of the Alphabet”, *Classical Quarterly* 58, 2008, 401-23 (he supports Woodard on the sibilants, pp. 415-7).

<sup>76</sup> “Sur la date de la création de l’alphabet grec”, *Mnemosyne* 51, 659-61. «Il faut conclure que l’emploi de l’alphabet pour graver des inscriptions sur pierre et sur métal est secondaire: il appartient à une phase ultérieure de l’écriture alphabétique, où la connaissance de l’écriture se répandait de plus en plus et parvenait à d’autres groupes sociaux que ceux de la noblesse <commerçante>» (p. 661; he supposes the Greek alphabet to have appeared very early, around 1000 B.C. in round numbers, cf. “D’Homère aux origines proto-mycéniennes de la tradition épique. Analyse dialectologique du langage homérique, avec un excursus sur la création de l’alphabet grec”, in J. P. Crielaard (ed.), *Homeric Questions...* [Amsterdam, Gieben, 1995], 1-96 at 26-46). On the contrary, one of the key aspects of the problem is the nearly instantaneous spread of literary once the Phoenician alphabet was adapted.

ending, graffito at Osteria dell'Osa, a personal name dated to *ca.* 775, but otherwise is hard to reconcile with the fact that early inscriptions are all versified save this one, see S. Sherratt, "Visible Writing: Questions of Script and Identity in Early Iron Age Greece and Cyprus", *Oxford Journal of Archaeology* 22, 2003, 225–42 at 230–4). Therefore the major difference boils down to the joint issues of typology and diglossia: whereas the Greeks needed not learn West Semitic to use its alphabet, since only the sounds matter in a syllabic (i.e. phonetic) writing system, so they never developed any bilingualism beyond a smattering of loanwords, the Japanese who faced the Chinese logographic system in which the language mostly comes from the writing needed to become bilingual, as we have just seen. If an analogy was to be sought for the linguistic economy of Old Japanese, Akkadian *vis à vis* Sumerian provides it. From the twenty-sixth century B.C. onwards, the Semitic speakers of Old Akkadian had lived alongside the Sumerians in Southern Babylonia; the former took up the cuneiform script devised for the language of the latter even though their own was enormously different<sup>77</sup> and ended up with marked Sumerian influences on their phonology, syntax, and lexicon (but adjectives and verbs remained immune<sup>78</sup>). What is more, speakers of Akkadian never stopped practicing Sumerian, which stayed artificially alive as the vehicle of the higher culture<sup>79</sup>. Thus the semasiographic status of Old Japanese was, to all extent and purposes, that of Early to Late Middle Chinese as mediated through Korea, whereas the Greek vocal alphabet remained from the start radically discrepant from its West Semitic model —

<sup>77</sup> Very little bibliographical guidance is necessary here, unless one wishes to reinvent the wheel; cf. R. Labat, *Manuel d'épigraphie akkadienne* ('Revu et augmenté par F. Malbran-Labat', Paris, Geuthner, 1988<sup>6</sup> [1948]), 1–4, 6–15, and von Soden, *Grundriss der akkadischen Grammatik* ('3., ergänzte Auflage unter Mitarbeit von W. R. Mayer', Rome, Pontifical Institute, 1995 [1952]), 7–11. In some cases, e.g. the seal inscriptions of the Old Akkadian kings, which are short and formulaic and make heavy use of logograms, it even proves difficult to determine whether the language of a particular piece is Sumerian or Akkadian (Kienast and Gelb, *Die altakkadischen Königsinschriften des dritten Jahrtausends v. Chr.* [Stuttgart, Steiner, 1990], 39–48). «Archaic cuneiform was not a tool for writing poetry or narrative. It was an administrative tool with extraordinary flexibility» (N. Veldhuis, in P. Michalowski and idem (edd.), *Approaches to Sumerian Literature. Studies in Honor of Stip (H. L. J. Vanstiphout)* [Leiden-Boston, Brill, 2006], 197).

<sup>78</sup> Loanwords excepted, of course. So it cannot be assumed *a priori* that whatever is not markedly Semitic in Akkadian must be borrowed or derived from Sumerian: Kienast, *Historische semitische Sprachwissenschaft* (Wiesbaden, Harrassowitz, 2001), § 398, pp. 435–6; N. J. C. Kouwenberg, *The Akkadian Verb and its Semitic Background* (Winona Lake, Eisenbrauns, 2010), 159–60, 244.

<sup>79</sup> Thus the most essential part of the scribal education — another commonplace of Assyriology: despite its title, S. Tinney, "On the Curricular Setting of Sumerian Literature", *Iraq* 61, 1999, 159–72, is less useful here than Veldhuis, "Sumerian Proverbs in their Curricular Context", *Journal of the American Oriental Society* 120, 2000, 383–99 at 384–8, or P. D. Gesche, *Schuhunterricht in Babylonien im ersten Jahrtausend v. Chr.* (Münster, Ugarit, 2001), 12–24.



as different indeed from Phoenician as this West Semitic syllabary was with respect to Middle Egyptian and its twenty-four unilaterals, or alphabetic signs<sup>80</sup>. For Ugaritic and Phoenician-Punic have consonant phonemes, whereas Egyptian does not — actually it cannot have them due to the utter indifferentiation between the unilaterals and the bi- or trilaterals which they often reinforce<sup>81</sup>, notwithstanding the claims of some (about the unilaterals, C. T. Hodge, “The Role of Egyptian Within Afroasiatic (/Liskah)”, in Baldi (ed.), *Linguistic Changes and Reconstruction Methodology* [Berlin-New York, Mouton de Gruyter, 1990], 639-59 at 644-7, contends that «from the manner in which they are used, we may assume that each represents a different consonant phoneme. As they are presumed to come from words beginning with these consonants, they represent only those phonemes which occur initially», 644). Last but not least, whereas the West Semitic influences to which the Greeks owed their alphabet were, if not a one-time occurrence, at least a stream which soon dried up never to regain full momentum, there were no less than three successive waves of Sino-Japanese: *go-on*, based on Early Middle Chinese, in the sixth century; *kan-on*, modelled after Late Middle Chinese and adopted by the imperial court of Japan in the eighth century; finally *tō-in*, *tō-on*, or *tō-sō-on*, the product of the spelling of words brought from China’s Wu region by Japanese Zen monks in the thirteenth or fourteenth centuries<sup>82</sup>. For the Chinese acculturation in Japan to operate as a valid analogy for the conjectural Egypto-Canaanite penetration in Greece, you thus need to demonstrate that the latter occurred on the *longue durée*, otherwise both phenomena are far too dissimilar to be compared. Mere

<sup>80</sup> Gardiner, §§ 17-8, pp. 25-6; Hoch, *Middle Egyptian Grammar* (Mississauga, ON, Bemben, 1997), § 4, pp. 5-6; L. Depuydt, *Fundamentals of Egyptian Grammar. Elements* (Norton, Mass., Frog, 1999), 17-25; Allen, *Middle Egyptian. An Introduction to the Language and Culture of Hieroglyphs* (Cambridge, Cambridge University Press, 2000), § 2.1 p. 13; etc.

<sup>81</sup> Powell, *Writing. Theory and History of the Technology of Civilization* (Oxford, Wiley-Blackwell, 2009, reprinted Chichester, *ibid.*, 2012), 163-5, 184-5, conveys the essentials pithily if cryptically, cf. Tropper, *Ugaritische Grammatik*, § 32, pp. 90-135 (not nearly identical to Sivan, *Grammar of the Ugaritic Language*, pp. 20-48), and Krahmalkov, *Phoenician-Punic Grammar*, 19-37. The opposite stance is now heralded by G. J. Hamilton, *The Origins of the West Semitic Alphabet in Egyptian Scripts* (Washington, Catholic Biblical Association of America, 2006), cf. 5-6, 269-75. W. W. Hallo, *Origins. The Ancient Near Eastern Background of Some Modern Western Institutions* (Leiden-New York-Köln, Brill, 1996), 35-41, assists us for the names and order of the West Semitic letters, since Powell underplays them (see p. 165, etc) — from, say, T. Nöldeke, *Beiträge zur semitischen Sprachwissenschaft* (Strasbourg, Trübner, 1904), 124-36, to Hamilton, 21-8 or Rosól, *Frühe semitische Lehnwörter...*, 113-132, the progress is essentially a matter of documentary increase.

<sup>82</sup> See I. Taylor - M. M. Taylor, *Writing and Literacy in Chinese, Korean and Japanese* (Amsterdam, Benjamins, 1995), 300-1; Loveday, *Language Contact in Japan*, 40-1; and Irwin, *Loanwords in Japanese*, 62-3, for the basics unencumbered with technicalities; richer expositions in Miyake, *Old Japanese*, index, s.vv., particularly pp. 100-9, 144-8; Frellesvig, *A History of the Japanese Language*, 275-8; or Heffernan, 62-3, 65, 68-9, 73-81. I reproduced Irwin’s dates for these waves (p. 63).

possibilities do not suffice to make such a claim palatable; unfortunately, there is no evidence whatsoever to bear it out. The conclusion is inescapable: you must be either linguistically ignorant or unscrupulous to assert that the influence of Chinese on Japanese mirrors the impact of Egyptian and West Semitic on Greek. In my mind, Bernal is both.

Yet, far from withdrawing this analogy, *BA III* force-feeds it from the start to the unsuspecting reader: «over the last twenty years, the study of language contacts and mixture has become more fashionable<sup>83</sup>. Approaches have varied but share various features. The most important commonality is the conviction that languages are not autonomous entities but social creations spoken by living populations. Therefore, linguistic contact is a reflection of social contact. A corollary is that, while similarity of language, such as that between English and German, may ease borrowings from one language to another, the social and cultural relations between the two groups of speakers form the determining factor. Thus, for instance, substantial cultural contact over many centuries has led to massive Chinese influence on the Japanese lexicon, even though the two languages are completely unrelated» (p. 14). The reason I see behind this grandstanding of Bernal's lies in the absence of refutation of the Sino-Japanese analogy in *Black Athena Revisited*, but for an incidental remark by Jasanoff-Nussbaum (p. 188)<sup>84</sup>. Now that it has been exposed for the fabrication it is, I hope that no one will blindly swallow such nonsense<sup>85</sup> in the foreseeable future, nor deem Bernal vindicated when

<sup>83</sup> Mostly in the case of contemporary languages, so the sleight of hand is patent. This creed's fundamentals may be sought in A. Y. Aikhenvald, "Mechanisms of Change in Areal Diffusion: New Morphology and Language Contact", *Journal of Linguistics* 39, 2003, 1-29 at 2-4; cf. J. G. Heath, "Language Contact and Language Change", *Annual Review of Anthropology* 13, 1984, 367-84. Heath emphasizes «the complexity of language mixing and the importance of relatively abstract psycholinguistic mechanisms» (p.382) — a fundamental point since nowhere in his entire oeuvre does Bernal analyze the thought processes his Egyptian loans evince. Yet Pliny wrote of Africa that *populorum eius oppidorumque nomina uel maxime sunt ineffabilia praeterquam ipsorum linguis* (5.1.1), cf. J. Desanges ('C.U.F.', Paris, Belles Lettres, 1980), 78-9; C. Briand-Ponsart, "À propos de la mémoire africaine d'Apulée", in idem and S. Crogiez (edd.), *L'Afrique du Nord antique et médiévale* (Rouen, Presses Universitaires, 2002), 59-76 at 65-6.

<sup>84</sup> Of the acknowledged Semitic and Egyptian borrowings in Greek (viz. those in E. Masson's book now largely superseded by Rosól's), they remark that «such words, several of which are already found in Mycenaean, suggest lively Greek-Phoenician commercial relations going back to the second millennium B.C.E. But unlike the Norman French loan words in English or *the Chinese loan words in Japanese*, these do not suggest the kind of prolonged, transformative cultural contacts that the Revised Ancient Model presupposes» (emphasis mine).

<sup>85</sup> I shall stand my ground here. Legitimate irritation rises when one reads such declarations as «in their semantic range, the non-Indo-European elements in Greek resemble the French and Latin words in English, the Arabic in Swahili and the Chinese in Korean, Japanese and Vietnamese. These parallels would tend to go against the explanatory principle of a conquest of non-Indo-European speakers by Proto-Greeks» (p. 167), or «if, on the other hand, the Revised Ancient Model is applied, Greek fits neatly into the larger group including English, Swahili,

he rehearses it to maintain his aberrant phonetics. This happens in *BA III*, 246-7 (*≈ Black Athena Writes Back*, p. 115): «in their critique of my work, Jasanoff and Nussbaum found my proposal that *ntr* was “given five different phonetic treatments in Greek” to be absurd and outrageous. Parallels from varying manifestations of Chinese loans into Japanese or Romance loans into English, however, make the number in itself unexceptionable. (...) See the character for ‘lark’ or ‘pipit’ pronounced *liu* in modern Chinese. It has eight different *on* (Chinese) readings in Japanese; *ryū*, *ru*, *bō*, *hyū*, *mu*, *kyu*, *gu*, and *ryō*. Thus, unlike Jasanoff and Nussbaum, I have no difficulty in believing that the Egyptian *ntr*, which is more complicated phonetically than the prototype of *liu*, could have had “five distinct phonetic treatments”<sup>86</sup>. The moral is obvious. As an academic outsider, you must take steps to keep in check the intellectual habits of your former speciality lest they mislead you into familiar yet not necessarily warranted territory. The Sinologist Bernal was unconcerned about having a one-track mind even in the midst of his Panafrican inquiries, or, rather, he did not suspect that he had one; as a consequence, *Black Athena* wrings subtleties out of the languages and civilizations it is supposed to compare only by gross insensitivity to their characteristics, treating them as it would the East Asian ones<sup>87</sup>.

In his memoir, Bernal states: «I continue to put my faith in schizophrenia — that younger scholars in these disciplines will simultaneously hold the two models, Aryan and Revised Ancient, and that sooner or later the better one will emerge» (*Geography of a Life*, 447). This implies that both paradigms

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Vietnamese, Japanese, Old Javanese and many others» (p. 168). It is bad method enough to lump together under the same linguistic entity (note the fuzzy use of the term ‘group’ by Bernal) some dead languages and modern tongues; in a book concerned with historical linguistics, this process is positively abhorrent.

<sup>86</sup> A Japanese character is susceptible of having several, in some cases: multitudinous, Sino-Japanese readings (they are authoritatively compiled in the Morohashi dictionary), but this does not vindicate Bernal in the least. Such a phenomenon is normal in the case of a *logographic* writing system, and a complex one at that (compare the polysemy of Sumerian words used as logograms in Akkadian); it does not apply to Egyptian, which, though not (really) *logosyllabic*, is *logophonetic*, and makes no sense whatsoever for the firmly *syllabic* Greek. Egyptian loanwords in Greek are thus perfectly incapable of the diversity of phonetic treatment seen in Sino-Japanese.

<sup>87</sup> To this insensitivity he adds a tactical sense of the battles he can wage and those he had best avoid — a sure proof of his cynical approach to the evidence. As he prefers to focus on divine names, toponyms, or flashy words, Bernal eschews the true enigmas of the Greek lexicon (ἀγαθός, ἄλλος, ἄλω(ι)ή, ἀμνιον, ζωρός, λᾶς, λαῖμα, πέτρα, ταναός, ὑάκινθος...); nor does he etymologize the highly emblematic Νεῖλος, though the Egyptian *n3 r3w-h3w(t)*, ‘the river mouth(s)’ (Goedicke, “Νεῖλος: An Etymology”, *American Journal of Philology* 100, 1979, 69-72 at 71-2), and an intriguing Berber etymon (*Aristarchus antibarbarus*, 35 note 80), were at hand (a further problem being the alternate name for the Nile, Τρίτων, which Herimippos of Smyrna, fr. 90 Bollansée, tells us is the original one: commentary in *Die Fragmente der griechischen Historiker Continued*, IV A.3 [Leiden-Boston-Köln, Brill, 1999], 589-90).

stand on equally strong foundations. Yet, far from proving that the gist of the preceding tomes was correct, pending the day when some of Bernal's main conclusions would enter the mainstream, his linguistic case, completed at last, demonstrates that the whole series comes from an amateur scholar of the ancient worlds who was not well-placed to lecture Indo-Europeanists, Hellenists, and historians about their supposed contempt for Africa. (If contempt there is, it must be put at Bernal's door, for always discarding the well-known partiality of the Greek language to loanwords and semantic calques from Mesopotamia<sup>88</sup> or Anatolia). Contacts between Egypt and parts of the Hellenic mainland — Crete was rather more advanced in the Late Bronze Age due to its exposition to Syro-Phoenicia, Cyprus too because of its copper<sup>89</sup> — indeed happened as early as Mycenaean times, but they seldom, if ever, were so intensive as to provoke the kind of cultural transfers Bernal needs for his model to work. The ceramologist Van Wijngaarden even cautions that, since, «at most sites, the Mycenaean pottery appears to derive from multiple sources in the Aegean (...), it therefore remains questionable

<sup>88</sup> Much work remains to be done. Since the sixties, nobody doubted the Akkadian origin of  $\mu\upsilon\tilde{\alpha}$  (W. Burkert, *Die Orientalisierende Epoche in der griechischen Religion und Literatur* [Heidelberg, Winter, 1984], 39, *The Orientalizing Revolution. Near Eastern Influence on Greek Culture in the Early Archaic Age* [Cambridge, Mass-London, Harvard University Press, 1992], 37; West, *The East Face of Helicon. West Asiatic Elements in Greek Poetry and Myth* [Oxford, Clarendon Press, 1997], 24 note 99; Beekes, II, 960), yet no one before B. Lourié saw that its sixtieth multiple, Akk. *biltu*, *kakkaru*, talent (CAD B [1965], 231; CAD K [1971], 49-50; *AHw.*, I, 126, 422; cognates in Del Olmo Lete-Sanmartín, I, 435 *kkv*), likely existed before leaving the field to  $\tau\acute{\alpha}\lambda\alpha\nu\tau\omicron\nu$ , viz. \* $\acute{\kappa}\acute{\epsilon}\rho\omicron\varsigma$  ("Between Babylonia and Ethiopia: Some Thoughts about a Recent Book on the Qumranic Calendars", *Scrinium* 6, 2010, 413-32 at 419-23, with M. A. Powell, "Maße und Gewichte", *Reallexikon der Assyriologie und Vorderasiatischen Archäologie*, VII [Berlin-New York, De Gruyter, 1987-1990], 457-517 at 508, 510, or J. P. Brown, "Proverb-Book, Gold-Economy, Alphabet", *Journal of Biblical Literature* 100, 1981, 169-91 at 183-4  $\approx$  *Israel and Hellas* [Berlin-New York, De Gruyter, 1995], 290-308 at 307-8). The explanation of the Homeric  $\epsilon\upsilon\tilde{\rho}\acute{\epsilon}\alpha$   $\pi\acute{\omicron}\nu\tau\omicron\nu$  by the Akkadian *tāmtu rapaštu* (West, 221; cf. *uznu rapaštu*, 'broad understanding') is yet more brilliant, since the epithet conveying the vastness of the sea looks like a fixed one in the Homeric epics and in Akkadian poetry — *tāmtu rapaštu mala tarbaši* CAD T [2006], 220: 'the wide sea is as (small as) a fold';  $\epsilon\upsilon\tilde{\rho}\acute{\upsilon}\varsigma$  is of sound PIE pedigree, *pace* Beekes, I, 484 (Wodtko-Irslinger-Schneider, 251). On loanwords in general, Francis, "Impact...", 483-93 and now Rosól, *Frühe Semitische Lehnwörter...* (where Akkadian could have played a still larger part: see 288-290).

<sup>89</sup> Suffice it to mention N. C. Stampolidis - A. Kotsenas, "Phoenicians in Crete", *apud* Deger-Jalkotzy and Lemos, *Ancient Greece*, 337-60 at 340-9, who put flesh on J. Whitley, *The Archaeology of Ancient Greece* (Cambridge, Cambridge University Press, 2001), 102-24, notably 120-1, and A. B. Knapp, *Prehistoric and Protohistoric Cyprus. Identity, Insularity, and Connectivity* (Oxford-New York, Oxford University Press, 2008), 307-41, mostly 311-3, 317-8, 325-7; on the types and distribution of artefacts in the Mediterranean, G. J. van Wijngaarden, *Use and Appreciation of Mycenaean Pottery in the Levant, Cyprus and Italy (ca. 1600-1200 BC)* (Amsterdam, University Press, 2002), 13-22, is foundational. Bernal sometimes pays heed to such geography of the archaic trade, but only to misconstrue it as a map of linguistic penetration, a step which ought not to be taken lightly and is certainly supported by nothing cited in *BA III* (pp. 122, 236, 283 sqq. [Crete], 526-7 [Cyprus]).

whether exclusive connections between overseas places and particular regions in Mycenaean Greece were common practice» (p. 13). These contacts<sup>90</sup> never left the lexicographical traces in the Greek language that Bernal has obsessed about for decades. As there are more cogent ways than his of dealing with the proportion of words destitute of a reasonably sound Indo-European descent, his experiment is moot. The amount of such unexplained lexemes complicates the task of present-day etymologists of Greek despite their cutting-edge models of Proto-Indo-European which leave Pokorny far behind. Some of the most senior among those experts accordingly theorize a Pre-Greek stratum of non-PIE stamp. This attempt to refine on the ‘Asianic’ or ‘Mediterranean’ substrates (e.g., Baldi, *The Foundations of Latin* [Berlin-New York, Mouton de Gruyter, 1999, 2002<sup>2</sup>], 100, 100-1 note 2) one may reasonably be leery of, as this theory does not appear to have progressed that much since E. J. Furnée adumbrated it after an hecatomb of the extant views up to the middle of the sixties (*Die wichtigsten konsonantischen Erscheinungen des Vorgriechischen. Mit einem Appendix über den Vokalismus* [The Hague, Mouton, 1972], 29-98)<sup>91</sup>. Even today, considerable sweeps of the phonetic system of this Aegean Pre-Greek keep eluding us, including the number of vowels (were they three or five?), so probably the least adventurous conclusion would be that it should serve mainly as a starting point in piecemeal reconstructions, leaving aside, for the time being, the broader aspects (this minimalism informs D. Testen, “Iranian and Anatolian Cognates to Greek (*k*)*sún*”, *Journal of the American Oriental Society* 131, 2011, 287-93 at 288 top). Anyway, no matter the stand one takes with respect to Pre-Greek, the proportion of the Greek lexicon we cannot elucidate through Proto-Indo-European and borrowings, *Wanderwörter*, or *Lehnwörter*, is not quite troublesome enough to convince classicists to push the clock back to preparadigmatic times and behave à la Bochart under the pretence of catering to the new, global agenda. Though it found no acceptance

<sup>90</sup> On the scriptural side: S. Nimis, “Egypt in Greco-Roman History and Fiction”, *Alif* 24, 2004, 34-67 at 34-8; A. B. Lloyd, “The Reception of Pharaonic Egypt in Classical Antiquity”, in idem (ed.), *A Companion to Ancient Egypt* (Malden, Blackwell, 2010), II, 1067-85 at 1068-78; I. Moyer, *Egypt and the Limits of Hellenism* (Cambridge, Cambridge University Press, 2011), 53-8. On the material side, e.g.: A. Möller, *Naukratis. Trade in Archaic Greece* (Oxford-New York, Oxford University Press, 2000), 32-8, 182-215; A. M. Greaves, *The Land of Ionia. Society and Economy in the Archaic Period* (Chichester, Wiley-Blackwell, 2010), 166-8, 188-90, 194-7, 205-6; C. Mee, *Greek Archaeology. A Thematic Approach* (*ibid.*, 2011), index, p. 322.

<sup>91</sup> Its heart (pp. 101-331, plus the three appendices) functions as a dictionary, with data so raw and detached from their context (temporal and geographical distribution is disregarded) as to be open to any manipulation; it hardly throws light on what it purports to map out, viz. the consonantal alternations. «Wir wollen das nicht fortsetzen; zweifellos wird mit ähnlichen Einwänden ein Teil von F.’s Material eliminiert oder bestritten werden» (G. Neumann, *Gnomon* 46, 1974, 433-7 at 437; so too Beekes, I, XIV-XV).



and slipped out of memory so completely that, for once, Bernal incurs no blame for ignoring it, the attempt by W. Merlingen to explain much of the unetymologized lexicon of Greek by borrowings from a new Indo-European language, which he termed *Psigriechisch*<sup>92</sup>, would suffice, anyway, to show that counsels of despair are uncalled for.

Bernal's demonstration can in no way soar higher in persuasiveness than these foundations. Unable of rising up to the standards he was dared by his critics to observe, he identifies them with scholarly blinkers, so we have to put up with a shameless double standard: since the tree model is dead — long live the nongenetic contacts and areal shifts! —, mainstream linguists are ordered to prove everything they claim that concerns Indo-European and are vilified whenever they do not, whereas Bernal merely points the way to the possibility that any given snippet of Egyptian entered Greek. For the cross-linguistic elucidation of lexicographical cruces an alert and nimble mind is necessary, but so too is sobriety of reasoning. Bernal dresses up his fancy<sup>93</sup> as the cutting edge of sociolinguistics, counting for proof on the sheer mass of his etymologies.

Most of them are merely ludicrous; they appear to be marginal jottings in Faulkner clumsily amplified regardless of all standards of logic and common sense. Ἀπόλλων < *hprrr* comes at the end of an amazing chain of fake deductions: «Apollo as the youthful divinity of the sun» resembles Horus<sup>94</sup>;

<sup>92</sup> Fair assessment in J. Chadwick, *Journal of Hellenic Studies* 85, 1965, 186-7; D. M. Jones, *ibid.* 89, 1969, 157-8; and, on a specific point, in Bader, *La langue des dieux, ou l'hermétisme des poètes indo-européens* (Pisa, Giardini, 1989), 266.

<sup>93</sup> Any comparison between his idea of PIE and what can be found in the *LIV*<sup>2</sup>, Wodtko-Irslinger-Schneider, or the *Encyclopedia of Indo-European Culture*, will show the unsoundness of his linguistics. On the theoretical side, Bernal's model of social archaeology is hopelessly crude and should not be allowed to swing the pendulum back to notions long superseded, witness M. Shanks, *Classical Archaeology of Greece. Experiences of the Discipline* (London-New York, Routledge, 1997), 86-91, cf. 89 sqq., or J. Baines, in *Black Athena Revisited*, 38. The very same verdict applies to his positions in ethnography and cultural anthropology, as they were already belated in the early nineties (S. Krech III, "The State of Ethnohistory", *Annual Review of Anthropology* 20, 1991, 345-75 at 359-63); his grasp of religious history (the most conspicuous case-in-point being his treatment of the Hermetica: Baines, 44-5; A. Bach, "Whitewashing Athena: Gaining Perspective on Bernal and the Bible", *Journal for the Study of the Old Testament* 77, 1993, 3-19 at 8-9; S. Marchand - A. Grafton, "Martin Bernal and His Critics", *Aion* 5, 1997, 1-35 at 3-6); or his command of historiography (Marchand-Grafton, 6-7, 11-3, 20-4). 'Fancy' and 'fanciful' are thus relatively tame epithets for Bernal.

<sup>94</sup> This false idea (note 96) even crops up in J. Gwyn Griffiths, *Plutarch's De Iside et Osiride. Edited with Introduction, Translation, and Commentary* (Cardiff, University of Wales Press, 1970), 301: «his identification with Apollo begins with Herodotus (2.144) and may well have arisen from the solar attributes held in common by the two gods». If Fontenrose's identification of Python with an earlier Typh(a)on is accepted — the objections being mostly linguistic: D. Ogden, *Drakōn. Dragon Myth and Serpent Cult in the Greek and Roman Worlds* (Oxford, Oxford University Press, 2013), 151, cf. 152-3 —, it would appear that Apollo as the slayer of this beast came to be identified with Horus the opponent of Seth («Typhon is a

the Egyptians linked him with the sun; there are titles ‘Horus of the sun / horizon’ that equate to *hprrr*; as Ἀπόλλων and *hprrr* have both «a double liquid», they balance one another, but for the troublesome change /h/ > /a/ <sup>95</sup>; just suppose, then, a Phoenician intermediate for this borrowing, which must have been late enough for the merging of the glottal and pharyngeal fricatives to be in full swing (*BA III*, 462)<sup>96</sup>. Who was this providential Canaanite deity that equates to the Egyptian Kheper (for we possess fairly precise notions of how *hprrr* must have sounded<sup>97</sup>)? Of the same ilk is ἄτομος < *itm*, Atum<sup>98</sup>. It compels one to postulate that those Presocratics who use

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very well-known name for Seth. (...) Even the word itself is borrowed from a foreign language, so accentuating the foreign nature of its bearer» H. te Velde, *Seth, God of Confusion. A Study of his Role in Egyptian Mythology and Religion* [‘Reprint with some corrections’, Leiden, Brill, 1977 (1967)], 149).

<sup>95</sup> As witnessed by the transcriptions of Hebraic names in the Septuagint and of Demotic names in Greek, the Semitic /h/ became χ, while /ħ/ was turned into a vowel (Steiner, ‘Dating...’, 233-4): e.g. ἠῆ > Χαρράν vs. ῥῆ > Ἰσαάκ. So the Semitic distinction between the two fricatives was felt by some speakers of Greek until at least the second or the first century B.C. Bernal’s «the problem is that /h/ was very seldom transcribed into Greek as /θ/» makes the whole point opaque (even if one flips back to p. 738 bottom).

<sup>96</sup> The hypothesis stands and falls on the confusion of Apollo with Helios, an idea utterly alien to the early Greek literature (P. Curtis, *Stesichoros’s Geryoneis* [Leiden, Brill, 2011], 97-101); the sun is a distinct god and an autonomous poetic / religious concept, cf., say, K. Tsantsanoglou, *Of Golden Manes and Silvery Faces. The Partheneion 1 of Alcman* (Berlin-Boston, De Gruyter, 2012), 39-41. The spread of the solar cult of Apollo can hardly predate the sixth-century Homeric Hymn to Hermes (E. F. Cook, *The Odyssey in Athens. Myths of Cultural Origins* [Ithaca, Cornell University Press, 1995], 82-3), and a renewed importance of Apollo, *more Ægyptiaco?*, in Ptolemaic times, has some appeal as a conjecture (Noegel, “Apolonius’ Argonautika and Egyptian Solar Mythology”, *Classical World* 97, 2004, 123-36 at 129-31). Consult: F. Buffière, *Les mythes d’Homère et la pensée grecque* (Paris, Belles Lettres, 1956), 187-200; P. Boyancé, “L’Apollon solaire”, in *Mélanges...Carcopino* (Paris, Hachette, 1966), 149-70, who insists on the part played by the Pythagoreans (157 sqq., cf. Buffière, 189); and M. A. van der Sluij, “Who are the ‘Attendants of Helios?’”, *Journal of the American Oriental Society* 129, 2009, 169-77 at 171 note 15.

<sup>97</sup> From the verb *hpr* and comparing, e.g., *H3r*, Khor / Khar (Syria). From ‘scarab, beetle’, *hprrr* (Hannig, I, 940, II.2, 1870-1) came to mean Kheper / -pri, ‘Scarab god (qua symbol of the sun)’, rejuvenated form of the sun-god at dawn: J. Assmann, in *Lexikon der Ägyptologie*, I, coll. 934-940; Allen, *Genesis in Egypt. The Philosophy of Ancient Egyptian Creation Accounts* (New Haven, Yale Egyptological Seminar, 1988), 10-1; O. Keel, *Corpus der Stempel-siegel-Amulette aus Palästina / Israel. Katalog* (Freiburg / Göttingen, Universitätsverlag / Vandenhöck & Ruprecht, 1997), I, 779-81; M. Minas-Nerpel, *Der Gott Chepri. Untersuchungen zu Schriftzeugnissen und ikonographischen Quellen vom Alten Reich bis in griechisch-römische Zeit* (Leuven, Peeters, 2006), 463-77, cf. 148-9, 366, 376, 382, for his title ‘Lord of the sky’. Bernal should have told us how Kheper / -pri could sound like Ἀπόλλ-.

<sup>98</sup> Endorsed in *Black Athena Writes Back*, 390-1, as part of Bernal’s attack on Lefkowitz, *Not Out of Africa. How Afrocentrism Became an Excuse to Teach Myth as History* (New York, Basic, 1996; 2nd ed., 1997), this idea of G. G. M. James’ (a writer on whose fantasies see Slack, 234-85, 332-74) is lost sight of in *BA III*. The mere suggestion that ἄτομος should be set aside from τάμνειν (Attic τέμνειν; τάμνω < \*τάμνημι, Ruijgh, “*Dama / duma δάμαρ / δύμαρ* et l’abréviation *DA*, notamment en PY En 609,1”, in P. H. Ilievski - L. Crepajac (edd.),

ἄτομος (cf. W. Kranz, *Wortindex*, in Diels and Kranz, *Die Fragmente der Vorsokratiker*, III [‘12., unveränderte Auflage’, Dublin-Zürich, Weidmann, 1967], 82) knew about the theology of Atum in a more or less pure form or else would have been unable to take his Hellenized name as a byword for what is materially primary. A gratuitous theory, this ignores the fact that Middle and New Kingdom sources are uncomfortable with what represents Atum’s main claim to the quality of a cosmogonic god, viz. the creation of the primary divine couple Shu and Tefnut through his self-pleasuring, to such an extent that they have them born instead from the mouth or the heart of Ptah; which puts the final touch to the effacement of Atum that began as early as the jibes of Coffin Texts, I 75 §§ 354c-356 (contra, e.g., II 77 § 18, II 80 § 31; at an earlier stage preference was given to a creation by the heart and spoken word of the god: H. Willems, *The Coffin of Heqata* [Leuven, Peeters, 1996], 304-8, after L. Kákosy, “A Memphite Triad”, *Journal of Egyptian Archaeology* 66, 1980, 48-53 at 52-3). Neither Democritus’ Egyptian trip nor the claim that «the cult of Atum, though often fused with that of Re, retained its independence in the Late and Ptolemaic periods» (*Black Athena Writes Back*, 391) suffice to overcome those strictures<sup>99</sup>. Other Graeco-Egyptian propositions of Bernal’s exhibit a less gratuitous semantic match, yet remain very much on the wild side, such as σοφία, σοφός < sbꜣ<sup>100</sup>, ‘to teach, educate, make wiser’, as a substantive ‘pupil’ (pp. 262-3), or φιλία, φίλος<sup>101</sup> < mri,

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*Tractata Mycenaea...* [Skopje, Macedonian Academy of Sciences and Arts, 1987], 299-322 at 311), a verb of stout PIE pedigree (\*temh<sub>1</sub>-, ‘to cut’: LIV<sup>2</sup>, 625; De Vaan, s.v. *temnō*, -ere, 609-10; Beekes, II, 1466), to drag in Atum, is indeed bogus. Nor does *itm/n* signify “fullness” or ‘being’ and ‘non-being’” (*BA III*, 391): a form of *tm*, ‘to complete, finish’ (Hannig, I, 1430-1; II.2, 2693-4) or ‘to cease to be, not be’ (Hannig, I, 1431; II.2, 2694), it means ‘whole, finished (in both creative and destructive capacities)’ (Allen, *Genesis*, 9; cf. Assmann, *Lexikon der Ägyptologie*, I, coll. 526-43).

<sup>99</sup> K. M. Cooney, “Androgynous Bronze Figurines in Storage at the Los Angeles County Museum of Art”, in S. H. D’Auria (ed.), *Servant of Mut. Studies in Honor of Richard A. Fazzini* (Leiden-Boston, Brill, 2008), 63-72 at 65-8, reminds us that the images of Atum are rooted in the Egyptian notion that divine forms are ever changing — a far cry from the monad.

<sup>100</sup> Hannig, I, 1096-7, II.2, 2157-8; Lesko-Switalski Lesko<sup>2</sup>, II, 26-7. Read further R. J. Williams, “Scribal Training in Ancient Egypt”, *Journal of the American Oriental Society* 92, 1972, 214-21, particularly 215-6.

<sup>101</sup> In typical fashion, he cannot explain the verb φιλεῖν and suppresses the width of meaning of φίλος in early and classical poetry. Progress since H. Fränkel (*Dichtung und Philosophie des frühen Griechentum* [München, Beck, 1962], 92 = *Early Greek Poetry and Philosophy* [Oxford, Blackwell, 1975], 83) has been immaterial: the standard studies take the attributive value as primary (M. Landfester, *Das griechische Nomen ‘Philos’ und seine Ableitungen* [Hildesheim, Olms, 1966], 5-92, e.g. 72 «φίλος, Freund’ ist ein emphatisch gebrauchtes substantiviertes Possessivpronomen»; Beekes, II, 1574) before guest-friendship pushed the word towards the affective end of the scale, or have (ritualized) friendship for the earliest sense (H. T. Kakridis, *La notion de l’amitié et de l’hospitalité chez Homère* [Thessaloniki, s.ed., 1963], 5-10, 86-108; Benveniste, *Vocabulaire*, I, 335-53; D. B. Robinson, “Homeric φίλος: Love of Life and Limbs, Friendship with One’s θυμός”, in E. M. Craik (ed.), *Owls to Athens. Essays...*

‘to love, like, appreciate’, ‘to want, wish, desire’<sup>102</sup> (p. 206), cf. Lefkowitz, *History Lesson. A Race Odyssey* (New Haven–London, Yale University Press, 2008), 182–3, on the necessity of rejecting these interferences — not least because of the unlikelihood of an absence of native Greek bywords for such fundamental notions.

Finally, several etymologies showcase Bernal’s manipulations at their worst. He has not retracted his impossible Θῆβαι < *dbzt* / *dbt* despite the easily increased strictures of Jasanoff–Nussbaum (see *Aristarchus antibarbarus*, 196–7) and keeps the pot boiling by putting forward a fresh suggestion in which extravagance coupled with ignorance reaches new heights, namely σῖτος < *swt* (*ibid.*, pp. 200–5). Nonetheless, the fittest sample remains his emblematic treatment of Athena, for nowhere else in his four tomes are the defects of his methods; the limits of his learning; his lack of phonetic and philological rigor; and his absence of scruple more on display. He has successively derived Ἀθηνᾶ from *H{w}t nt* (*BA I*, 51–2) and *Ht-ntr {nt} Nt* (*BA III*, 581), ‘temple of Neith’, the religious name of the city of Sais. Apart from the subtle differences between *hwt* and *pr* which were kept out of the debate; the fact that *hwt-ntr* designates «the cult-centre (*hwt*) of a god as opposed to the cult-centre of a deified king» (P. Spencer, *The Egyptian Temple. A Lexicographical Study* [London, Kegan, 1984], 42–55 at 55, cf. Hannig, I, 784–5, II.2, 1632–5 — 1634 sqq. for the diverse gods involved); and a fundamental mistake in the revamped etymon which I cannot allow to remain unexposed<sup>103</sup>, nobody could ever swallow this etymology but

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for sir Kenneth Dover [Oxford, Clarendon Press, 1990], 97–108]; cf. too Konstan, *Friendship in the Classical World* (Cambridge, Cambridge University Press, 1997), 28–31. J. T. Hooker, “Homeric φίλος”, *Glotta* 65, 1987, 44–65, declares himself in favor of an original affective value (63 sqq.), then writes that «we shall make an approximation to the truth if we recognize that φίλος (...) underwent a change in meaning during the course of the epic tradition, with the result that the word was available to the bards both in its original and in its developed sense. Only some such explanation as this will account for the observed fact that the meaning of φίλος in Homer ranges from a strongly-marked affectionate use, through a strongly-marked possessive use, to a weak possessive use» (p. 64). This is best.

<sup>102</sup> Cf. Jones, *Index*, 436–46 n<sup>o</sup>1607 to 1670. Its range increased with time: Lesko-Swital-ski Lesko<sup>2</sup>, I, 192 (‘to love, to desire, to want, to cherish, to prefer, to covet’); F. Junge, *Late Egyptian Grammar. An Introduction* (Oxford, Griffith Institute, 2005), 335 (‘to love, cherish, adore, covet, demand someone or something; to wish or want something; to wish, want, desire something for one’s self (with preposition *n* and reflexive pronouns); desire, choose’). Now compare *mrw.t* in the Old and Middle Kingdoms (Mathieu, *La poésie amoureuse de l’Égypte ancienne. Recherches sur un genre littéraire au Nouvel Empire* [Cairo, IFAO, 1996], §§ 54–5, pp. 170–1): «(...) la *mrw.t*, à l’Ancien et au Moyen Empire, obéissait à des règles de circulation très précises: c’est invariablement le supérieur qui ‘aime’ l’inférieur, c’est à dire qui provoque un sentiment d’amour, la relation réciproque s’exprimant par le verbe *dw3*, adorer. Il faut attendre le Nouvel Empire pour rencontrer des exemples d’inversion du processus. (...) Le verbe *mrj* se rapproche dès lors de notre verbe *aimer*» (§ 55). Bernal should have drawn a semantic line between the New Kingdom and the earlier periods.

<sup>103</sup> Despite *BA III*, 581 top, *h(w)t-ntr n/Nt* does not mean ‘house/temple of Neith’, but merely

Rendsburg (“An Etymological Response”, 72-3). «If *hwt nt* is the source of *Athēnai*, one final *-t* remained while the other was lost. I would explain this case by postulating the borrowing during a period in the history of the Egyptian language when final *-t* was lost in absolute forms (*nt*) but was still retained in construct forms (*hwt*) (Gardiner 1957.34 n.1a)» (p. 73), that is, any time after 2000 B.C. Not only is the chronological range thus produced very broad; this idea of Rendsburg’s has been exploded<sup>104</sup> and he uses swagger rather than caution<sup>105</sup>. His semantic support is no firmer<sup>106</sup>: the

‘house/temple of’ (plus a theonym). The only correct spellings for ‘temple of Neith’ are *h(w)t-Nt* and *h(w)t-nṯr nt Nt*, the former very frequent in the Saite material and current elsewhere (R. El-Sayed, *Documents relatifs à Saïs et ses divinités* [Cairo, IFAO, 1975], index, 296; also Dendara II at 6,10, 120,5, 149,8, etc), the latter scarcely attested. Indeed Hannig does not record one *h(w)t-nṯr nt Nt* in Old and Middle Egyptian in the face of the multiple instances of *h(w)t-nṯr nt R<sup>c</sup>* (Dendara I, 90,6; Nitocris Stela, 24), *h(w)t-nṯr nt Imn* (Urkunden IV 186,3 1258,6), *h(w)t-nṯr nt Wsir* (P. Sekowski, col. X, title), and *h(w)t-nṯr nt H(w)thr* (Dendara V, 107,6), as a glance at, say, F. Gomaà, *Die Besiedlung Ägyptens während des Mittleren Reiches, I Oberägypten und das Fayūm* (Wiesbaden, Reichert, 1986), index, 436, makes it plain.

<sup>104</sup> Egberts, “Consonants in Collision: Neith and Athena Reconsidered”, reprinted in *Black Athena Comes of Age*, 149-63 at 155-9. Working on a statistical basis, he collected all known cases of the Coptic and Greek transcriptions of feminine composites in *ht*; in 11 cases, *t* left no trace in the adapted forms, so must have been elided in the Egyptian pronunciation before it came to be adapted; in the three remaining ones, Greek and Coptic retained it (*Ht-hry-ib* > ατρηπε, αορνβι / αορεβι > Ἄθηριβις; *Ht-hr* > ελαοαρ, αοαρ, ελαοαλ > Ἄθῆρ; *Nbt-ht* > νεβωα > Νέφθως). Egberts thus asserted that the dental in such composite words was dropped unless a laryngeal spirant follows, which forbids the *Ht* in *Ht-nt* from sounding /at/ after 2000 B.C.: since the *n* in *nt* is a nasal dental, *Ht* in *Ht-nt* was merely /a/ to the ears of the Greeks, and they had no reason whatever to turn it into the ἄθ- of Ἀθηνᾶ. Bernal negates this carefully-crafted scheme; after an amount of special pleading, he has the gall to reduce *Ht-hry-ib*, *Ht-hr*, and *Nbt-ht* to the first-cited item, then to call this case-in-point merely a ‘complication’ (*BA III*, 580), instead of the lethal objection it embodies. For a breezy rejoinder, cf. *Black Athena Comes of Age*, 165-71 at 169 sqq.: instead of meeting this careful phonology, Bernal accuses Egberts of being an old-style Neo-Grammarians (168, 170: a familiar tune!). As we call it in French, *c’est noyer le poisson*.

<sup>105</sup> The quality of his article perpetuates his master Gordon’s standards (note 121), which can be lax (notes 144, 149-50, 184, 185). How will he explain the Masoretic text פְּנִיָּהּ פְּנִיָּהּ at Genesis 41:45 in the vicinity of נְּטָרָה (< *ns-nl.t*, ‘she belongs to Neith’: Ἀσεννέθ) and פְּרַעַן פְּרַעַן (< \**p3-di-p3-r<sup>c</sup>*, ‘he who was given by Ra’: Πετεφρη, Potiphar)? The sense is likely to be ‘the god has decreed: he will live’, but whether it should be attained by \**dd-p3-nṯr-iw.f<sup>c</sup>nh* (lastly Loprieno in J. Tait (ed.), *Never Had the Like Occurred. Egypt’s View of its Past* [London, UCL, 2003], 154) or by *dd-iv.f<sup>c</sup>nh* (J. Lanckau, *Der Herr der Träume. Eine Studie zur Funktion des Traumes in der Josefsgeschichte der Hebräischen Bibel* [Zürich, Kösel, 2006], 272) is unsettled. Unfortunately, the pattern of this name might be doubted (Schulman, “On the Egyptian Name of Joseph: A New Approach”, *Studien zur Altägyptischen Kultur* 2, 1975, 235-43 at 239-41) and Ψονθομφανήχ of the LXX points to \**p3-sndm-ib<sup>c</sup>n3*, ‘der das Herz mit Leben angenehm macht’.

<sup>106</sup> Egberts, 154 «since there is at least one example in Greek mythology of a personage who owes his name to a town in the Nile delta, viz. king Busiris of Egypt, Bernal’s explanation (...) need not be impeded by semantic considerations» vs. Jasanoff-Nussbaum, 194 «‘temple of Neith’ is no more likely, a priori, than ‘olive grove’, ‘rocky crag’, or countless other possible



putative  $h\{w\}t nt > \text{Ἀθηνᾶ}$  would mirror the deities Hathor ( $H\{w\}t-hr$ ) =  $hwt hr$ , ‘house (temple) of Horus’ (e.g. Hannig, I, 1600, II.2, 3106-10), and Bethel =  $bêl \text{ ʿ}l$ ,  $\text{בֵּית-אֱל}$ , ‘house of god / of El’, parallels to which it will be objected that they are merely unilingual<sup>107</sup>. Furthermore, what of the epigraphic hesitation Bethel ( $\text{בתאל}$ ) versus Baytel ( $\text{ביתאל}$ ) and of the underlying theology<sup>108</sup>? Even Van Binsbergen was sharply critical (“Dans le troisième millénaire avec *Black Athena*”, in Fauvelle-Aymar, 127-50 at 144; *Black Athena Comes of Age*, 53-5). Rightly so: it must be said that  $H\{w\}t nt$  is phonetically possible but breaks down on its lack of explanation for Athena’s middle  $\eta/\bar{\alpha}$  (Jasanoff-Nussbaum, 194 top, 204 note 16; despite *BA III*, 694 note 273, Egberts missed this point), whilst the evolution  $Ht-ntr nt Nt > \text{Ἀθαναία} > \text{Ἀθηναίη}$  Bernal fashioned to evade this stricture collapses under its own weight. The ‘earliest form’ (*BA III*, 582)  $\text{Ἀθαναία}$  actually appears in dialects which contributed nothing to the Homeric language (*Aristarchus antibarbarus*, 8, adding W. Blümel, *Die aiolischen Dialekte* [Göttingen, Vandenhöck & Ruprecht, 1982], § 27, p. 35, and L. Dubois, *Inscriptions grecques dialectales de Sicile* [Rome, Ecole Française, 1989], n<sup>o</sup>51.2, 78.4-5, 141, pp. 60, 75, 162); as such, the epichoric  $\text{Ἀθαναία}$  is well nigh unlikely to have produced  $\text{Ἀθηναίη}$  (which Bernal seizes since the epicizing Herodotus uses it when he speaks of Sais and Neith: II 28, cf. the commentary of Lloyd [Leiden-New York-Köln, Brill, 1994<sup>2</sup>], 111) in the *Iliad* and the *Odyssey*.  $Ht-ntr nt Nt$  equally and foremost depends on  $\text{Ἀθηναίη}$  being an archaism preserved in the *Kunstsprache*: this, although possible, is far from obvious (*Aristarchus antibarbarus*, 8-9), therefore cries for demonstration instead of assertion («Homer, whose language was sometimes more archaic than that written in Linear B, uses  $\text{Ἀθηναίη}$ », but it rather appears to be a secondary

glosses». Egberts jumps to conclusions; Busiris is comparatively recent, unlike *Atana*, and I deem it unsound to adduce such a peripheral, unimportant figure to support the notion that the name of a panhellenic goddess of Athena’s importance could have come from an Egyptian city.

<sup>107</sup> Viz. they do not cross any linguistic and cultural boundaries. See the respective entries in K. van der Toorn - B. Becking - P. W. van der Horst (edd.), *Dictionary of Deities and Demons in the Bible* (‘Second Edition, Extensively Revised’, Leiden-Boston-Köln / Grand Rapids-Cambridge, Brill / Eerdmans, 1997), 385-6 and 173-5. On Hathor, R. A. Orehov, “Earthly Hathor and Heavenly Hathor”, in *Egyptology at the Dawn of the Twenty-First Century...* (Cairo-Chichester, American University in Cairo-Wiley, 2003), II, 423-7 (424 for her name) is handy; on Bethel, Van der Toorn, “Anat-Yahu, Some Other Deities, and the Jews of Elephantine”, *Numen* 39, 1992, 80-101 at 83-5 (not a Phoenician god, but a late Aramaean one worshipped in Northern Syria) packs a punch.

<sup>108</sup> Bethel / Baytel is an evolution of the more archaic El (G. Athas, *The Tel Dan Inscription. A Reappraisal and a New Interpretation* [Sheffield, Academic Press, 2003], 210-1, 309-10) with an intermediate hypostasis El-Baytel,  $\text{אֱל-בֵּיתאֱל}$ , ‘the Bethel-stone, El’ (314-5, with Beekes, I, 193, for the obviously borrowed  $\beta\alpha\iota\tau\lambda\omicron\varsigma$ ); such betyls, *maṣṣēbôt* (cf. E. C. LaRocca-Pitts, ‘Of Wood and Stone’. *The Significance of Israelite Cultic Items in the Bible and its Early Interpreters* [Winona Lake, Eisenbrauns, 2001], 205-28; etc), would seem to have stood at city gates to witness legal proceedings qua symbols of El (Athas, 310 sqq.).

derivative of Ἀθήνη, as suggested by its absence in Mycenaean: F. Graf, in J. Latacz (ed.), *Homers Ilias. Gesamtkommentar, Prolegomena* [‘3., durchgesehene Auflage’, Berlin-New York, De Gruyter, 2009], 119). A third hindrance weakens Bernal’s case: he behaves as if the long form Ἀθηναίη were a variant of the short one, but neither this nor an adjectival ‘of Athena, Athenaic’ makes much sense, even in the sporadic tag Παλλὰς Ἀθηναίη (unless rendered ‘Athenian maid’, which is not warranted by the Greek). So N. Wachter reminds us of the old notion that it must be the name of the city<sup>109</sup> (*Non-Attic Greek Vase Inscriptions* [Oxford-New York, Oxford University Press, 2001], 263 § 247) — a difficult solution in several respects<sup>110</sup>, it explains in a pellucid way lines like *Iliad* 6.300, τὴν γὰρ Τρῶες ἔθηκον Ἀθηναίης ἰέρειαν, «she whom the Trojans had established to be priestess of the Athenian (goddess)»<sup>111</sup>.

All that remains of the several Egyptian etymologies of Ἀθηνᾶ ~ Ἀθηναίη which epitomize *Black Athena* are the similarities between Athena and Neith, adding J. Quaegebeur, W. Clarysse, and B. Van Maele, “Athēna, Nēith and Thoēris in Greek Documents”, *Zeitschrift für Papyrologie und Epigraphik* 60, 1985, 217-32 at 218-22, plus M. Malaise, *Pour une terminologie et une analyse des cultes isiaques* (Brussels, Académie Royale de Belgique, 2005),

<sup>109</sup> Cf. Latacz - R. Nünlist - M. Stoevesandt, *Homers Ilias. Gesamtkommentar*, I.2 (‘3., durchgesehene Auflage’, Berlin-New York, De Gruyter, 2009), 91 at I (199-)200 ἀτύκᾳ δ’ ἔγνω | Παλλὰς Ἀθηναίην, and Burkert, *Griechische Religion der archaischen und klassischen Epoche* (‘Zweite, überarbeitete und erweiterte Auflage’, Stuttgart, Kohlhammer, 2011), 217, for this view.

<sup>110</sup> So soon as the seventh century, Ἀθαναία was already felt to be the name of Athena in an inscription on the Judgement of Paris (= Wachter, 31, PCO 2; between 670 and 630), cf., a century later, Ἀθηναίη on a scene illustrating the myth of Geryon (M. Robertson, “Geryoneis: Stesichorus and the Vase-Painters”, *Classical Quarterly* 19, 1969, 207-21 at 208-9). If only we could tell which, of the goddess or the city, came first... For Wachter’s arguments are quite weak: «it seems natural that the Athenians should claim the goddess as theirs. And it is most interesting that other Ionians, including those who must have emigrated to the East around 1000 BC, behaved as if they were Athenians in this respect. As for the Dorian (and Rhodian?) Ἀθαναία, we should perhaps consider it a Pre-Doric form (like Ποιοιδᾶνι (...))» (this looks as arbitrary as Pre-Greek: Beekes, I, 29). Finally, there is the spurious yet intriguing divide Ἀθηναίαι versus ἄσταί, ἀττικάί (Slater, *Aristophanis Byzantii Fragmenta* [Berlin-New York, De Gruyter, 1986], fr. 3, pp. 6-8).

<sup>111</sup> This verse so much escaped, or bored, exegesis that not one commentary, whether for schoolboys or seasoned scholars, in the past two centuries, included a note on it (I checked Bothe, Dugas-Montbel, Dübner, Faesi, Doederlein, Ameis-Hentze(-Cauer), La Roche, Pierron, Leaf<sup>1-2</sup>, Monro, Van Leeuwen, and Kirk; Heyne *maior* has an irrelevant one in his fourth volume). So only Stoevesandt, *Homers Ilias. Gesamtkommentar*, VI.2 (Berlin-New York, De Gruyter, 2008), 104, explains Ἀθηναίης ἰέρειαν, but far too briefly and in keeping with the translation in the fascicle of text: ‘als Priesterin Athenes: zum Athene-Kult in Troia s. 86-101n.’, viz. p. 38. I cite the version by G. Nagy, *Homer the Preclassic* (Berkeley-Los Angeles-London, University of California Press, 2010), 269, in the wake of his discovery of a ‘split reference’ here: the poet has in his mind Troy and the historical Athens at the same time (p. 270).

121-2. One cardinal parallel, the symmetry between Athena's owl and Neith's vulture, has been overlaid by Bernal (on 214, 554, 564, 578), judging from the respective importance of her beasts in El-Sayed, *La déesse Neith de Saïs* (Cairo, IFAO, 1982), II *Documentation*. The chapter devoted to her theriomorphic depictions in the first tome *Importance et rayonnement de son culte* tells the same tale: Neith appears mainly as a cow, qua one of the personifications of the celestial mother; then as the crocodile of Sobek, her son (on this filiation, read El-Sayed, I, 101-6); thirdly as the *lates niloticus*, the Nile perch (see of late R.-A. Jean - A.-M. Loyrette, *La mère, l'enfant et le lait en Égypte ancienne* [Paris, L'Harmattan, 2010], 117-9); her vulture persona is at best scarce<sup>112</sup>. A better scholar would have cited the posthumous suggestion of P. E. Newberry that there was a Owl-City in Pre-Dynastic times to be identified with Saïs ("The Owls in Ancient Egypt", *Journal of Egyptian Archaeology* 37, 1951, 72-4 at 73-4; those insights were never bourne out). On the whole, the connection of Athena with Neith remains what it is for Egyptologists and most scholars of Greek religion: a popularizing equation meant to smooth out the dealings of Egyptian natives and Greek traders or tourists in the seventh and sixth centuries. This view is not shaken by Solon's Egyptian trip according to both Plato (an indirect, unreliable witness: G. Griffiths, "Atlantis and Egypt", *Historia* 34, 1985, 3-28; *infra*, note 158) and Herodotus (whose testimony looks rather shaky: M. Noussia-Fantuzzi, *Solon the Athenian, the Poetic Fragments* [Leiden-Boston, Brill, 2010], 297-300); it receives some measure of confirmation from the Hellenistic astrological evidence related to the traditional Egyptian group of the decans (J. F. Quack, "Les Mages Égyptianisés? Remarks on Some Surprising Points in Supposedly Magusean Texts", *Journal of Near Eastern Studies* 65, 2006, 267-82 at 277)<sup>113</sup>. The connection of Neith with Athena is accordingly best left unmentioned (as in the standard compendia of Greek religion — by Wilamowitz, Nilsson, Burkert — and in the Neith entries in both H. Bonnet, *Reallexikon der ägyptischen Religionsgeschichte* ['3., unveränderte Auflage', Berlin, de Gruyter, 1999 (1952)], 512-7, and D. B. Redford (ed.), *The Oxford Encyclopedia of Ancient Egypt* [Oxford,

<sup>112</sup> Neith's animal iconography is closely linked to her family and triad — a vast topic: H. Sternberg, *Mythische Motive und Mythenbildung in den ägyptischen Tempeln und Papyri der griechisch-römischen Zeit* (Wiesbaden, Harrassowitz, 1985), 36-109 *passim*; E. Hornung, *Der ägyptische Mythos von der Himmelskuh. Eine Ätiologie des Unvollkommenen* (Freiburg / Göttingen, Universitätsverlag / Vandenhöck & Ruprecht, 1991), 96-101; D. Klotz, *Adoration of the Ram. Five Hymns to Amun-Re from Hibis Temple* (New Haven, Yale Egyptological Seminar, 2006), 41-2; Jean-Loyrette, 119-20.

<sup>113</sup> P. W. Haider, "Der Neith-Mythos und seine Historisierung bei Herodot", in C. Ulf and Rollinger (ed.), *Geschlechter - Frauen - Fremde Ethnien. In antiker Ethnographie, Theorie und Realität* (Innsbruck-Vienna-München-Bozen, Studien, 2002), 58-78, draws a vivid picture of the goddess in Late Egypt as Herodotus must have come to know about her; it ill-accords with Bernal.

2001], II, 516) or explained differently («Ähnlichkeiten der Wesensmerkmale und der Funktionen führen in der Sp(ät) Z(ei)t zu einer Verschmelzung mit der griech. Göttin Athene» contends R. Schlichting in the *Lexikon der Ägyptologie*, IV [Wiesbaden, Harrassowitz, 1982], coll. 392-4 at 392). As regards a borrowing of *Ht-ntr nt Nt* into Ἀθηνα(ῖνη), the obvious conclusion is that it can safely be ruled out<sup>114</sup> as fundamentally misguided from an historical point of view; for someone has to pay the price of working out a purely formal design, compare J. P. Fokkelman, *Narrative Art and Poetry in the Books of Samuel, I King David (II Sam 9-20 & I Kings 1-2)* (Assen, Van Gorcum, 1981), 422-7. — To etymologize the name of a god is a risky venture even when enough evidence survives to sketch his profile (L. Feliu, *The God Dagan in Bronze Age Syria* [Leiden-Boston, Brill, 2003], 278-9); Bernal's analyses of Athena are so unaware of the pitfalls and exacting standards of the game that they dub as a worthy symbol of the arbitrariness of the method behind his series. By reckoning on his principles, howbeit with a display of confidence and skin-deep scholarship well calculated to bluff the unwary, viz. the 'lay jury' to which he has chosen to defer (Slack, 6-8), anything could indeed be derived from anything else<sup>115</sup>.

<sup>114</sup> Better no etymology than this one (*pace* p. 582: «neither the lexicographers nor my critics — Egberts, Jasanoff and Nussbaum — have proposed any alternative, let alone a more convincing etymology. Until someone does that, the derivation of Ἀθηναῖη from *Ht-ntr (nt) Nt* remains plausible and should be allowed to stand»). Ἀθηναῖη is emphatically no gibberish, no matter what Bernal has been contending for decades (in Van Sertima (ed.), *African Presence in Early Europe* [New Brunswick-Oxford, Transaction, 1985], 79), cf. U. von Wilamowitz-Möllendorff, *Der Glaube der Hellenen*, I<sup>2</sup> (Darmstadt, Wissenschaftliche Buchgesellschaft, 1955), 229 «Athena hat einen Namen, der sich aus dem Griechischen nicht erklären läßt, auch wenn das Ableitungssuffix, griechisch klingt», or Szemerényi, "The Origins of the Greek Lexicon: Ex Oriente Lux", *Journal of Hellenic Studies* 94, 1974, 144-57 at 154-5.

<sup>115</sup> Formula: take an unexplained puzzle in the Greek lexicon, preferably an early, absolute hapax on which not much scholarly literature exists — Hipponax, fr. 148 Degani ἄβδης: μάστιξ παρ' Ἰππώνακτι = Hesychius α 97, fits the bill, being either borrowed or Pre-Greek (Beekes, I, 4) — and try looking for a Semitic word, or, better, a bunch of words, showcasing a comparable / similar pattern of consonants; their semantics must somehow be made to tally with (one of) the meaning(s) of your *unicum*. Ἄβδης, 'scourge' ('nom du fouet' Chantraine, rather than merely 'whip' Bernal, 419, under λάμβδα), might be predicated of pests, which were a scourge in classical Athens (M. Davies - J. Kathirithamby, *Greek Insects* [London, Duckworth, 1986], 13); now Akkadian has a lone hapax *dabdabum* qua the gloss of Sumerian *ur-me* to denote a crawling insect. So Å. W. Sjöberg, "UET 7 n° 93. A Lexical Text or a Commentary?", *Zeitschrift für Assyriologie* 86, 1996, 220-37 at 228 obverse 39, citing an impressive array of Arabic comparanda: *dabba*, 'to walk slowly', 'to creep' (of insects, ants, scorpions, or beasts); *dabdab*, 'the walk of a long-legged ant'; *dabbāb*, 'crawling, quadruped'; *dabīb*, 'creeping, reptile'; and *dabbāt*, 'reptile', plus a Amorite proper name *de-eb-de-bu-um*. The meaning of the Semitic √*DBB*, 'to walk slowly', is beyond doubt (Smith-Pitard, 422), and Arabic lexicography corroborates this evidence (Wehr, 375 = Wehr-Colwan, 269-70; S. Tlili, "The Meaning of the Qur'anic Word 'dābba': 'Animals' or 'Nonhuman Animals'?", *Journal of Qur'anic Studies* 12, 2010, 167-87 at 167-73, with the caveat that this participle is all-encompassing, from animals, its most usual gloss, to human beings and even angels [Tlili,

It will not be denied now that the work was worth doing and has been done badly<sup>116</sup>. Why could the project only be expected to go badly astray? Partly because the Chinese scholar Bernal was in no way the right man for a task that would have required, to be competently and impartially discharged, a team of specialists in the various Near Eastern languages concerned working under the supervision of a senior linguist at home in the entire civilization and history of Greece (someone like F. R. Adrados), partly due to the very danger of misusing bits and pieces of all those languages in an uncritical attitude towards the rules of evidence, to which such an imaginative inquest as the exploration of the West Semitic influences over Greece was inevitably exposed every step of the way. In a nutshell, goals too lofty were aimed at, in conformity with the highly inflated opinion Bernal has of his own capacities; the result is that a modest but serviceable achievement has been

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173 sqq.]). Since Bernal likes irrelevant matters, a Sumerian onomatopoeia for the clatter of rocks, dub-dub...sa, can be thrown in (J. Black, "Sumerian Noises: Ideophones in Context", in W. Sallaberger - K. Volk - A. Zgoll (edd.), *Literatur, Politik und Recht in Mesopotamien. Festschrift für Claus Wilcke* [Wiesbaden, Harrassowitz, 2003], 35-52 at 40-1). Being Afrocentric-minded, you easily accept the Afroasiatic  $\sqrt{*daba}$  postulated by Orel-Stolbova partly from the Arabic (p.140 n°606), swallowing it or, on the contrary, leaving it to others with a firmer linguistic grasp to pronounce on the rightness of their gloss 'insect'. Thus the semantism of the Semitic cognates 'beautifully' fits and the descent  $\sqrt{*daba}$  > *DBB* looks strong; you are now ready to boast that ἄβδης must be the result of a loan veiled by curtailing and the loss of its prothetic consonant. — As one can see, there is no major difficulty in concocting Semitic etymologies that demonstrate nothing but the cunning and loose associative logic that constitute Bernal's stock-in-trade. The only superiority of my parody over his purportedly serious propositions is that mine contains no factual error (*pace BA III, loc. cit.*, Chantraine does not 'believe' that 'the earliest Greek form is not λάμβδα but λάβδα', for such is the truth: E. Schwyzler, *Griechische Grammatik* [München, Beck, 1939], I, 140 and note 2; G. P. Shipp, *Modern Greek Evidence for the Ancient Greek Vocabulary* [Sydney, University Press, 1979], 352; Powell, *Homer...*, 35 note 98; Brown, *Israel and Hellas*, I, 43-4; Willi, "Cow, Houses, Cooks", 402, 403 and note 8; also, but tangentially, H. D. Jocelyn, "A Greek Indecency and its Students: ΑΙΚΑΖΕΙΝ", *Proceedings of the Cambridge Philological Society* 26, 1980, 12-66 at 31-4, 42-4).

<sup>116</sup> One needs not be microscopically minded to acknowledge that, as research consists of the setting up of hypotheses to which evidential rigor was applied, any book whose main theses strain credibility and which is demonstrated to twist the data systematically through ignorance or prejudice (or both), can only be condemned, and its author held accountable for bad method and inadequate scholarship. «Base your assertions on evidence, not on bias or wishful thinking», wrote R. L. Ponder apropos of *BA II*; «in reexamining the past, we must not cast aside the real for the likely, the known for the hoped-for» (*American Historical Review* 97, 1992, 464). In the case of *BA III*, so little remains once all that is erroneous, arbitrary, and simply irrelevant has been discarded that one suspects this bulk to rank among the *raisons d'être* of the book: Bernal wanted to establish his credentials by appearing enormously learned; he needed to canvass massive amounts of detail if his hope to rally Afrocentric readers and cower students into acquiescence was to pass muster. Neither here nor *infra* (note 142) am I preaching a counsel of perfection unattainable for one mere mortal; I am merely advocating modesty and accuracy in the pursuing of inquiries which exact much more from the researcher than the average work of scholarship.



shunned in favor of an absurd cesspool. Instead of dubbing as an accurate appendix to our obsolete etymological lexica, as Van Windekens did, *BA III* carts back the so-called garbage of contemporary scholarship, staking out huge claims for a grid that takes over literally the results of the *interpretatio Graeca* of Hecataeus, Herodotus, and Plato<sup>117</sup> (not Plutarch; he went his own way<sup>118</sup>). This anxious adherence to the ethnography and pseudo-linguistics of antiquity where better historians are forced to discard them (Slack, 116–25), stems from a lack of familiarity with the period — this might not be very conspicuous at first glance, between the seemingly professional writing, the austere typography replete with linguistic shorthands or IPA symbols, and

<sup>117</sup> Which Bernal persists in not acknowledging as such, viz. the fashioning, most usually entailing deformation through explanation, of a ‘foreign’ fact or structure in order for it to make sense for fellow Greeks (Lloyd, “Egypt”, in E. J. Bakker – I. J. F. de Jong – H. van Wees (edd.), *Brill’s Companion to Herodotus* [Leiden-Boston, Brill, 2002], 415–35 at 430–2; J. D. Mikalson, *Herodotus and Religion in the Persian Wars* [Chapel Hill, University of North Carolina Press, 2003], 155–65; E. S. Gruen, *Rethinking the Other in Antiquity* [Princeton, Princeton University Press, 2011], 82–3; etc), no more than Bernal is aware that the fondness for Egypt of the early classical Greeks only makes sense within the Hellenic-Barbarian divide (Herodotus 2.158.5–159.1 βαρβάρους δὲ πάντας οἱ Αἰγύπτιοι καλέουσι τοὺς μὴ σφίσι ὁμογλώσσους as analysed by A. A. Lund, “Hellenentum und Hellenizität: Zur Ethnogenese und zur Ethnizität der antiken Hellenen”, *Historia* 54, 2005, 1–17 at 15–6; «die zivilisierten Ägypter bilden mit anderen Worten insofern eine Analogie zu den zivilisierten Hellenen, als sie wie diese auch alle Fremden per definitionem für Barbaren hielten» [p. 16]). One feels accordingly much distress at finding that Bernal neglects to cite the passages of Herodotus where Egyptian kings honor Athena in their own country (e.g. 2.175.1 καὶ τοῦτο [sc. Ἄμασις] μὲν ἐν Σαῖ τῇ Ἀθηναίῃ προπύλαια θωυμάσια οἱ ἐξεποίησε), all of which prove beyond any doubt that the historian is transferring into Greek terms the particulars of the Egyptian cult (in fact Bernal fails to quote Herodotus *anywhere* in his section about Athena, but for a snippet from Frazer [p. 575], whose ancient authorities he endorses without having checked them; the reader is thus directed towards *Enquiry*, II 59 where Athena does not appear — a blemish repeated by Van Binsbergen, ὁ ἀνήρ κομποφακελορήμων, in his opening chapter of *Black Athena Comes of Age*, 20 note 24).

<sup>118</sup> The writer best acquainted with Egyptian of this lot (as evidenced by G. Griffiths, *Plutarch’s De Iside et Osiride*, 101–10; read also H.–J. Thissen, “Plutarch und die ägyptische Sprache”, *Zeitschrift für Papyrologie und Epigraphik* 168, 2009, 97–106 at 106: «es dürfte aber aus den vorangehenden Erläuterungen klar geworden sein, daß Plutarch entweder direkt durch Quellenstudium oder indirekt durch Gewährsmänner sehr gute Kenntnisse (vom Wesen) der ägyptischen Sprache gehabt hat»), he writes, at *De Iside et Osiride*, 2, 351 F, Ἑλληνικὸν γὰρ ἡ Ἴσις ἐστὶ. This snippet is best taken as the vindication of the Greekness of the name of Isis (cf. Griffiths, 258: «since Isis is here praised for her wisdom, the Greek explanation of her name probably is to derive it from such forms as ἴσμεν or εἴσομαι, from οἶδα, ‘I know’») for reasons detailed by D. S. Richter, “Plutarch on Isis and Osiris: Text, Cult, and Cultural Appropriation”, *Transactions of the American Philological Association* 131, 2001, 191–216 at 195–9. In a nutshell, «the cultic activities of the Egyptians lack meaning in the absence of a Greek interpretive frame» (p. 200); Plutarch is hellenizing the Egyptian myth in a conscious effort to make it true and submit it to Greek philosophy. Confirmation of the rightness of this view in E. Finkelpearl, “Egyptian Religion in *Met.* 11 and Plutarch’s *Dio*: Culture, Philosophy, and the Ineffable”, *apud* W. Keulen – U. Egelhaaf-Gaiser (edd.), *Aspects of Apuleius’s Golden Ass. Volume III The Isis Book* (Leiden, Brill, 2012), 183–201 at 185–6.

the discouraging perspective of flipping back and forth hundreds of pages to consult the endnotes, but there is far less to the learning of *BA III* than what strikes the eye. One must pin on Bernal an ignorance of the *status quaestionis* that makes one dubious of the relevance of his book; a low proportion of primary texts actually read as opposed to consulted third-hand or seen in some old translation; and the skimpiness on display everywhere. Worse, the work rings painfully hollow in its tone and restless polemics against mainstream lexicography. Bernal is not merely guilty of considerable sloth: he does not behave as the gadfly that stings the sacred cows of stolid conformity and lazy acquiescence. Such a diligent insect goes straight to the most sensitive fleshy parts, whereas *BA III* attempts, none too hard, to make mileage out of feints and flanking movements. He seldom works critically at the brick-and-mortar level; since he rejects so-called rigidities (whether phonetic or graphemic) in his attempts to have Greek and Afroasiatic lexemes cohere, he is thus free to cut the Gordian knot by which his betters were embarrassed. As this amounts to a rhetorical rather than a linguistic treatment, his book is imbued with a false sense of discovery that contrasts with its achievement. So much for the flanking movements. The feints are glaring too. We are asked to believe that not everything goes and that ‘limits’ have been imposed on what he terms his flexibility (p. 584), namely his incapacity to ‘find an Afroasiatic origin’ for ἄνθρωπος and Διόνυσος. The intractable substantive needs not be a loan or a substrate (so Beekes, I, 106) though, since a PIE analysis exists (\*-h<sub>2</sub>ndʰ(e)ro-h<sub>3</sub>k’-o, from \*√-h<sub>3</sub>k’-o- :Wodtko-Irslinger-Schneider, 372, 381 note 72); the theonym remains utterly baffling, but Semitic perspectives may not be wholly bleak given the Canaanite B<sup>c</sup>LBK = BA<sup>c</sup>LABAKKU = Βάαλ Βάκχος (Steiner, “Rise...”, 516-8). Bernal’s inability to explain ἄνθρωπος and Διόνυσος through Afroasiatic registers as a stinging defeat, not as limits, let alone self-imposed ones.

Nor does he play quite fair when he argues against works he mistakes for cutting-edge scholarship that they are guilty of perpetuating the ‘Aryan model’ (smear tactics: Slack, 8-9) or of being too conservative in their assessment of the genetic links between Proto-Indo-European and Afroasiatic. Straw men are Bernal’s favorite device: Pokorny is reviled as forever trying to expand the range of common Indo-European; Frisk, Chantraine, for their readiness either to seize on Indo-European cognates or to admit defeat; Vycichl, for his misplaced striving after precision in the elucidation of Coptic or Egyptian lexemes<sup>119</sup>; the few lexicographers of Greek posterior to Chantraine Bernal

<sup>119</sup> Word-formation, phonetic structures, and Wortphilologie concern Takács less. The entries of his lexicon too often do not progress beyond tedious lists of etymologies he deems absurd; what is more, numerous finer points of his phonological doctrine have met with resistance (J. Osing, *Bibliotheca Orientalis* 58, 2001, 565-81; Quack, *Orientalistische Literaturzeitung* 97, 2000, 161-85). For countless important words, theonyms included, it simply appears futile to look for an etymology either Egyptian or Afroasiatic, unless one passes off

mentions, for being ‘Aryanists’. It follows that, after the *Dictionnaire étymologique complémentaire* and the *Etymological Dictionary of Greek*, for all their unorthodox dodges, have discharged the duty of stirring up the etymological consensus — the former by integrating both dialects of Tocharian in the reconstruction of the PIE lexicon while infusing new life in many conventional glosses, the latter out of its decisions to favor a Pre-Greek approach and cut down to the barest minimal bibliography, philology, and updating —, the holy cows of Indo-European can rest peacefully with the Bernal-shaped bug nearby<sup>120</sup>. He is guaranteed not to trouble much their slumber, for he clearly attempted a task far beyond his powers<sup>121</sup>. Whether

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the championing of oddities for standard scholarly protocol: as a case-in-point, let me mention the demolition of J. Zeidler, “Zur Etymologie des Gottesnamens Osiris”, *Studien zur Altägyptischen Kultur* 28, 2000, 309-16 (by no means undistinguished; his Forschungsbericht, 309-312, and his scientific apparatus will save trouble to future workers) by R. Shalomi-Hen, *The Writing of Gods. The Evolution of Divine Classifiers in the Old Kingdom* (Wiesbaden, Harrassowitz, 2006), 101-2.

<sup>120</sup> Particularly whenever he dwells heavily on coincidences which specialists in the fields he invades incline to consider superficial or of no great general moment. So the Greek identifications of Neith with Athena (Quaegebeur, “De l’origine égyptienne du griffon Némésis”, in F. Jouan (ed.), *Visages du destin dans les mythologies. Mélanges Jacqueline Duchemin* [Paris, Belles Lettres, 1983], 41-54 at 49-51 *passim*) and Horus with Apollo (G. Griffiths, *The Conflict of Horus and Seth from Egyptian and Classical Sources* [Liverpool, University Press, 1960], 93-5; E. Bernand, *Les inscriptions grecques de Philae* [Paris, CNRS, 1969], II, 170-3; S. Stephens, *Seing Double. Intercultural Poetics in Ptolemaic Alexandria* [Berkeley, University of California Press, 2003], 20-1, 53 — contrast P. Bing, *The Well-Read Muse. Present and Past in Callimachus and the Hellenistic Poets* [Göttingen, Vandenhöck & Ruprecht, 1988], 132-43). I find Apollo’s Anatolian roots, viz. his identification with the tanzalizing Luwian god *Ap(p)aliunaš* (after Beekes, “The Origin of Apollo”, *Journal of Ancient Near Eastern Religions* 3, 2003, 1-21 at 12-7, 19-20; E. L. Brown, “In Search of Anatolian Apollo”, *Hesperia Supplements* 33, 2004, 243-57, who understands *Appaliunaš* to be ‘the hunter’, cf. at 248, 250, and 254; Beekes, “Palatalized Consonants in Pre-Greek”, in A. Lubotsky - J. Schaecken - J. Wiedenhof (edd.), *Evidence and Counter-Evidence. Essays in Honour of Frederik Kortland* [Amsterdam-New York, Rodopi, 2008], I, 45-56 at 50-1; and Beekes, I, 118-9), firmer and more cogent than the Semitic ones (Phoenician for Rosól, “Die Herkunft des Gottesnamens Apollon”, *Glotta* 83, 2007, 222-42), if indeed, as Beekes 2003 pleaded, there are grounds for disregarding the Dorian connection through ἀπέλλα, ἀπελλάζειν (Burkert, “Apellai und Apollo” [1975], *Kleine Schriften*, VI [Göttingen, Vandenhöck & Ruprecht, 2011], 3-20; E. Lévy, “La grande Rhètra”, *Ktéma* 2, 1977, 86-103 at 95-6; F. Graf, *Apollo* [Oxford, Routledge, 2009], 109-11). Pace A. D. Papanikolaou, “Ein Versuch zur Etymologie des Namens Ἀπόλλων”, *Glotta* 64, 1986, 184-92, all other Greek roots look moot.

<sup>121</sup> A reviewer of Astour (see following note) pictures the ideal interpreter: «in language and onomastics as in art it is too easy to pull a parallel of some sort out of the Near Eastern hat and our tests must be rigorous. (...) We require a Semitist with a sense of history, a deep understanding of the Greek evidence, and no axe to grind» (J. Boardman, *Classical Review* 16, 1966, 86-8 at 87, 88). Here I shall paint with the same brush Bernal and Rendsburg, who has written haughtily about the refusal of Hellenists to adopt his master Gordon’s ‘decipherment’ of Linear A (“Someone Will Succeed in Deciphering Minoan’: Cyrus H. Gordon and Minoan Linear A”, *Biblical Archaeologist* 59, 1996, 36-43 at 40-2): neither of them comes close to Boardman’s

he second-guessed his claims of improving on Pokorny, Frisk, Chantraine, whom he takes to task for behaving like hapless bunglers whose achievements were limited by the constraints of method and intuition, one cannot tell. Yet his usual reluctance to do more than mention a Semitic or Egyptian word, or cluster of words, in front of a putative Greek isogloss, and leave the reader to pass judgment on the semantic and consonantal matches thus adumbrated, amounts to a step away from Bernal's own daring which I take to underscore a serious lack of confidence in his scholarship. In fact, whenever he contrives to explain himself, we usually see him sending smoke screens to the reader; more accurately, it would appear that Bernal indulges in the sport of slaying the bugbears of his own imaginings in manners strikingly reminiscent of Astour's. His predecessor<sup>122</sup> «makes no allowance for coincidence» (T. T. Duke, *Classical Journal* 61, 1965, 135) and «take[s] the work of men like Beloch and Ed. Meyer as representing the current consensus of classical scholarship. (...) Mr Astour has deliberately set up a straw man in order to be able to knock it down with ease» (J. D. Muhly, *Journal of the American Oriental Society* 85, 1965, 586). Today as in the sixties, it remains true that «until some rigid ground-rules can be worked out for playing the game, all efforts by the mythographical-etymological school will remain what they have always been: personal creations which reveal nothing so much as the author's own predilections and idiosyncrasies», to quote from Muhly once more. His view is scarcely upset by the passing of time; as we know far about more cuneiform, Egyptian, and Semitic than our forefathers did, so have raised our expectations, and one of the main reasons why the Near Eastern games of *BA III* fail to make strides proportional to our information is precisely that Bernal underestimated the growing sophistication of the field. This skewed notion of progress culminates in his confusing the least plausible kind of cross-cultural discourse with a quantum leap in the salvaging of the 'Ancient model'<sup>123</sup> he

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requirements. The latter scholar in particular is bound to irritate the follower of Burkert and West, for whom only the facts unencumbered with theory matter: Rendsburg *in contraria currit* and asseverates that «an equally important factor is the training that Gordon's disciples received. Gordon's unique view of the ancient world, with sightlines recognizing interconnections over large swaths of both time and place, was transmitted to his students in the classroom. Thus, when they became scholars in their own right, they were in a unique position both to understand Gordon's approach and to accept his conclusions» (p. 40: 'unique view of the ancient world' stands for 'unproven fancies').

<sup>122</sup> M. C. Astour, *Hellenosemitica. An Ethnic and Cultural Study in West Semitic Impact on Mycenaean Greece* (Leiden, Brill, 1965, 1967<sup>2</sup>). Even the impassive Burkert shows his irritation: 'weitgehend Astour', 'am weitesten ging A.' (*Die Orientalisierende Epoche*, 11 note 22, 42 note 37), 'going too far was A.', 'A. (1965) went furthest in this sport' (*The Orientalizing Revolution*, 157 note 22, 177 note 37). Understandably so, cf., e.g., Francis, 'Impact...', 486; Astour is all too frequently unreliable and wild (note 185).

<sup>123</sup> 'Preentious farrago' for Lloyd-Jones ("Interesting Times", *International Journal of the Classical Tradition* 4, 1998, 580-613 at 596 note 56; *The Further Academic Papers of Sir Hugh Lloyd-Jones* [Oxford-New York, Oxford University Press, 2005], 284-315 at 317

bestows upon Herodotus — notwithstanding the enduring debate about the modalities of the anthropological grid used by that historian<sup>124</sup>. By digging up Egyptian antecedents for countless Greek words, intellectual structures, and institutions, *Black Athena* harks back to the least believable parts of the works of Foucart and Bérard<sup>125</sup> in defiance of the achievements of Gordon or Hoch in West Semitic; for to tell how much you admire a scholar is not tantamount to emulating him. As K. A. Kitchen once wrote, «no useful purpose is served by refuting old works long made obsolete by more recent discoveries, or by merely setting one opinion against another without reference to the underlying facts»<sup>126</sup>. Comparative scholarship has little business resembling a

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leaves out much of the note). Bernal's character assassination of the great Hellenist (*Geography of a Life*, 435-6) reminisces about petty incidents that would not be worth evoking even if Bernal had told them right (which he did not, according to Prof. Lefkowitz).

<sup>124</sup> Moyer, *Egypt and the Limits of Hellenism*, 42-83 at 43-51, marshalls the essentials. For his aptitudes as an historian, L. Scott, *Historical Commentary on Herodotus Book 6* (Leiden-Boston, Brill, 2005), 1-37, is fine; on his non-Greek sources and the way he envisioned himself as wiser than them, see Luraghi, "The Importance of Being λόγιος", *Classical World* 102, 2009, 439-56.

<sup>125</sup> Viz. the essential identity of Demeter and Kore with Isis and Osiris discovered by P. Foucart, whence the Egyptian origin of the Eleusinia (*Recherches sur l'origine et la nature des mystères d'Éléusis* [Paris, Imprimerie Nationale, 1895], 13-29; *Les mystères d'Éléusis* [Paris, Picard, 1914; reprinted New York, Arno, 1975, Puisseaux, Pardès, 1992, 1999], 1-113 at 47-89), and the 'doublets gréco-sémitiques' witnessing a Egypto-Phoenician condominium (sic) over the Mediterranean in the second millenium (preposterous, cf., e.g., Albright, "Some Oriental Glosses on the Homeric Problem", *American Journal of Archaeology* 54, 1950, 162-76 at 174-5), by which V. Bérard thought he could reconstruct a Phoenician περίπλους adapted into the hexameters of adventure tales that were to become our *Odyssey*: *Les Phéniciens et l'Odyssee* (Paris, Colin, 1902-1903), 2 vol. in-4°, revised under the same title in 1927, 2 vol. small in-8°, with *Les navigations d'Ulysse*, *ibid.*, 1927-1929, 4 vol. (and see *Tables odysseennes* [*ibid.*, 1932], 27 sqq.). The first edition repays dipping for several enigmatic words like φοῖνιξ or Σειρήν, but is damaged by its very spirit of philological levity: cf. G. Bonfante, "The Name of the Phoenicians", *Classical Philology* 36, 1941, 1-20 at 3-4, before *DELG*, p. 1219 (J. Tailardat), 1439-1440 (*Supplément*), Beekes, II, 1584-5; G. K. Gresseth, "The Homeric Sirens", *Transactions of the American Philological Association* 101, 1970, 203-18 at 204 note 5. A collation may be helpful; here it is: I, pp. 125, 126-7, 128, 131, 192-4, 206-11, 213-5, 218-21, 224-5, 232, 229-30, 255-6, 266-7, 285-8, 291, 299-300, 302, 323-4, 325, 330-2, 337-8, 344-5, 346, 349, 350, 354, 355, 357-8, 364, 365, 375-6, 378, 379-80, 389, 403-4, 405, 406, 407, 411-4, 420-1, 425, 427, 429, 431, 433, 434, 439, 441-2, 446-8, 454-5, 456-8, 464, 467-9, 474-5, 497, 500-1, 502, 574, 579, 598; II, pp. 5-7, 20-2, 49-50, 59-60, 62, 66-7, 70-1, 76-9, 81-3, 87-8, 102-3, 114-7, 132, 146, 159, 163, 174, 189-90, 190-2, 224-5, 255-6, 243-4, 245, 264, 288-9, 297-9, 316-7, 319, 323, 327, 334-6, 344, 350, 363, 381-3, 386, 400, 446.

<sup>126</sup> *The Third Intermediate Period in Egypt (1100-650 B.C.)* (Warminster, Aris & Phillips, 1973), XI. For the sake of antithesis, let me adduce "Self-Reflection, Egyptian Beliefs, Scythians and 'Greek Ideas': Reconsidering Greeks and Barbarians in Herodotus", *The European Legacy* 11, 2006, 1-19, by the, presumably Greekless, scholar of philosophy and political science A. Ward. Her «argument (...) seeks to mediate between the claims of Bernal and the claims of scholars such as Lefkowitz and Palter. Like Bernal, I argue that for Herodotus significant elements of Greek culture, specifically Greek religious thought and practice, have their source in Egypt to the south. Yet, like Lefkowitz and Palter, I also conclude that the development of phi-



flea market in which the older the stuff the more valuable it turns out to be. So far as the *Hellenorientalia* extend, we needed a well-ordered store with the latest novelties on display and a wise selection of vintage lore looming in the background for the sake of evaluation or illustration. Astour attempted, within his chosen parameters, to furnish us with this tool; the dilettante Bernal, who could not aim at such a goal, did not care one bit about what was doable and needed. This is where I draw the line between them.

Not only will Indo-Europeanists conceive renewed contempt for the Semitic comparanda owing to his transgressions; the danger is considerable of them growing ever more sectarian and distrustful of all outsiders. Their lack of sympathy for intruders from neighboring fields appears to be rivalled only by their, more or less benign, acceptance of the trifunctional lore of the French. (Even such a linguistic powerhouse as West<sup>127</sup> is as good as wasted on them, whether *The East Face of Helicon* or *Indo-European Poetry and Myth*<sup>128</sup> — perhaps the much more focussed *Old Avestan Syntax and Stylistics. With an Edition of the Texts* [2011] will put down the walls of their isolationism.) Thanks a lot, *Black Athena*: far from promoting a spirit of friendly emulation between philologists from the non-Indo-European Levant, especially Egyptologists (whose specific blinkers so far as theoretical linguistics extend are already spectacular, see Winand, *Temps et aspect en égyptien*, 6-9), and classical linguists, this series has put in

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losophy and science among the Greeks, especially in contrast to the Scythians, was a uniquely Greek achievement. The core of my argument that unites these two claims is that, although the development of Greek philosophy and science and other cultural achievements may be attributed to the Greeks themselves, Herodotus indicates that the grounds or conditions that made these developments possible can be attributed to the prior impact of Egyptian religion» (3-4). Predictably, this article is a confusing mixture of borrowed detail and unhelpful generality.

<sup>127</sup> The victim of Bernal's jealous scrimping and scraping at *Black Athena Writes Back*, 319-44, his lifelong engagement with the Near East (minus Egypt) is well assessed in a review article by K. Dowden: "West on the East: Martin West's *East Face of Helicon* and its Fore-runners", *Journal of Hellenic Studies* 121, 2001, 167-75. On his way, Dowden presents the least biased survey to date of the past two centuries of research on the Oriental influences on Greece (168-71). A fair-minded researcher, he even tries to rehabilitate the rather unlikable Astour (within reasonable limits, that is: in a most tentative and modest manner): pp. 170, 175.

<sup>128</sup> They must have heard about his quirks. His prehomeric dating of Hesiod, which he has maintained since the late sixties in the face of very well articulated opposition (overwhelmingly on linguistic grounds: G. P. Edwards, R. Janko, Ruijgh), casts a shadow on his, otherwise splendid, verbal scholarship, while it leaves him vulnerable to biased endorsement by Bernal (G. Nisbet, "Hesiod, 'Works and Days': A Didaxis of Deconstruction", *Greece & Rome* 51, 2004, 147-63 at 150, 150-1 note 14). His Iliadic scenario is fated not to gain West many followers and might even cut down the number of his admirers, in that it caters neither to the prevailing Oral Poetics nor to the Neo-Analytic current still entrenched in Germany while displaying what can be construed as high-handedness (*The Making of the Iliad. Disquisition and Analytical Commentary* [Oxford, Oxford University Press, 2011], 1-68, 431; his tone is flippant, his arguments extremely uneven, and what is gained by calling 'P' the poet who revised and amplified his own creation?).

the head of the epigons of Brugmann and Meillet a renewed emphasis on the need to ignore the folly of Indo-European bashers! One shudders at the thought of the consequences for higher education, research, or the interethnic relations, of Bernal's teachings ever becoming mainstream. «Subjects like history and philosophy will be replaced by indoctrination, and each of us will believe the brand of "truth" that best serves his or her own selfish purposes» (Lefkowitz, *History Lesson*, 76-7); the cause of African-Americans, or Europeans of black descent, will trump the pursuit of truth; dedication to this cause will replace academic training, visceral engagement (through racial inheritance) with the 'spirit' of those times will cancel the patient weighing of the evidence; the need for any updating of our handbooks and dictionaries of Egyptian, Hebrew, Phoenician, or Akkadian will no longer be felt, as the century-old ones are enough to bury the would-be lexicographer of Greek under a morass of Semitic etymologies, whose superficial neatness guarantees their rightness to uninstructed eyes; and fact-based research being thus disheartened, eventually there may come to be «no progress, no revolution of ages, in the history of knowledge, but at most a continuous and sublime recapitulation»<sup>129</sup>. One needs not wonder why initially sympathetic scholars like Berlinerblau, Bomhard, Jasanoff, Levin, Moore, grew distant<sup>130</sup>. If these experts thought they could not

<sup>129</sup> U. Eco, *The Name of the Rose* (San Diego, Harcourt, 1983), 399, as the ultimate wish of Jorge. This bleak view contrasts with the picture drawn by Van Binsbergen when he insists on the liberating potential of Bernal's intellectual undertaking in the context of our era's global politics of knowledge (Fauvelle-Aymar *et al.*, 130, 132, 145-6, 147, assumed in *Black Athena Comes of Age*, 62). No matter the strength, or lack thereof, one concedes to his arguments, the suspicion can hardly be quelled that they are but an instance of politically correct rubbish designed to save face and salvage the main thrust of *Black Athena*. All the more so in the light of Van Binsbergen's lack of qualification as a classicist (whoever remains skeptical is advised to read his "Before the Presocratics. Cyclicity, Transformation, and Element Cosmology: The Case of Transcontinental Pre- or Protohistoric Cosmological Substrates Linking Africa, Eurasia and North America", *Quest. An African Journal of Philosophy* 23-24, 2009-2010, 394 pp.; they will face a recrudescence of hollow rambling about systems or thought processes citing decontextualized evidence, Greek words, and secondary literature, with a view to collapse the standards of philosophical history in favor of a wide interchange of ideas actually so vague as to be unremarkable). No Semitist, classicist, or Indo-European scholar worth their reputation would get it into their mind to decide what is valuable in African studies, Van Binsbergen's province, from the viewpoint of their speciality, and where the succulent pastures lay; yet he does the reverse as an anthropologist, and presumes to teach us where we stand with respect to progress. This I call impudence.

<sup>130</sup> According to Bernal's own testimony, and generous allowance being made for mnemonic lapses and *interpretationes a posteriori*. See *Geography of a Life*, 247, 398-9 (Jasanoff); 399 (Gordon); 403 (Levin); 404-405 (Bomhard); 441, cf. 446 (Moore); 445-6 (Berlinerblau). Although not paranoid, this retelling of past relationships eschews candor inasmuch as Bernal always questions the reasons of these friends for becoming estranged or shunning him, but never asks himself whether or not his ideas, his manners, and his public behavior were congenial to them qua experts (only Moore was not a Semitic scholar or a linguist; of his reluctance to edit more manuscripts of Bernal's after his work on *Black Athena Writes Back*, I am tempted to declare: 'once burned, twice shy').

go on with being his friends; if pioneers in the comparative assessment of the Greek and Semitic languages and cultures of the quality of Levin and Brown declined to enter the fray yet never ever declared themselves in favor of anything akin to cultural transfers from the Levant to Greece *more Bernalio*<sup>131</sup>; and if even Gordon, the great paragon of Pan-Semitism, did not see fit to support Bernal after his review of *BA I* — why on earth should Classicists, when presented with a massive amount of evidence as to his inanity<sup>132</sup>, shy away from calling *Black Athena* intellectually brain-dead?

<sup>131</sup> Unlike Griffith, *Mummy Wheat. Egyptian Influence on the Homeric View of the Afterlife and the Eleusinian Mysteries* (Lanham-Plymouth, University Press of America, 2008): a learned, if ultimately unconvincing, book (N. Lazaridis, *Bryn Mawr Classical Review* 2009.04.25) that, in my view, improves very little on his “Sailing to Elysium: Menelaus’ Afterlife (*Odyssey* 4.561-569) and Egyptian Religion”, *Phoenix* 55, 2001, 213-43. This article was as remarkable for its etymologies in need of qualification (a smackdown of his championing of μάκαρ < mǝ-ħrw in *Aristarchus antibarbarus*, 198-9 note 10) as for its flights of imagination (read rather the pedestrian L. Albinus, *The House of Hades. Studies in Ancient Greek Eschatology* [Aarhus, University Press, 2000], 81-97).

<sup>132</sup> By which I mean both *Black Athena Revisited* and *Not Out of Africa*<sup>2</sup>. It is nothing short of extraordinary, in my eyes, that Bernal’s sour disposition towards these two books, along with his blackening of their authors in an *ad hominem* manner, could be misconstrued by some classicists as a case of ebullient yet basically right *droit de réponse*. He fabricated a narrative identifying *Black Athena* with a progressive stance versus the old-fashioned conservatism of Lefkowitz (whom he also charges with incompetence for her ‘so many mistakes’: *Black Athena Writes Back*, 377) and experts shaken in their certainties — a cunning obfuscation propagated by the sociologist of religions J. Berlinerblau, *Heresy in the University. The Black Athena Controversy and the Responsibilities of American Intellectuals* (New Brunswick-London, Rutgers University Press, 1999), 5-6, 36-8; the historian of classical scholarship C. Stray, *Journal of Hellenic Studies* 117, 1997, 229-31 at 230-1, who caters to Afrocentrists by writing such sentences as this one: «*Black Athena Revisited*, to some the publication (sic) of scholarly assessment by a group of serious academics, is dismissed in other quarters as a concerted attack by reactionary intellectuals, backed by right-wing foundations, on radical views which offer hope to oppressed minorities» (p. 230); and, at its most trenchant, by the Latin and gender scholar M. M. Levine, “The Marginalization of Martin Bernal”, *Classical Philology* 93, 1998, 345-63, notably 354-61. I will go farther than the understated ‘failed effort at evenhanded criticism’ Lloyd-Jones directed at this piece (“Interesting Times”, 596 note 56): her fondness for snappy commonplaces (p. 348 «it is always easier to “deconstruct” than to “construct”. If nothing more, *Black Athena 2* compels us to confront this truism once again» etc) and apodeictic utterances (p. 349 «as a group, the essays most useful to classicists are those that treat the substance of Bernal’s evidence for an Egyptian presence in late Bronze Age Greece» etc) cloaks an ignorance of language and philology (she can only pit Rendsburg’s ‘Etymological Response’ against Jasanoff-Nussbaum [p. 358 notes 9-10], before impugning what she calls the ‘absolutism’ [p. 359-60] of the latter scholars), and of the Near East in general, which should have dissuaded her from acting all high and mighty with masters in those disciplines. The narrative of a progressive Bernal vying with hard-nosed pundits stuck in their rigidities from eras past was exploded by T. A. Schmitz in an e-pamphlet that does not seem to have made many ripples (“Ex Africa Lux. *Black Athena* and the Debate about Afrocentrism in the US”, *Göttinger Forum für Altertumswissenschaft* 2, 1999, 17-76 at 47-50; neither Slack nor the contributors to Fauvelle *et al.*, *Afrocentrismes*, noticed it, whereas *Black Athena Writes Back* barely pays lip-service to its riches). It was only natural that this fallacy should

Whether or not he made the profession more self-conscious in race-related issues is irrelevant, since he never put such a claim at the core of his ambitious revisionism. Let those who stick to this *fable convenue* — from Rankine, *Ulysses in Black. Ralph Ellison, Classicism, and African American Literature* (Madison, University of Wisconsin Press, 2006), 71-7, 204-6, to D. E. McCoskey, *Race. Antiquity and its Legacy* (London-New York, Tauris, 2012), 172-81 — digest the review of *BA II* by L. Lesko in the *Journal of Interdisciplinary History* 24, 1994, 518-21, then come back to my piece and watch by themselves how Bernal's Near Eastern scholarship, methods, and technical standards have improved between 1991 and 2006<sup>133</sup>. Has he even deferred to the rule evoked by Marchand-Grafton, that intellectual historians should read the sources on which they profess to pass judgment, not merely quotations from them or derivative works about them ("Martin Bernal and His Critics", p. 4)? I will take a leap of faith and predict the answer of our yes-sayers: not much. «It is Bernal's own intolerance that I find most revolting», writes Lesko (p. 519); «among the most painfully obvious flaws in volume II are the countless repetitions of phrases, sentences, paragraphs, quotations, even erroneous calculations (...). Inconsistent transliterations, missing diacritics, meanings far removed from the root meanings of words could be excused, but for someone attempting to convince scholars rather than merely impress the masses, his use of consonantal and vocalic shifts, metathesis, and analogy to produce whatever equation he desires is far too loose. I expect more careful work at all levels» (p. 520); and finally, «"misplaced precision" is his critique of careful scholarship that resulted in conclusions different from

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leave traces in subsequent scholarship, whether in monographs of the highest order (Moyer, *Egypt and the Limits of Hellenism*, 29 and note 105, 50 note 28, who seems to put Bernal and Lefkowitz on a par, takes for granted the calumnies of the former scholar on the latter) or in works of a more lightweight quality (P. Vasunia, *The Gift of the Nile. Hellenizing Egypt from Aeschylus to Alexander* [Berkeley, University of California Press, 2001], 16-7 and elsewhere, displays an enthusiasm about *BA I-II* which borders on hysteria and contrasts with his own control of the evidence). Let the present pages set the record right: despite Levine's vocal assertions of the contrary (356, 360), waiting for Bernal's 'answers' proved vain; as in the case of *Black Athena Writes Back* (rightly shot down by P. Cartledge, *Classical Review* 53, 2003, 238-9, as a chutzpah and a self-indulgence), these answers merely contribute more confusion to an already muddled field of inquiry.

<sup>133</sup> The degree of bias, ignorance, and levity in the Rankine contribution which purports to assess Bernal's qualitative impact ("Does Black Athena Make a Critical Contribution to our Understanding of the Ancient World?", in P. A. Miller (ed.), *History in Dispute. Classical Antiquity and Classical Studies* [Farmington Hills, Mich., St. James Press, 2005], 1-10) is little short of astonishing. One may cite, among its numerous *reductiones ad absurdum*, «it is perhaps important to note, as Lefkowitz does, that Bernal had no formal training in classical philology or Egyptian» (p. 4) and «certainly Bernal's Models are crude, but even his opponents concede several crucial arguments, including the notion that classical scholars of the nineteenth century were essentially Eurocentric» (p. 5); he even fails to disclaim that *BA I-II* are error-ridden. I entertain little hope, by way of consequence, of teaching Rankine something.

his. (...) Bernal tries to accumulate enough possibilities to demonstrate his preconceived ideas. It is the worst possible example of “scholarly” writing that could be recommended to students» (*ibid.*)<sup>134</sup>. He cannot by any means be turned into a pioneer whose power of asking the right questions, albeit embarrassing ones, redeems the technical mediocrity. Who but a dimwit would take unexplained Latin words from Egypt without any intermediate after having declared that «no evidence exists of an Egyptian presence in Italy in the first half of the first millenium BCE. Thus, any Egyptian influence on early Rome would be indirect, through Etruscan, Punic or Greek<sup>135</sup>» (p. 184)? Who but a dilettante would explain the verb ἐλεγαίνειν from the substantive *izkb*, ‘mourning, bereavement’, mistranslated ‘tearing the hair’ because it is spelt with the sign for the lock of hair (D3), then dream up that «ἔλεγος was a ‘song of mourning’. The diminution of women’s rights brought an attempt to tame mourning and to have it controlled by men in

<sup>134</sup> «One hesitates to review a book so incompetent as this one. It is a thankless task. Readers who know any of the (...) languages will see in a moment that it proposes nonsense. But Classicists (...) should be warned not to take this latest attempt seriously» (Hoffner [note 3], 37, on Davis’ last book). So I did not follow L. P. Williams, “Why I Stopped Reading *Black Athena*”, *Academic Questions* 7, 1994, 37-9, even if every aspect of *BA III* is abysmal and throws away the few points Bernal might have scored. Unhappily, ill-informed classicists (Rankine; F. M. Ahl, *Two Faces of Oedipus. Sophocles’s Oedipus Tyrannus and Seneca’s Oedipus Translated and with an Introduction* [Ithaca, Cornell University Press, 2007], 76, who also cites Budge [!]; etc) and philosophers (T. Curnow, *Wisdom in the Ancient World* [London, Duckworth, 2010], 81, 149, 170), as well as seasoned philologists who should know better (I. Rutherford: *Aristarchus antibarbarus*, IX-X), draw on *BA III* as if it were a mainstream or trustworthy authority. So the colossal pretence behind Bernal’s work could not be left unadressed, lest sheer quantity be allowed to impose upon the tired or the gullible.

<sup>135</sup> Egyptian influences on Italy are attested between the eighth and the fifth centuries B.C., see P. Kingsley, “From Pythagoras to the *Turba philosophorum*: Egypt and Pythagorean Tradition”, *Journal of the Warburg and Courtauld Institutes* 57, 1994, 1-13 at 3-4; they include items traded by the Phoenicians which were recovered in Italian tombs (by no means recent news, cf. T. J. Dunbabin, *The Western Greeks* [Oxford, Oxford University Press, 1948], 233-5, 461-5), and religious or ‘wisdom’ traditions (as reflected in some of the Orphic gold plates, though this is not quite certain, *pace* Kingsley, and rival Mesopotamian influences on these texts, or some strikingly similar documents, do exist: A. Lebedev, “Pharnabazos, the Diviner of Hermes: Two Ostraka with Curse Letters from Olbia”, *Zeitschrift für Papyrologie und Epigraphik* 112, 1996, 268-78 at 276-8). On the other hand, Pythagoras’ ideological debt to Egypt, a predominantly Middle- and Neo-Platonician view (D. J. O’Meara, *Pythagoras Revived. Mathematics and Philosophy in Late Antiquity* [Oxford, Clarendon Press, 1989], 13 — but Numenius, fr. 1, makes for extremely poor evidence: E. des Places, *Numénius. Fragments* [‘C.U.F.’, Paris, Belles Lettres, 1973], 103 note 3 —, 26-7, 93-5), we have very little reason not to deem a fiction after Burkert (*Weisheit und Wissenschaft. Studien zu Pythagoras, Philolaos und Platon* [Nürnberg, Carl, 1962], 103-5 = *Lore and Science in Ancient Pythagoreanism* [Cambridge, Mass., Harvard University Press, 1972], 126-8). Bernal’s suppression of all Egyptian links to Italy, the country of election for Orphic-Dionysiac rites in the fourth century and later (e.g. West, *The Orphic Poems* [Oxford, Clarendon Press, 1983], 24-6) is as bad as his awareness of Egyptian motives in the *Orphica* (*BA III*, pp. 260, 374, 467).



the ἔλεγεϊον ‘elegy’, spoken in a male voice» (p. 436)<sup>136</sup>? Even if Bernal had been a pioneer who did good things in the long run so can be forgiven for his sins, the likes of McCoskey could hardly tell<sup>137</sup>. Indeed, it is not incumbent upon literary scholars of Greek to spearhead the final assessment of *Black Athena*, no matter their sympathy (or lack thereof) for the ideas of Levine. Whether or not they are experts on the reception and historiography of the classics, or on race, or on cultural contacts in general, has little bearing on the issue, particularly when they can only contribute theory. And having no inkling of textual criticism even in their home turf, they can only turn a blind eye to the crudely mechanical handling of Near Eastern compositions<sup>138</sup> by wild diffusionists. To put things in a blunt manner: as they lack tools and credentials for this endeavor, if McCoskey and co. nail their colors to the

<sup>136</sup> A piece of demented balbutience, it ignores, among other considerations, that the early Greek elegy was emphatically not a mere song of mourning; for a discussion of this literary genre, encompassing a rich analysis of ἔλεγχος, ἔλεγεϊον, and ἔλεγεία, go to West, *Studies in Greek Elegy and Iambus* (Berlin-New York, De Gruyter, 1974), 2-21, particularly 3-9. Significant material has accrued since then, which sheds light especially on the historical elegy favored by Simonides of Ceos (see L. M. Kowerski, *Simonides on the Persian Wars. A Study of the Elegiac Verses of the ‘New Simonides’* [New York-London, Routledge, 2006], 63-146, not without the reminder by D. Sider, “The New Simonides and the Question of Historical Elegy”, *American Journal of Philology* 127, 2006, 327-46, that ‘historical’ is quite the misnomer, particularly when brought to bear on generic discussions [331-7]).

<sup>137</sup> Even though they (should) have enough technical equipment in classics to know when Bernal is acting like a schoolboy (witness his unenlightened use of Loeb editions; at p. 213, cf. 628 note 22, he goes to the length of quoting an Hesiodic fragment from Evelyn-White instead of Rzach, or Merkelbach-West!) and when he has left unopened the most basic bibliography (Hesiod is subpoenaed before his court with no regard whatsoever for West’s commentaries, let alone all later scholarship, like L. Koenen, “Greece, the Near East, and Egypt: Cyclic Destruction in Hesiod and the Catalogue of Women”, *Transactions of the American Philological Association* 124, 1994, 1-34; Bernal would have learned from it about «the cultural layers out of which both Hesiod and the author of the *Catalogue* formulated their own concepts» [p. 34], yet he only cites Koenen as a foil, 4-6). For a classicist, to remain blind to such proofs of Bernal’s inadequacy as an Hellenist comes tantamount to admitting one’s own incapacity / unwillingness to assess his work. See Rosól, *Frühe Semitische Lehnwörter...*, where Bernal is castigated, 300 words previously deemed Semitic are disproved (155-216), and 65 validated as such (21-112).

<sup>138</sup> Whose degree of textuality inamusch as it bears on editorial technique has always been a sore spot for assyriologists, particularly in matters sumerological (the standard position, per se and for the building of a ‘composite’ text versus a ‘score’, is articulated best in Michalowski, *The Lamentation over the Destruction of Sumer and Ur* [Winona Lake, Eisenbrauns, 1989], 21-5). A quantum leap can be expected from M. Worthington, *Principles of Akkadian Textual Criticism* (Boston-Berlin, De Gruyter, 2012), here at 41-4; P. Delnero, *The Textual Criticism of Sumerian Literature* (Boston, American School of Oriental Research, 2012), concerns itself with the issues of orthographic and finer semantic variations in Old Babylonian texts (cf. his 2525-page *Variation in Sumerian Literary Compositions. A Case Study Based on the Decad*, diss. Pennsylvania, 2006, at 1844-56). Michalowski, *The Correspondence of the Kings of Ur* (Winona Lake, Eisenbrauns, 2011), 211-24, tangles with the overwhelming difficulty of matters of authenticity.

mast with the view that *Black Athena* has somehow been epoch-making or foundational, their making a fool of themselves will be far less momentous than the stigma affixed on our discipline. For classics get debased in the eyes of specialists of the areas and disciplines infringed upon by Bernal each time one of us asseverates that the core of *Black Athena* is the triad race - Rezeptionsgeschichte - Geistesgeschichte<sup>139</sup>, viz. an a priori construct which stands by itself and may impress the unwary practitioners of these three crafts, rather than the languages and civilizations of Greece, Africa, and the Near East. The implication is of course that neither a minute knowledge of the particulars of the Egypto-Levantine gamut of disciplines supposed to root *Black Athena* nor the control of comparative ingenuity by a sure and steady judgement are what it takes to treat Bernal fairly. The amount of detail the present discussion canvasses aims at eliminating for good this kind of apology, all too attractive in an age where doctrinaire models are allowed to run afoul of the limits of philology (what passes for theory in our post-postmodern era ought not to be allowed to strong-arm the reader).

With respect to pre-Hellenistic Greece, such truth as there is to the proverb *ex Africa semper aliquid novi* has come undone before our eyes as regards *BA III*: the serious reader with access to a fully staffed Near Eastern and Egyptological library might be able to glean useful information on a range of particulars<sup>140</sup>, yet the construction includes so much that is of doubtful

<sup>139</sup> His all-pervasive notion of a white racist conspiracy engaged in the downplaying of Egyptian achievements whatever their date is lost on these yes-sayers and even some reviewers (A. H. Joffe, *Journal of Near Eastern Studies* 64, 2005, 146-50, whose parting shot on *Black Athena Writes Back* is «in the end was Greece a European or Levantine creation? It was neither; it was a Greek creation. If nothing else, Martin Bernal and the Black Athena project remind us of this fact»). Not only should they read the demolition of some of the broader swipes of Bernal's portrayal of Mohammed Ali by Slack, 554-6, plus Blok, 'Proof and Persuasion in 'Black Athena': The Case of K. O. Müller', *Journal of the History of Ideas* 57, 1996, 705-24 (barely damaged by the rebuttal in *BAWB*, 190-6); they ought to keep in mind the extent to which the historiographical truth is the reverse of what Bernal holds. Levine, "The Use and Abuse of Black Athena", *American Historical Review* 97, 1992, 440-60, deemed *BA I* on the right tracks; she was too ready to believe (e.g. «armed with a formidable aegis of scholarly apparatus (...) marshals considerable epigraphic, linguistic, and archaeological evidence», p. 442) and ignorant. For D. Gange, "Religion and Science in Late Nineteenth-Century British Egyptology", *The Historical Journal* 49, 2006, 1083-103, has demonstrated that «the imperial agendas that would have encouraged a negative image of ancient Egypt were in fact pushed deep beneath the surface of Egyptology, being submerged under agendas relating to the Bible which inspired a remarkably positive image of Egyptian civilization» (p. 1085). Cf. also p. 1103: «far from denigrating ancient Egypt as Bernal suggests, most Egyptologists reacted to it with awed respect, considering it to be the civilization in which Moses - according to St Paul - had learnt his wisdom, providing evidence that Christian ethics were God-given: as old as civilization itself. Egyptian archaeology, rather than being 'the handmaiden of history', remained the handmaiden of theology for several decades past the point at which religious belief has been considered to have ceased to be definitive of archaeological practice (...).»

<sup>140</sup> On the express condition that he begins by discarding Bernal's bibliographical indica-

truth and relevance that it belongs in the clouds. Other than a fascination with Egypt envisioned as the cradle of civilization and mother of wisdom<sup>141</sup>, the Egyptian impact on the religion, literature, language, and way of life of the late Archaic and Classical Greeks is virtually non-existent. Bernal eschewed this very barrenness of evidence by using an approach that harks back to times when there was no method<sup>142</sup> other than the compilation of whatever primary data could be dug up regardless of age and pedigree. So when *BA III* prides itself on having embroidered a large-scale continuum around the testimonies of Herodotus and Plato interpreted at face value, it passes off for compelling demonstrations a proliferation of detail, some of it indeed neglected this century, but the huge majority of it perfectly incapable of supporting the weight of Bernal's deductions, so great is his

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tions or other pointers to avail himself afresh of the *status quaestionis* whenever he feels something is amiss. One example will show what I mean. Αἰών is supposed by *BA III* both to be Indo-European (e.g. Keizer, *Life-Time-Entirety*, 17 note 4; we now reconstruct PIE \**h<sub>2</sub>ai-u-on-*: Wodtko-Irslinger-Schneider, 278, 281-2 note 11) and to have been influenced by *iwn*, 'pillar', a semantic absurdity which, among other explanations, stems from Bernal's ignorance of G. Griffiths, "An Egyptian Antecedent of ΑΙΩΝΕΡΓΕΤΗΣ", *American Journal of Archaeology* 89, 1985, 167-8: the epithet of Sarapis αἰωνεργέτης, 'creator / maker of eternity', seems to be the exact calque of the participial phrase 'he who creates eternity', *ir-dt*. Bernal fails to quote the literature posterior to Onians; he has thus missed that a special use of αἰών = ὁ νοητικὸς μυελός is possible (so E. Degani, *Αἰών. Da Omero ad Aristotele* [Padua, Cedam, 1961], 21-2; etc) but leaves too much to be desired, particularly if one understands the medical sense as 'life-fluid' and takes it as the root value of αἰών (viz. Onians' thesis; it has been nearly unanimously discarded, from Degani, 131-5, to Keizer, 19-20). Read also Grimal, *Termes*, 46 note 6.

<sup>141</sup> R. Baumgarten, *Heiliges Wort und heilige Schrift bei den Griechen. Hieroi Logoi und verwandte Erscheinungen* (Tübingen, Narr, 1998), 171-221 (Egyptianizing corpus); A. Henrichs, "'Hieroi Logoi' and 'Hierai Bibloi': The (Un)Written Margins of the Sacred in Ancient Greece", *Harvard Studies in Classical Philology* 101, 2003, 207-66 at 225-7. Even a great specialist of religion can blur the lines here, see R. Turcan, *Les cultes orientaux dans le monde romain* (Paris, Belles Lettres, 1992<sup>2</sup> [1989]), 77-8 — and correct his «très tôt les récits 'sacrés' (*hieroi logoi*) de l'Héraion d'Argos identifèrent Io avec Isis, Épaphos avec le taureau Apis» in light of L. G. Mitchell, "Euboean Io", *Classical Quarterly* 51, 2001, 339-52, especially 342, 346, and "Greeks, Barbarians and Aeschylus' *Suppliants*", *Greece & Rome* 53, 2006, 205-23 at 207 note 6 (not before the late seventh or early sixth century B.C.).

<sup>142</sup> You must go patiently and circumspectly to work, approaching your topic from several sides and sparing no pains to ascertain both that your primary evidence excludes nothing vital and that your selection from secondary sources is representative yet not tautological; weighing, or trying to weigh, the probability of all alternative solutions to the one you prefer, comes once you are done checking out the textual data your argument will embed. This examination of the linguistic evidence you ought to perform not the way your sources handpicked it, particularly if these are old or not mainline, but as the contemporary, acknowledged experts of these languages use it, viz. critically. In our technological era there are solutions to overcome a lack of linguistic expertise in the Near East (attractive advice in P. Briant, *Histoire de l'empire perse. De Cyrus à Alexandre* [Paris, Fayard, 1996], 13 = *From Cyrus to Alexander. A History of the Persian Empire* [Winona Lake, Eisenbrauns, 2002], 4-5); so Bernal's blindness and lack of sifting cannot be forgiven.

lack of philological control and awareness of what was really at stake in each case. It takes a classical scholar of some calibre to pick out the wheat from the chaff in matters of lexicography and historiography without falling prey to the animus of the Greek writers fascinated by Egypt<sup>143</sup>; one needs have too an extensive acquaintance with Egypt and Mesopotamia to be able to handle these civilizations without living from hand to mouth in the trail of the pundits. Yet like the average Afrocentric writer, Bernal merely possesses an indirect grasp of Egyptian and Semitic, if not Greek. A state of affairs that did not improve from *BA I* to *BA III*, it makes one doubtful of the depth of his dedication during the last quarter century. As he combines these paltry standards with Afrocentrists' usual distrust of mainstream authorities and his personal love of obscure scholarly resources, no matter how obsolete they can be, it is no cause for wonder that he was blinded to the inevitable: the probability of a boastful layman succeeding where Gordon and his first-hand familiarity with all the Levantine scripts, languages, and bibliography, had

<sup>143</sup> Thus Hecataeus of Miletus (S. R. West, "Herodotus' Portrait of Hecataeus", *Journal of Hellenic Studies* 111, 1991, 144-60 at 151-9), Herodotus, and Plato were much more eager to credit the impressive, long-remembered Egyptians than to bet on the Hellenic ingenuity or on Near Eastern influences. Other than broad surveys like C. Froidefond, *Le mirage égyptien dans la littérature grecque d'Homère à Aristote* (Aix, Ophrys, 1971), 115-207 (130-5 for Herodotus 2.35 sqq.); S. M. Burstein, *Graeco-Africana. Studies in the History of Greek Relations with Egypt and Nubia* (New Rochelle, Caratzas, 1995), 7-8, 9-12; or Vasunia, *Gift of the Nile*, 75-116, 216-47, read L. V. Zakbar, "Herodotus and the Egyptian Idea of Immortality", *Journal of Near Eastern Studies* 22, 1963, 57-63 («all we can say is, that the information of the Egyptian priests, or rather insufficiently instructed interpreters, about the various forms which the dead could assume in his happy after-life was associated by Herodotus with the Orphic-Pythagorean doctrine of transmigration known to him from the Greek sources and the origin of this doctrine was attributed by him to the Egyptians» [p. 63]). I deem it striking that the amount of crypto-Egyptian lore embedded in the works of the classical authors who spear-headed this Hellenic fascination for Egypt does not cut a figure that much higher than the total of similar nuggets lurking in Imperial writers, mainly Plutarch and the romancers (J. J. Winkler, *Auctor & Actor. A Narratological Reading of Apuleius's Golden Ass* [Berkeley-Los Angeles-Oxford, University of California Press, 1985], 306-19; Rutherford, "Kalasiris and Setne Khamwas: A Greek Novel and Some Egyptian Models", *Zeitschrift für Papyrologie und Epigraphik* 117, 1997, 203-9, for Heliodorus; etc). Even the belated Nonnus preserves mythographical data from Middle Kingdom Egypt (P. Chuvin, *Mythologie et géographie dionysiaques. Recherches sur l'oeuvre de Nonnos de Panopolis* [Clermont-Ferrand, Adosa, 1991], 236-9). Is not Herodotus' own brand of Egyptomania supposed to have been vital or groundbreaking (the latter view being Moyer's: *Egypt and the Limits of Hellenism*, 277-8)? Centuries of Graeco-Roman rule over Egypt played a role in the diffusion of the *Aegyptiaca*; of course, things were slowly changing there and an inevitable amount of obsolescence, discounting the philosophical interferences of Greek pedigree like Middle Platonism, ensues in later accounts (G. Griffiths, *Plutarch's De Iside et Osiride*, 18-52 *passim*; J. Hani, *La religion égyptienne dans la pensée de Plutarque* [Paris, Belles Lettres, 1976], 26-117; etc), yet I wonder whether or not the Imperial writers were better acquainted with aspects of Egypt than Herodotus or Plato could (hope to) be. In other words, there were perks to being a πεπαιδευμένος in the first centuries AD.

failed<sup>144</sup> was nil right from the start. Healthy levels of modesty on Bernal's part were warranted: did not Astour, a yet more encyclopaedic mind than Gordon, only manage to burn himself out?

Now, unless they desire to be crowned kings of shreds and patches by readers more savvy than they in Near Eastern minutiae, there is no rosy future for those who thirst after Egyptian influences on, let alone roots in, the Greek language. They will not be wasting their time in sterile pursuits provided they cast their net far narrower than Bernal and allies; for the main progress one can (tentatively) identify in this field<sup>145</sup> rests with the identification of loanwords from the Nile valley among those terms which lack clear-cut Indo-European or Mesopotamian cognates and apply to goods, items, and things that seem to have been unknown, or exotic-sounding, to the Greeks<sup>146</sup>. Trouble is, most of these loanwords lead nowhere. Suffice

<sup>144</sup> Not because all classicists remained skeptical out of sheer ignorance and incapacity, as Rendsburg malignantly claims ('Someone Will Succeed...', 40), but because Gordon's Semitic decipherments of Linear A, Eteochyriot, and Eteocretan collapsed under the scrutiny of ideology-free specialists. Let it be enough to defer to the Mycenologist Y. Duhoux, in *L'étéocrétois. Les textes, la langue* (Amsterdam, Gieben, 1982), 222-33, with a nod to the superb connoisseur of Anatolia A. Heubeck (review of Gordon, *Evidence for the Minoan Language*, in *Gnomon* 39, 1967, 705-9; Heubeck was unafraid to speak of 'dilettantischen Versuchen' [p. 709]). As for Gordon's knowledge of Greek, good according to Rendsburg, it was demonstrated by no less a scholar than E. L. Bennett to be poor, at least for philological purposes (review of *Evidence...* in *Language* 44, 1968, 110-8; read also *infra*, note 185). On the other hand, Semitists unaffiliated to Gordon have complained about his handling of the Levantine evidence: J. C. Greenfield, *Journal of Biblical Literature* 86, 1967, 241-4 at 242 sqq. — This bleak picture admits of one explanation only: so little did Gordon's models function, that he had to tailor his materials to them. The misrepresentation Rendsburg perpetuates has been allowed to fester for too long, feeding off the resentment of Afrocentrists towards classicists; it is timely to expose the weak sides of those Bernal regards as his model (Gordon) and his 'dear friend' (Jasanoff-Nussbaum, 203 note 1), if only to push back against that misconception.

<sup>145</sup> Leaving aside the *cultural image* carried by the Greeks among Levantines, a cardinal blind spot of such studies. As J. C. Waldbaum asks, «more important (...) is the interaction between these transplanted Greeks and their eastern hosts. Did Greeks in the East make their presence felt in any significant way, other than as purveyors of attractive tableware and other commodities? Or did the sophisticated easterners simply ignore them as western barbarians?» ("Greeks in the East or Greeks and the East? Problems in the Definition and Recognition of Presence", *Bulletin of the American Schools of Oriental Research* 305, 1997, 1-17 at 12). The earliest Greeks may have received recognition due to their and their forefathers' partaking of shared traditions (B. Wells - F. R. Magdalene (edd.), *Law from the Tigris to the Tiber. The Writings of Raymond Westbrook* [Winona Lake, Eisenbrauns, 2009], I, 303-53).

<sup>146</sup> An example of what it is possible to do is Rosól, "Griechisch κάλαθος - ein ägyptisches Lehnwort?", *Historische Sprachforschung* 123, 2010, 176-80. His rebuttal of Bernal's etymon *q/krh.t* deserves to be better known: «gegen diese Etymologie spricht jedoch ein ernsthaftes Problem im Bereich der Phonetik. Es geht hier um das auslautende -t, das ein Femininumsuffix ist. Dieses Suffix wird zwar regelmäßig geschrieben, wurde aber schon seit der Zeit des Alten Reiches nicht mehr ausgesprochen. Bernal ist sich dieses Problems bewusst und gibt die folgende Erklärung: "The borrowing could have taken place before final -t were dropped or later re-inserted as an archaism, very common in religious contexts" (S. 447). Ohne Zweifel



it presently to mention στῖμ(μ)ι, στῖμμις, Koine στῖβι, ‘stibium, kohl’, which Eustathius, in a little-known piece of information, calls ‘Egyptian’ (*Commentaries on the Iliad*, II, p. 635, 5-7 Van der Valk ὡς συμμηνηστέον καὶ ὅτι τὸ εἰρημένον οὐδέτερον ὄνομα τὸ στῖμμι καὶ στῖμμις εὐρηται λεγόμενον θηλυκῶς. γράφει γοῦν τις οὕτω· στῖμμις ἢ εἰς τὰ ὄμματα χρήσιμος, Αἰγυπτίων φωνῆι) and indeed originates in the Demotic *stm*, ‘eyeshadow’, Coptic **CTHM** — so Lewy, *Fremdwörter*, 247, Vycichl, 199, Takács, III, 602-3; absent in both editions of Prellwitz, accepted by Boisacq, Frisk, Chantraine. A descent from the Egyptian *sdm*, ‘to make up’ (Hannig, I, 1271, II.2, 2395; *sdm* Erman-Grapow, Faulkner), is remoter and should be divorced from *msdm.t*, as shown by Takács, 602 N.B. 1; both Brown, *Israel and Hellas*, 241 note 57, endorsing Fournet as his sole authority, and B. Hemmerdinger, “Noms communs grecs d’origine égyptienne”, *Glotta* 46, 1968, 238-47 at 243, are thus put out of court (the French scholar, whose sources are Crum’s Coptic dictionary and Erman-Grapow, viz. an insufficient basis, if not an inadequate one, prints (*m*)*sdm.t* as if it were a matter of triangulation from Egyptian and Coptic...). Now the derivation *stm* > στῖμμι tells us nothing except that the cosmetic blackening of eyes must have arrived in Greece from the Semitic Levant. It might turn out, too, that Homeric hapaxes or formulae preserve a Levantine residue, though one will have to look hard and elaborate cogent evidence going beyond mere echoes thematic and grammatical<sup>147</sup>. Thanks to Burkert and West studies of the Near Eastern

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ist jedoch eine Entlehnung schon im 3. Jahrtausend historisch nicht nachvollziehbar. Wenig plausibel scheint auch die Annahme einer archaisierenden Aussprache zu sein. Des Weiteren ist diese Etymologie aus semantischen Gründen fragwürdig, denn ägypt. *qrh.t* bedeutet ‚Topf und nicht ‚Korb‘» (pp. 177-8). Rosl then advocates the New Egyptian *krh.t*, a container mainly used for flowers, as the source of the syllabically spelled κάλαθος (178-89); the phonetic changes are attractive and I simply miss references (in his note 16) to Y. Muchiki, “Spirantization in Fifth-Century B.C. North-West Semitic”, *Journal of Near Eastern Studies* 53, 1994, 125-130 at 129 note 141, or Lipiński, “The Inscribed Marble Vessels from Kition”, in Z. Zevit - S. Gitin - Sokoloff (edd.), *Solving Riddles and Untying Knots. ... Studies in Honor of Jonas C. Greenfield* (Winona Lake, Eisenbrauns, 1995), 433-42 at 435, who reminds us of the Egyptian variant spelling *q/kzh.t* matching the Phoenician *q’ht* (134-5). I shall also mention T. Pommerening’s demonstration that the component mixed with human milk in a Greek remedy against female infertility is not the ‘butter’ of Latinists and Hellenists alike (βούτυρον, *butyrum*; lastly, F. J. Barnett, “The Second Appendix to Probus”, *Classical Quarterly* 56, 2006, 257-78 at 271) but, as had been seen for a century by Egyptologists, the fruit or sweet vegetable *bdd(w-kz)* (Hannig, II.1, 830), identified as the water melon by L. Manniche, *An Ancient Egyptian Herbal* (Austin, University of Texas Press, 1989), 92-3 — “Βούτυρος, ‚Flaschenkürbis‘ und κουροτόκος im Corpus Hippocraticum, *De sterilibus* 214: Entlehnung und Lehnübersetzung aus dem Ägyptischen”, *Glotta* 86, 2010, 40-54.

<sup>147</sup> I have one to put forward here. The Epic ὄρχαμος, which, ossified in the epithets ὄρχαμος (-με, -μον) ἀνδρῶν or λαῶν, applies to warriors or heroic figures in the *Iliad* before being extended to low life in the *Odyssey* (Eumaios, Philoitios), is usually interpreted with reference to Linear B *o-ka* > \*ὄρχά, ‘command’, cf. ἀρχή (*DMic.*, II, 19-21), thus ‘leader of men / people’: an awkward phrasing for servants even if one qualifies the offence as a mechanistic

impact on Greece have moved far beyond mere assertion and generalization backed by a handful of case-specific samples. Therefore whoever attempts to recover the conjectural Egyptian prototype of an epic tidbit must emulate the standard set by these masters or lack credibility<sup>148</sup>. No one ought to rebuke the existing models for lacking credibility on philological or logical grounds only to end up with a proposal which is actually far wilder. So Griffith, “Elysium Revisited”, *Journal of Indo-European Studies* 27, 1999, 79-85: he chides Burkert, Puhvel, Alford yet adduces the West Semitic *taršîš* touted by Gordon as a *Wanderwort*<sup>149</sup>; «once the weaknesses of Burkert’s

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use of the technique of oral formulaic composition (M. Ndoye, *Groupes sociaux et idéologie du travail dans les mondes homérique et hésiodique* [Besançon, Presses universitaires de Franche-Comté, 2010], 218-20; differently, and better, M. Skempis, *‘Kleine Leute’ und grosse Helden in Homers Odyssee und Kallimachos’ Hekale* [Berlin-New York, De Gruyter, 2010], 110-4 *passim*). Thus I would rather explain ὄρχαμος as imbued with the strength of a superlative, comparing the Ugaritic epithet of Baal ‘al’iy qrdm, ‘mightiest of warriors’ (Smith, *The Ugaritic Baal Cycle. Volume I* [Leiden, Brill, 1994], 153; Del Olmo Lete-Sanmartín, I, 53; Rahmouni, *Divine Epithets in the Ugaritic Alphabetic Texts*, 49-52), and the Middle and New Egyptian ḥzw.ti nfr, ‘perfect fighter’, applied to the hero of the Tale of the Two Brothers, Bata (*apud* W. Wettengel, *Die Erzählung von den beiden Brüdern. Der Papyrus d’Orbiney und die Königsideologie der Ramessiden* [Freiburg / Göttingen, Universitätsverlag / Vandenhöck & Ruprecht, 2003], 29 and note 105; on ḥzw.ti cf. Erman-Grapow, I, 217-8, *Belegstellen*, I, 38; Vycichl, 319 s.v. ȝooyr, “māle”, aussi “impétueux” et “sauvage” (dit de plantes); Hannig, II.1, 553; Schulman, *Military Rank, Title, and Organization in the Egyptian New Kingdom* [Berlin, Hessling, 1964], 49). So Homer, far from paying an extravagant and most artificial compliment to some of his humblest heroes, even under the compulsion of orality, could in fact call Achilles (3x), Agamemnon, Asios (2x), Menelas (7x), Peisistratus (3x), Polites, or Ulysses the ‘mightiest of their peers’, viz. the best at their craft, no less well than the swineherd Eumaios (6x) and the goatherd Philoithios (3x): a pointed alliance of decorum and relevance. Difficulties remain though, not least the secondary stamp of the use of ὄρχαμος for servants: indeed, both Hesiod and Aeschylus conform to the *Iliad* in predicating ὄρχαμος ἀνδρῶν / λαῶν of heroic figures exclusively (A. Sideras, *Aeschylus Homericus. Untersuchungen zu den Homerismen der aeschyleischen Sprache* [Göttingen, Vandenhöck & Ruprecht, 1971], 36, 192), thereby making it likely that such was the epithet’s original range of meaning.

<sup>148</sup> The point of departure should be a widely recognized perplexity rooted in philological fact, such as the puzzling characterization of Atlas close to the start of the *Odyssey*. F. Mewes, “Atlas und Schu. Zur Deutung von Od. 1.52-4”, *Hermes* 133, 2005, 131-8, solves it by adducing the primeval Shu and *Theogony*, 517-9, 746-8: «auch die Atlas gewidmeten Verse der etwa zeitgleich mit der „Odyssee“ entstandenen „Theogonie“ Hesiods enthalten einige Hinweise auf eine Verbindung mit dem ägyptischen Schöpfungs-mythos. Atlas wird als *stehend* und die *Breite* des Himmels mit seinem *Kopf* und den *Händen* stützend beschrieben. Halten wir uns die Darstellung des den Leib der Himmelsgöttin stützenden Schu vor Augen, so ist der Kopf samt Schmuck auf nahezu einer Linie mit den Händen, womit man ihn, da der Leib Nuts in seiner Spannung der Linie *linke Hand - Kopf - rechte Hand* folgt, als neben den Händen stützend betrachten kann» (p. 137). This might very well be right; but all the Near Eastern parallels to the deed of Atlas put together leave one crucial issue unaddressed, namely the problematic, if not pointless, epithet ὀλοόφρων affixed to the Titan.

<sup>149</sup> Not *tršš* (Griffith) — from ‘wine-red, wine-dark’ (not ‘vinous, of wine’, G.), it would have come to apply to the Mediterranean (as against the Red Sea and the Black Sea); whence

and Puhvel's theses have been clearly exposed and the objection to the bastard quality of the phrase on Alford's theory has been met, the probability that Egyptian *šht izrw* in fact inspired Homer's Ἡλύσιον πεδίον is revealed to be very high indeed» (p. 84)<sup>150</sup>. The traditions compared need preferably be homogenous enough for a straightforward line of transmission to appear<sup>151</sup>;

a loanword θάλασσα and the calques ἐπὶ οἴνοσα πόντον ~ ἐνὶ οἴνοσι πόντωι. Griffith analogizes this to the hypothetical derivation *mš<sup>c</sup>-hrw* > μάκαρ, then he posits «a pattern: if Greek likewise borrowed *šht izrw* twice, as a loan, \**sekha(n)* Ἡλύσια(ν), and as a calque, \**δονάκων* πεδίον, it is not hard to imagine a further stage wherein by mutual contamination the two forms produced the attested phrase Ἡλύσιον πεδίον» (p. 83). This overly complex chain of hypotheses does not rest merely on thin air; its point of departure has collapsed. The Hebrew *שִׁשְׁבִּיט*, *taršiš*, designates a precious or semiprecious stone in the greenish-yellowish range (*Theological Dictionary of the Old Testament*, XV, 790-1[Lipiński]; etc), and the toponym; Gordon's etymology comes from *שִׁירֵי, tīrōš*, 'wine, but also 'grape' (*שִׁשְׁבִּיט* would then be a *qatlil* formation of the root which gave us *tīrōš*, viz. *TRŠ*: Hoftijzer-Jongeling, II, 1234, s.v., 1). However, the whole gamut of Semitic etymologies is ruled out by the Iberian \**√TRS* / *TRT* (M. Koch, *Tarschisch und Hispanien* [Berlin-New York, De Gruyter, 1984], 111-2, 139-40, as endorsed and amplified by Lipiński, *Itineraria Phoenicia* [Leuven-Paris-Dudley, Peeters, 2004], 248-52 at 248-9). In his later contribution, which is a very broad, and imaginative, reconstruction of the Mediterranean networks in which the details are allowed but little place, Gordon, 190, did not deign to admit that the toponym *Taršiš*, still an enigma in 1978, had been identified in the meantime. Griffith, whose philological blunders as put right *supra* demonstrate either a hurried perusal of Gordon 1978 or, and more probably, a dependence on Gordon's just-mentioned "The Mediterranean Synthesis" (in W. A. Ward - M. S. Joukowski (edd.), *The Crisis Years. The 12th Century B.C. from Beyond the Danube to the Tigris* [Dubuque, Iowa, Kendall/Hunt, 1992], 188-96), was accordingly not informed that the parameters of the linguistic-etymological game he was playing around *שִׁשְׁבִּיט* in the footsteps of Gordon had changed.

<sup>150</sup> He might be excused by the tone of utter confidence Gordon's initial exposition of his idea was couched in: "The Wine-Dark Sea", *Journal of Near Eastern Studies* 37, 1978, 51-2. However, a critical scholar has to cry havoc when faced with the assimilation of *שִׁשְׁבִּיט* and *οἴνοψ* from which Gordon postulates *√TRŠ*: «Homer applies to the Sea the epithet *oinops* which is commonly rendered "wine-dark" in English translations. There is a Hebrew passage in which "dark" is applied to the color of wine (Gen. 49:12): *hkllyl ynyym myxn wlbw šnyym mhlb*, "dark of eyes from wine and white of teeth from milk" (or alternatively, "darker of eyes than wine and whiter of teeth than milk"). The Akkadian cognate of *hkl* is *ekēlu*, "to be dark"» (51-2). Lest I be accused of scholarly cruelty, let me repeat that whenever one endorses the material of a predecessor, one needs to control it, particularly if such material is arcane; otherwise, one is merely parroting that forerunner. I am afraid most of Griffith's comparative output showcases this defect and do not deserve to be praised in any capacity, witness his virtual absence in Rosól, *Frühe Semitische Lehnwörter...*

<sup>151</sup> In the case of ὄρχαμος, *al'iy qrdm* and *hzw.ti nfr* are likely to be genetically related: it has been suggested, with cogent evidence, that the perplexing Egyptian source was fashioned after an Ugaritic composition about Baal (T. Schneider, "Innovation in Literature on Behalf of Politics: The Tale of the Two Brothers, Ugarit, and 19<sup>th</sup> Dynasty History", *Ägypten und Levante* 18, 2008, 315-26, particularly 317-9); much light has been shed on the *interpretationes Aegyptiacae* of Baal in Ramesside Egypt by the same scholar in "Texte über den syrischen Wettergot aus Ägypten", *Ugarit-Forschungen* 35, 2003, 605-27, where he first adumbrated the linkage of the Tale to Ugarit (for Baal being understood qua Seth from the Middle Kingdom onwards see "Texte...", 601-10).

in attempts at theological or cultic comparatism<sup>152</sup>, the literary core of the deities between whom a parallel is drawn should never be lost sight of, lest the cross-cultural process degenerates into infecund bowdlerization<sup>153</sup>. The sun god of the Greeks, whether Helios or (post-)classical Apollo, ill-resembles the one of the Egyptians, who is born of the sky goddess Nut every morning (the earliest iconographical attestations of this motif do not antedate the Nineteenth Dynasty, but we know it to be much older: Assmann, *Liturgische Lieder an den Sonnengott. Untersuchungen zur altägyptischen Hymnik* [Berlin, Hessling, 1969], 120-2). Now many gods, including Horus, underwent a solarization process: G. Griffiths, *Plutarch's De Iside et Osiride*, 497-8, for Osiris and the sun, with J. C. Darnell, *The Enigmatic Netherworld Books of the Solar-Osirian Unity. Cryptographic Compositions in the Tombs of Tutankhamun, Ramesses VI and Ramesses IX* (Fribourg-Göttingen, Academic Press / Vandenhöck & Ruprecht, 2003), 452-3, for the preference given to the solarized Osiris over the mummified one by certain deceased; C. Graindorge-Hérel, *Le dieu Sokar à Thèbes au Nouvel Empire* (Wiesbaden, Harrassowitz, 1994), 344-82; Darnell, 305-8, particularly 307 sqq., for the solar eye / eye of Horus, alongside Lesko, "The Field of Hetep in Egyptian Coffin Texts", *Journal of the American Research Center in Egypt* 9, 1971-1972, 89-101 at 93-4, for Coffin Text, V 465 §§348b-352e, and Minas-Nerpel, *Der Gott Chepri*, 446-8, for Horus as against Khepri. This whole current is totally alien to the Greek and Roman religious mindset: only Apollo might be argued to have passed through a magnification of his solar aspects, whether or not this continues a putative, original trait of his (e.g. H. S. Versnel, *Inconsistencies in Greek and Roman Religion, Vol. II Transition and Reversal in Myth and Ritual* [Leiden-New York-Köln, 1993], 290-292, especially 292 note

<sup>152</sup> Judicious interpreters of Greek religion rightly tend to discard such lore (R. G. Edmonds III, "Tearing Apart the Zagreus Myth: A Few Disparaging Remarks on Orphism and Original Sin", *Classical Antiquity* 18, 1999, 35-73 at 50-1; etc) or decline to comment on it (A. Bernabé, "The Derveni Theology: Many Questions and Some Answers", *Harvard Studies in Classical Philology* 103, 2007, 99-133 at 108 and note 32; etc). Assyriologists who labor in the religious field equally abide by far more rigorous Arbeitsmethode: e.g. F. Bruschweiler, *Inanna, la déesse triomphante et vaincue dans la cosmologie sumérienne. Recherche lexicographique* (Leuven, Peeters, 1984), 9-14; Fritz, '...und weinten um Tammuz', 47-54, particularly 53-4.

<sup>153</sup> «Il est nécessaire de tabler sur des matériaux formant système et d'éviter de se livrer à des collages hasardeux de traditions. (...) En outre, nous devons, me semble-t-il, nous interdire d'aligner des ressemblances approximatives, anisomorphiques, ou trop vagues de séquences qu'un récit indien pourrait présenter avec un mythe grec (...). La comparaison ne peut aboutir à diluer la singularité (...) ou à l'affadir (...)» (Pirart, *La naissance d'Indra*, 64). G.-R. Vincent, "La poursuite de Jayadratha par Arjuna (*Mahābhārata*, VII, 32-121) vaut-elle pour celle d'Hector par Achille (*Iliade*, XX à XXII)?" *Gaia* 11, 2007, 131-73 at 158-72, accedes to this creed; the numerous parallels drawn by M. Mazoyer between the myths of Apollo and Telepinu do not (*Télépinu, le dieu au marécage. Essai sur les mythes fondateurs du royaume hitite* [Paris, L'Harmattan, 2003], 116-7, 119-20, 131, 156-8, 200-1, 214).

9); from the Archaic period onward, the sun is merely the highest point of comparison for human achievements, as evinced by the choral lyric and melic poetry (M. Puelma, “Die Selbstbeschreibung des Chores im Alkmans grossem Partheneion-Fragment”, *Museum Helveticum* 34, 1977, 1-55 at 7-19); a new, enduring twist on this solar symbolism appears in the Hellenistic era, when it fusions with the symbolism of the stars to become a crucial part of ruler ideology (S. Weinstock, *Divus Julius* [Oxford, Clarendon, 1971], 375-384; etc). Thus solarization widens to the dimensions of an unbridgeable chasm the divide we have seen between the Greek and Egyptian notions about the deified sun; this dissuades one of putting any credence in cross-cultural interminglings of myths or cultic devices no matter how good they might seem. Unless, of course, one is prepared to discard the irredeemably discrepant mindsets of the Greek and Levantine religions; come what may, I cannot imagine anyone taking that quantum leap.

All considered, though, the avenue of research most likely to raise to a higher plane the debate on the recognition of Egyptian patterns in the Greek literature would be the investigation of narrowly defined topics encompassing a restricted chronological range. This trend has already produced two monuments: one quite successful, Lazaridis’ *Wisdom in Loose Form. The Language of Egyptian and Greek Proverbs in Collections of the Hellenistic and Roman Periods* (Leiden-Boston, Brill, 2007), the other inadequate, A.-A. Maravelia’s *Les astres dans les textes religieux en Égypte antique et dans les Hymnes orphiques* (Oxford, Archeopress, 2006)<sup>154</sup>. The latter work rightly did not deem it superfluous to lay bare

<sup>154</sup> For a most impressive compendium of philology and ‘hard’ science, it is unfortunate that the details have been left to the reader to ponder and that the book merely achieves a compilation of all primary data intermingled with sparse interpretive comments. Far too much Egyptian lexicography is assumed, as in tables III.1 (92-121) and III.3 (139-209), which reproduce in the author’s own meticulous transliteration the utterances of the Pyramid and Coffin Texts relating to astronomy / cosmology: an exhausting tool for sure, it cannot be used directly since no translation has been provided (Greek and Egyptian are very seldom translated in the whole opus, with bizarre exceptions on 327-8 [Pyramid Texts of Wenis] and 365 [the Cosmic Egg]). Not only is the Egyptian section (89-304) thus made less reader-friendly than it should have been; Maravelia constantly misinterprets these tables, and the frequency lists she constructs from them, for an analysis of the scientific impact on literary texts (e.g., 274: «à partir de notre étude (voir Tables III.9 & III.10), il est évident que les éléments astronomiques de la première catégorie (jour et lumière, nuit et obscurité, &c.) sont assez nombreux», cf. 265, 266, 277, 278). The Orphic chapter looks like a hurried addition, being three times shorter and almost desultory in character. Under astronomical headings (the sun, the moon, the planets, etc), some Hymns are printed in Greek with illustrative footnotes before general conclusions are drawn (305-50). Obviously, these texts were far less important in themselves than as fodder for the, equally brief, comparison with Egyptian texts which follows (350-76). There Maravelia reproduces, in hieroglyphs and annotated transliteration, a solar hymn from Any’s Book of the Dead, 358-62, before one page of global commentary whose few references to the Greek thought are outdated (for the theology of Greek philosophers all she cites is Jaeger’s 1947 classic: p. 363 note 234), then a table canvassing the technical vocabulary of the hymn is adduced (362-4).



the idiocy of the Afrocentric valorisations of Egyptian astronomy (§ 1.6 ‘La “Science” Égyptienne: Une Croyance Arbitraire (voire “Désir Pieux”) de l’Afrocentrisme”, 409-10<sup>155</sup>). Yet I am not sure researches of this kind should become a fad, even if they lead straight to promoting junior scholars equally fluent in both languages. First, there is a dearth of ancient testimonies pointing out to an interplay between Greece and Egypt on those topics. When such evidence exists, it merely pertains to the background. So the tendency of the biographical materials to have classical and early Hellenistic artists or thinkers stay for a while in Egypt; actually, far from standing up to informed scrutiny, it amounts to nothing deeper than a formulaic theme (tabulated in M. Kivilo, *Early Greek Poets’ Lives. The Shaping of the Tradition* [Leiden-Boston, Brill, 2010], 227-31) informed by the desire of the Hellenistic Greeks of Alexandria to root facets of the culture they were heirs to in the prestigious mainstream of Egyptian learning (Lefkowitz, ‘Visits to Egypt in the Biographical Tradition’, in M. Erler and S. Schorn (edd.), *Die Griechische Biographie in Hellenistischer Zeit* [Berlin-New York, De Gruyter, 2007], 101-113). Additionnally, trying too hard to push uncertain or convoluted arguments on the basis of surface resemblance is what bedeviled comparative studies for the whole century, getting us in hot water with Astour, Gordon, and Bernal; we hardly need more research where apparent commonalities are never actually demonstrated not to be bogus or random since the contentions these shared traits serve to evidence hardly admit of proof. Further work along those lines can only serve to divert our attention from more pressing needs<sup>156</sup>. Moreover, apart from exceptionally

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Basing herself on this thin dossier, she declares that «on a essayé d’analyser comparativement deux hymnes anciens adressés au dieu solaire Hélios / Rē. Tous les deux montrent l’existence d’une pensée astronomique latente qui était plus développée dans l’hymne orphique au soleil (...)» (363). As her claim, pp. 375-6, that the Orphic texts reflect astronomical conceptions from the second millenium rests on a paper-thin basis and, for the time being, must be pronounced a figment of her imagination, I cannot grant her that she gave the evidence the ‘regard frais et interdisciplinaire’, let alone the ‘comparaisons de deux distinctes voies de pensée anciennes’, touted in the introduction (p. 12).

<sup>155</sup> «Si nous nous demandons quelle est la définition correcte du terme Science, dans l’interprétation rigoureuse du terme, comme il est utilisé aujourd’hui, nous allons comprendre que de ce point de vue l’on ne peut trouver le moindre élément ou notion scientifique dans aucun texte égyptien de la période pharaonique, voire dans les *PT*, les *CT* et le *B(ook of)D(ead)*. Il est vain de chercher pour eux ou de créer et imaginer des éléments scientifiques, comme le font les afrocentristes en interprétant le mythe comme une réalité certaine. (...) Il faut signaler que le mythe (bien qu’il puisse véhiculer indirectement des réalités historiques et des phénomènes physiques) est tout à fait différent de la Science» (p. 410, cf. 7-8). Read S. Howe, *Afrocentrism. Mythical Pasts and Imagined Homes* (London-New York, Verso, 1998), 259-64; J. Shavit, *History in Black. African-Americans in Search of an Ancient Past* (London, Cass, 2001), 129-38; Slack, 165-76; T. Adlake, *The Case Against Afrocentrism* (Jackson, University Press of Mississippi, 2009), 90-1, 93; and compare Burkert, *Weisheit und Wissenschaft*, 384-5 = *Lore and Science*, 407.

<sup>156</sup> What we need most are Hellenists with a grasp of cuneiform. As a sample of the work

favorable cases-in-point (Herodotus' Egyptian *logos*, Plutarch's *De Iside et Osiride*; contrast, say, Aeschylus' *Supplices* or the Egyptian allusions in Plato, the former virtually void of *couleur locale*<sup>157</sup>, the latter evincing no

left to do, cf. the semantic family of μισεῖν: a non-PIE enigma, it is explained by Bernal (303) from the Egyptian *msđi*, 'to dislike, hate, detest', which he supposes to «provide a perfect semantic correspondence and a reasonable phonetic one». Less arbitrarily, Akkadian preserves the dental-free *muhhušu*, D form of the verb *maḥāšu* (logographic sig [PA], RA: Borger, *Mesopotamisches Zeichenlexikon* [Münster, Ugarit, 2003], n<sup>o</sup>464, 511, pp. 333, 356, adding S. M. Maul, 'Herzberuhigungsklagen'. *Die sumerisch-akkadischen Eršahunga-Gebete* [Wiesbaden, Harrassowitz, 1988], 259 at 4f, for RA = *maḥāšu*); *muhhušu* conveys the meanings 'to hurt, hit with a vengeance, smash, destroy, kill' (*CAD M Part 1* [1977], 82-83 n<sup>o</sup>7; *AHW.*, II, 581), so coheres well with the rejection of someone or something as yesterday's garbage that constitutes the root value of μισεῖν. More phonetically distant, yet worth mentioning, is the Gt form of *maḥāšu* (more occurrences of this verb in volumes I, III, and IV of the *States Archives of Assyria* series, respectively pp. 219, 139, and 339), *mithušu*, literally 'to fight with each other', viz. 'to do battle, go to war, engage in combat' (*CAD*, 81-2 n<sup>o</sup>5; *AHW.*, II, 580; S. Lackenbacher, *Textes akkadiens d'Ugarit. Textes provenant des vingt-cinq premières campagnes* [Paris, Cerf, 2002], 80 note 218). What is more, μισητός and μισημα can be compared to the substantives *mithušu*, 'fight, combat, attack, assault, onslaught' (*CAD M Part 2* [1977], 138-9; *AHW.*, II, 662-3), and *mithurtu*. From its original adversative value 'conflict, contrast, clash, opposition', the latter came to signify 'balance, correspondence, harmony' (*CAD M Part 2*, 137-8; *AHW.*, II, 662), per se but particularly within the formulas *saniqu mithurti*, 'what brings opposite things together', 'matching one another' (M.-J. Seux, *Hymnes et prières aux dieux de Babylonie et d'Assyrie* [Paris, Cerf, 1976], 529-30 note 14), and *lišān mithurti* (translating *eme-ḥa-m-un*), whose meaning is a crux (from T. Jacobsen, *Towards the Image of Tammuz and Other Essays on Mesopotamian History and Culture* [Cambridge, Mass., Harvard University Press, 1970], 365 note 32 *ad finem*, to Hallo, *Origins*, 154-5; etc). Direct knowledge of Akkadian would be helpful to Egyptologists, too; in his analysis of the late *mzđi.w* (/madi.ou/) attested only in the plural ('bearers of offerings [?]', Takács, III, 129-30, utterly fails to adduce the Ugaritic *md* (an Akkadian loanword for a class of skilled workers: Del Olmo Lete-Sanmartín, II, 524) and its original, *mūdū*. For a discussion of the latter, Lackenbacher, 239 note 815: there were those who stuck to 'courtier' (e.g. J. J. Finkelstein, "On Some Recent Studies in Cuneiform Law", *Journal of the American Oriental Society* 90, 1970, 243-56 at 253-4), adducing the frequent *mūdū šarri / šarati*, 'm. of the king / queen' (J. D. Schloen, *The House of the Father as Fact and Symbol. Patrimonialism in Ugarit and the Ancient Near East* [Winona Lake, Eisenbrauns, 2001], 243, for figures), but most recent authorities maintain that 'knowing, expert, competent, well-tested, experienced, wise (in a craft)', thus 'acquaintance' (of the royals), is preferable (Seux, *Épithètes royales akkadiennes et sumériennes* [Paris, Letouzey et Ané, 1967], 168-70; *CAD M Part 2*, 163-7; *CDA*<sup>2</sup>, 214; Durand, *Les documents épistolaires du palais de Mari*, I, 285, III, 193-4: 'au courant').

<sup>157</sup> As opposed to the ideological construction of Egyptian characters and mores in this tragedy (Vasunia, *Gift of the Nile*, 40-58). I have in mind the general ambiance and the rarity of Egyptianizing details: let me mention Apis and the plain named after him (vv. 260-70; cf. G. Griffiths, "Lycophron on Io and Isis", *Classical Quarterly* 36, 1986, 472-7 at 475); the possible identification of Epaphos with Apis in the corrupt v. 41 (E. W. Whittle, "Two Notes on Aeschylus, *Supplices*", *ibid.* 14, 1964, 24-31 at 26; H. Friis-Johansen – E. W. Whittle, *Aeschylus. The Suppliants* [Copenhagen, Nordisk Forlag, 1980], II, 39-40; C. W. Willink, "The Invocations of Epaphus in Aeschylus, *Supplices* 40-57 and Euripides, *Phoenissae* 676-89", *Mnemosyne* 55, 2002, 711-9 at 713, cf. 715-6); or the ornithological detail of v. 212 καὶ Ζηνὸς ὄρνιν τόνδε νῦν κικλήσκετε (Friis-Johansen and Whittle, II, 170-2, take this to be an allusion to the solar hawk of Amun-Re mistaken by the poet for the eagle of Zeus and reject

independent awareness of what makes factual comparability between Greece and Egypt<sup>158</sup>), the law of diminishing returns applies implacably to Graeco-

the conjecture ἴτιν — they have persuaded West and A. H. Sommerstein in the new Loeb [Cambridge, Mass.-London, Harvard University Press, 2008], I, 37).

<sup>158</sup> The extent of Plato's direct awareness of Egypt is hard to assess; Mathieu, "Le voyage de Platon en Égypte", *Annales du Service des Antiquités d'Égypte* 71, 1987, 153-67, merely builds a house of cards which is not strengthened by speculations on the late sage Petese (so, rightly, K. Ryholt, *The Petese Stories II (P. Petese II)* [Copenhagen, Museum Tusulanum, 2005], 13-6), thus *caveat lector*. In at least one instance — Solon's Egyptian sojourn —, Plato's knowledge would appear to derive from Greek sources, namely Herodotus (G. Griffiths, 'Atlantis and Egypt', 4), not from Solon himself or through autopsy as is acknowledged even by H. Görgemanns, "Wahrheit und Fiktion in Platons Atlantis-Erzählung", *Hermes* 128, 2000, 405-19 at 417 (a scholar for whom «die interpretierenden Überlegungen haben uns zu einer allgemeinen Annahme über den Realitätsbezug der Atlantis-Erzählung geführt: Platon hat eine Überlieferung über historische Fakten benutzt, diese allerdings erzählerisch ausgestaltet» [p. 412]; for G. Griffiths, on the contrary, «the story certainly does not derive from Egypt *in toto*. Diverse sources and materials have been used, and the process is patently the construction of a pastiche» [p. 27]). Plato depends on Herodotus too for the story of the ring of Gyges (*Republic*, II, 359 c 7-360 b 3), a narrative he might very well have invented for the sake of the ethical points he was concerned with making; A. Laird, "Ringing the Changes on Gyges: Philosophy and the Formation of Fiction in Plato's *Republic*", *Journal of Hellenic Studies* 121, 2001, 12-29 at 13-23. Keeping in mind Plato's use of sources and his love of fictional narratives, all we can say with confidence on the topic of his allusions to Egypt is that the philosopher approved on very personal grounds of such-and-such a trait of (Late) Egyptian society or culture (W. M. Davies, "Plato on Egyptian Art", *Journal of Egyptian Archaeology* 65, 1979, 121-7, who begins in a suitably skeptical fashion [121-2] before piling hypothesis on hypothesis [123 §2] in support of the relevance to Plato of the view that [123 §1] «even if Greek experience in the sculptural 'factories' of Egypt is denied, of course, the Greeks could have procured considerable experience of Egyptian art by indirect means», helps little; rather Froidefond, *Mirage...*, 326-337, and N. Demand, "Plato and the Painters", *Phoenix* 29, 1975, 1-20 at 2, 19) whereas Plato no less individually frowned upon other Egyptian customs (mostly the use of writing; Baines, *Visual and Written Culture in Ancient Egypt* [Oxford-New York, Oxford University Press, 2009], 43-4, 108; Baumgarten, *Heiliges Wort...*, 171-81). The heart of the matter is thus a question of sources and authorial intent (cf. H. Joly, "Platon égyptologue", *Revue philosophique de la France et de l'étranger* 172, 1982, 255-66 — «Teuth, Saïs, Isis opèrent bien, par le langage et les images, comme des miroirs égyptiens, mais ce sont surtout les réalités grecques et la philosophie platonicienne qu'ils reflètent» [p. 65] —, and L. Brisson, "L'Égypte de Platon", *Les Études philosophiques* 2/3, 1989, 153-68 — speaking of his 'attitude (...) particulièrement ambiguë', he states that «l'Égypte lui permet avant tout de définir, en un double mouvement d'admiration et de répulsion, ce que devrait être l'Athènes de son temps» [p. 167]). We must leave aside all postmodern readings of historiography and their suppression of the criteria of truth in favor of supposedly more sophisticated models; so, e.g., D. Lenfant, *Ctésias de Cnide* (Paris, Les Belles Lettres, 2004), p. CLXXI: «au fil des siècles, la grande question est restée celle de la *fides* de Ctésias et de la *dichotomie mensonge-vérité dans laquelle on s'est enfermé depuis Aristote*» (emphasis mine). A creative mind not particularly concerned with ascertaining bare facts as he strives to reach higher levels of truth, Plato passes down mediated views of Egypt which he transforms with embarrassing freedom. (Aristotle and his school certainly went further, if A. I. Ivantchik was right to postulate that their use of Sesostriis pertook of attempts at a blending of Egyptian and Zoroastrian ideas: "Eine griechische Pseudo-Historie. Der Pharao Sesostriis und der skytho-ägyptische Krieg", *Historia* 48, 1999, 395-411 at 409-11; he asks himself whether this was already a conception of Plato's). I regret having to rehearse at such length things so

Egyptian inquiries. They must demonstrate that they have a point instead of proceeding as if they stood on stable ground and going their merry way. For no amount of minute achievement balances a skewed larger picture. The tendency towards accumulation of fact, let alone the outright exhaustivity of citation or reference in handling the ancient evidence, conspicuous in *Les astres dans les textes religieux...* and in Griffith's *Mummy Wheat*, tends to prevent any vigorous consideration of the value of the sources marshaled (I mean their pedigree, animus, scholarly exploitation, which are normally assessed before a fair-minded scholar uses these). Such a proliferation of the primary evidence kept more or less isolated from its scholarly interpretations circumvents *de facto* any criticism of the methodology to be adopted in working out a great variety of ancient texts — a neat trick in monographs whose guiding star is a wholly hypothetical view of the Graeco-Egyptian interplay. Unfortunately, it does violence to sound method; it shall not be encouraged by any means. Considering all of this, I find it impossible to tell what there is to gain if one works à la Lazaridis in a specific segment of the Greek and Egyptian literatures, virgin soil or not<sup>159</sup>; adding a few grains of learning to our stores would in no manner be a contemptible achievement, even if this outcome of book-length inquiries looks anticlimactic. It is far less tolerable, for the potential writers of such works, to misconstrue the so-called results they claim to have reached as so many proofs of a cultural impact of the older civilization on the younger one<sup>160</sup>; for who will be able to tell whether

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simple, yet easily overlooked.

<sup>159</sup> Partial allowance might be made for the concepts of allegory, on which fresh scrutiny of the Egyptian materials according to the sharpest reading grids (e.g., R. B. Parkinson's: *Poetry and Culture in Middle Egyptian Kingdom. A Dark Side to Perfection* [London - New York, Continuum, 2002], 290; *Reading Ancient Egyptian Poetry. Among Other Histories* [Chichester, Wiley-Blackwell, 2009], XI-XIV) could hope to reap some benefits. Yet, so far as I am aware, little in the abundant secondary literature on the allegorical interpretations of Homer (from Buffière to D. Dawson through J. Pépin, to mention only a few landmarks) suggests any proximity to the Egyptian lore; and, other than Plutarch's symbolism in the *De Iside et Osiride*, the Greek evidence hardly looks ample enough to warrant a book-length inquiry. It was marshaled by G. Griffiths, "Allegory in Greece and Egypt", *Journal of Egyptian Archaeology* 53, 1967, 79-102 at 83-99; his survey of possible cross-cultural affiliations concludes that «although Egyptian religion supplied the fundamental data by which Plutarch allegorized the Osiris-myth, the ultimate process is here a Greek achievement. Yet the fact remains that the use of allegory in both its forms originated in Egypt» (p. 102; cf. *Plutarch's De Iside et Osiride*, 100-5 *passim*, with D. Babut, *Plutarque et le stoïcisme* [Paris, P.U.F., 1969], 368-88, and Dawson, *Allegorical Readers and Cultural Revision in Ancient Alexandria* [Berkeley-Los Angeles-Oxford, University of California Press, 1992], 58-66, for Plutarch's allegorism).

<sup>160</sup> Every cross-cultural writer should keep as cool a head on their shoulders as S. L. Budin in her account (*Classical Review* 62, 2012, 345-7) of Louden, *Homer's Odyssey and the Near East* (Cambridge, Cambridge University Press, 2011), a book as ill-informed of the Levant as it is tendentious (*Aristarchus antibarbarus*, LVII-LVIII, LXVIII-LXXV). Exemplary too is Lazaridis, cf. 1-3. «The ancient Egyptian and Greek producers of the proverbial wisdom studied

or not a fascination for Egypt mired in nothing sounder than the myth of the 'stolen legacy' was the unspoken assertion behind their work? Such is the nature of Afrocentric concerns world-wide that even tame statements are incapable of not being twisted into the confirmation that Africa was ransacked by the Greeks, and through them the Western civilization. So I take issue at Maravelia's «nous constatons qu'à la fois Égyptiens et Hellènes dotèrent le monde de l'esprit, la mythologie archétypique et la science avec des dons uniques et valables» (p. 442).

Despite Bernal, his adepts, and their fanbase, the whole of the *Ægypto-Graeca* is a continuous stream from Solon down to Heliodorus; it has its highlights which repeatedly attracted scholarly attention, but does not lend itself to clear-cut divisions and broad generalizations, nor can it be mistaken to include the Homeric epics or any early poetry. The *Black Athena* posse has also been prevailed upon to follow the easy course of ignoring the unassailable, if scant and generally arcane, remains of the feelings of awe the Greeks entertained for Mesopotamia: in their eyes, the 'Assyrians' and

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here seem to have chosen to use a form of expression common to their cultures, following its rules and challenges, but also making use of the space and liberties it offered for adding pieces of their own cultural experience. In general, I do not believe that the structural similarities identified could have been products of direct inter-cultural communication for the following reasons: firstly, the fact that most of the forms of proverbial expression employed at this stage also existed in the Egyptian and Greek literatures produced in the era before the Hellenistic and Roman period; secondly, the absence of any interest in the other's grammar and syntax (since, even in the case of translations (...), no demotic grammatical constructions found their way into Greek writing and vice versa), and, thirdly, the absence of any major direct translations from each other's body of proverbs. (...) In conclusion, comments such as "the Egyptian material was influenced by the Greek proverbs" or "the Greek writers copied their Egyptian colleagues" cannot reflect the actual complexity of the problem» (241-2). We have known for decades how little Demotic was influenced by Greek, apart from proper names (see J. D. Ray, "How Demotic Is Demotic?", *Egitto e Vicino Oriente* 17, 1994, 251-64 in general, and the onomastic indices and discussion of Clarysse - G. van der Veken - S. P. Vleeming, *The Eponymous Priests of Ptolemaic Egypt. Chronological Lists of the Priests of Alexandria and Ptolemais with a Study of the Demotic Transcriptions of their Names* [Leiden, Brill, 1983], 72-117, 133-65); so Lazaridis' conclusion is hardly of such a nature as to cause uneasiness. Contrast now Griffith's certainties ('Sailing to Elysium...', 234): «to conclude, we have seen that the river, ship, and ferry-man whom dead Egyptians met on their way to the Reed-Field left their mark on the *Odyssey* and that Menelaus comes thither thanks to Zeus' anxiety over a specifically matrilineal succession better attested in Tuthmosid Egypt than in Greece itself. We have also seen that there is a reason, quite unlike those that let bards adopt these ideas, that kept them alive into the seventh century. These facts make it yet more likely that the nub of Egyptian funerary myth, the Field of Reeds, appears therein as Elysium». The contrast between the conclusions of Lazaridis and Griffith is not merely painful; it makes it clear how anxieties of influence discriminate the ideologues (those who channel most crudely their Egyptomania through such anxieties: Griffith, Maravelia) from mainstream researchers, who are apt to tell these bouts apart, control them, and attempt probative syntheses (G. Griffiths, Lloyd, Lazaridis). I would take a leap a faith here; all scholars should question their procedures who tout sensational findings of their own in a field which is both barren and hugely difficult to plow comprehensively in an ideology-free fashion — unless I am gravely deluded.



'Chaldaeans' were enviably great too, like the Egyptians<sup>161</sup>. It will not do to explain out this duality of sentiments through competitive plausibility, that is, through an instance of 'either...or' reasoning<sup>162</sup>. It is not even a matter of

<sup>161</sup> On this prestige of the Assyrians, J. M. Dillon, *Iamblichus Chalcidensis in Platonis Dialogos Commentariorum Fragmenta* (Leiden, Brill, 1973), 274-5, quotes and discusses a few striking texts with early classical roots in his treatment of Iamblichus' *On the Timaeus*, I, fr. 11 (itself well worth of mention: Ἀσσύριοι δέ, φησὶν Ἰάμβλιχος, οὐχ ἑπτὰ καὶ εἴκοσι μυριάδας ἐτῶν μόνας ἐτήρησαν, ὡς φησὶν Ἰππάρχος, ἀλλὰ καὶ ὅλας ἀποκαταστάσεις καὶ περιόδους τῶν ἑπτὰ κοσμοκρατόρων μνήμη παρέδωσαν πολλοῦ ἄρα δεῖ πρὸς ταῦτα παραβάλλεσθαι ἢ φαρ' Ἑλλησι πολυθρύλητος ἀρχαιολογία). For the Greeks and the Χαλδαῖοι (*proprio sensu* 'Iranians', but rather 'mages' or 'astronomers'), beyond, say, A. Momigliano, *Alien Wisdom. The Limits of Hellenization* (Cambridge, Cambridge University Press, 1971), 123-49, particularly 141-8, and the speculative work of Kingsley in the 1990s, e.g. "The Greek Origin of the Sixth-Century Dating of Zoroaster", *Bulletin of the School of Oriental and African Studies*, University of London 53, 1990, 245-65 at 252-4, the main evidence ranges from Herodotus down to Agathias (A. de Jong, *Traditions of the Magi. Zoroastrianism in Greek and Latin Literature* [Leiden-New York-Köln, Brill, 1997], 76-250), which qualifies the hostility of the Greeks towards Persia. Read further Cook, "Near Eastern Sources for the Palace of Alkinoos", *American Journal of Archaeology* 108, 2004, 43-77, for the conviction that the Neo-Assyrian culture impacted the *Odyssey* (44 note 5, on the relationships between Assyrians and Early Greeks), and Rollinger's proof that the adoption of the vocables Συρία and Ἀσσύρια ought to be dated to the eighth century B.C. and placed into the multicultural, Assyrian-fearing milieu of southern Anatolia or northern Syria wherein Greeks interacted with Luwians, Phoenicians, and Aramaeans ("The Terms 'Assyria' and 'Syria' Again", *Journal of Near Eastern Studies* 65, 2006, 283-7). A vital issue for the quite high regard in which some Greeks held Ἀσσύρια also escaped Bernal's notice: «universal historians of Greece and Rome made some effort to assign the Assyrians their proper place in world history. No similar effort was made on behalf of the Egyptians. Ephorus, Trogus, Nicolaus and Cephalion seem to have done nothing with Egyptian history. Diodorus had much to say on Egyptian geography, fauna, and customs, but precious little on its history. Yet Egypt was a fabled country, with its own unbroken tradition of the past, and was visited by countless Greeks and Romans. Assyria was more remote in time and space. The inclusion of Assyria in the universal histories can only be explained by the influence of Assyrian history upon the evolution of Greek views about pre-Trojan times, and upon the evolution and consolidation of a chronology for that period» (R. Drews, "Assyria in Classical Universal Histories", *Historia* 14, 1965, 129-42 at 137-8). Interest in what had happened in the western regions of Mesopotamia during the earliest ages of Greece was part of the trade of the global historian from the Hellenistic period on, whereas Egypt could safely be relegated to the margins, as a endless source of mirabilia — so much for an 'Ancient Model'...

<sup>162</sup> Bernal, *Black Athena Writes Back*, 295: «relating all cultural influences to ceramic design and restricting such influence to the seventh century has also had a number of *ideological advantages*. First, it enabled the establishment of the Olympic Games and the formation of the polis in the early eighth century to be seen as purely Hellenic developments. Second, *it located in Assyria the first "Orient" encountered by Greeks*. The Assyrian Empire fits remarkably well with the stereotype of Oriental despotism and, thus, *diverts attention from the Phoenicians*, the foreigners with whom Greece had had most contact in the tenth, ninth, and eighth centuries, and whose city states looked disconcertingly similar to those developing in Greece toward the end of that period» (emphasis mine). On the fallacy of equating without further ado foreign city-states with πόλεις, which betrays once more an ignorance of the extent to which the Greeks were fond of reading their own structures into other cultures, cf. M. H. Hansen - T. H. Nielsen, *An Inventory of Archaic and Classical Poleis* (Oxford-New

weighing the authority and bias of the various sources for these two mindsets. For they thrived side by side<sup>163</sup> in defiance of an all-prevailing, Egyptocentric, ‘Ancient Model’ whose existence outside of Bernal’s mind is actually little short of bogus. Berlinerblau and Levine and Rankine and McCoskey erred so spectacularly in their assessment of it — one thus feels bound to speculate that none but the hard-nosed pundit is equipped to unravel any segment of the *Aegypto-Graeca* free from the Afrocentric spell. This continuum ought to be explored by philologically robust historians endowed with strong cultural flair, whether Herodotean scholars, Lagid experts, or students of the (more or less heavily Egyptianized) magical and Hermetic corpora<sup>164</sup>. I also count among them those who, having made themselves at home in the ‘Oriental’ cults of the Roman empire, corroborate G. Griffiths’s conclusion that Plutarch and Apuleius are tolerably exact storehouses of Egyptian religious data<sup>165</sup>.

In light of the above, let me restate my main tenets. Egypt never was the prime linguistic, scriptural, technical, or religious influence over Greece during the (Late) Bronze Age and Early Classical period; that role belongs to Mesopotamia — with Anatolia a distant second<sup>166</sup>. The construction and

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York, Oxford University Press, 2004), 36–7, not without the caution advocated by E. C. L. van der Vliet, “Reflections on the polis and poleis”, *Mnemosyne* 60, 2007, 302–16 at 307, with respect to the issues raised in this repertory by the elimination or the classification amid the πόλεις of any given ‘Barbarian’ city mentioned by the Greeks; see also Hansen, *Polis. An Introduction to the Ancient Greek City-State* (Oxford-New York, Oxford University Press, 2006), 33–4, 38.

<sup>163</sup> After he has discussed the programmatic passage Herodotus 1.1–5, Luraghi states that «the contrast between Persians, Egyptians, and Scythians, in terms of being λόγιοι, appears to reflect a more general contrast between the level of competence and quality of information characteristic of these three peoples, and also, more broadly, their level of civilization. If we compare Egypt and the Black Sea, the contrast is particularly obvious in the enormous depth of the Egyptian past as opposed to the nonexistence of an ancient history of the Scythians. As for the Persians, they turn out to be more loquacious than reliable, as shown by Herodotus’ repeated references to multiple Persian versions of key episodes of Achaemenid history» (‘Being λόγιος’, 445). The cultural superiority given to Egypt by the historian (itself a problematic view, but let us discard this point in the present context) is therefore relative and mired in a comparative, a priori superstructure, marked by inclusivity rather than exclusion; Bernal’s crude notion of the indebtedness felt by Herodotus, Plato, and so on makes but a travesty of the relatively sophisticated ethnography of the fifth and fourth centuries B.C.

<sup>164</sup> A sample of such ventures is B. Legras’ prosopographical “Les experts égyptiens à la cour des Ptolémées”, *Revue Historique* 304, 2002, 963–91 (Egyptian evidence at 979–87). I spelled out the Egyptianization of magic since the classical uses of μάγος point to Iran (doxography by Bernabé at the *Derveni Papyrus*, vi 2: *Poetae Epici Graeci*, II.3 [Berlin-New York, De Gruyter, 2007], 196), despite T. Kouromenos – G. M. Parassoglou – Tsantsanoglou, *The Derveni Papyrus* (Florence, Olschki, 2006), 166–8.

<sup>165</sup> Lastly, V. Gasparini, “Isis and Osiris: Demonology vs. Henotheism?”, *Numen* 58, 2011, 697–728 at 698–702; compare, e.g., J. Aliquot, “*Aegyptiaca et Isiaca* de la Phénicie et du Liban aux époques hellénistique et romaine”, *Syria* 81, 2004, 201–28, notably 216–7 for a nuanced discussion of Plutarch (and notice the caveats expressed in the note 76 on 217).

<sup>166</sup> Pace, for instance, *Black Athena Writes Back*, 204: «(...) my concern is not with a

negotiation of the ethnic identity of the main Greek powers pay little heed to the feelings of indebtedness towards Egypt which Bernal reconstructs. The same conclusion obtains for the Greeks' creative engagement with their past called by most scholars the 'invention of tradition'. Hellenes merely labored under the impression that there was a particular kindredship between their culture and that of Late Egypt once they entered the country as mercenaries, then merchants and settlers. Travellers like Herodotus were soon moved to question the historical reliability of the Homeric epics when these ran contrary to Egyptian narratives of 'the truth of what happened', τὴν ἀληθείην τῶν πρηγμάτων<sup>167</sup>. The Greeks made heavy weather of their *interpretationes Graecae* so far as religion and political structures were concerned while borrowing a smattering of their words in the domain of material civilization, but hardly anything from the realm of higher culture. All efforts to enlarge the number of loanwords from the Nile valley to lexemes ill-explained by the common Indo-European stock break down on Bernal's abysmal grasp of the bare bones of linguistics and on his extraordinarily outdated and paltry knowledge of the languages he uses<sup>168</sup>.

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competition between Egypt and Mesopotamia over which was the greater or more creative civilization. I am interested merely in which had the greater impact on the formation of Ancient Greece. Here, archaeological evidence and Greek tradition overwhelmingly prefer Egypt, and this would seem obvious from a geographical point of view». The recoverable Egyptian impact on early Greek poets is neither extensive nor widespread (Koenen, 'Greece...', 14-8) and attempts to find more of it stand and fall on their wild conjectures (as in Rutherford, 'Kala-siris...', 206).

<sup>167</sup> *Enquiry*, II 116-20, best read with L. Kim, *Homer Between History and Fiction in Imperial Greek Literature* (Cambridge, Cambridge University Press, 2010), 30-7. Herodotus goes to the length of claiming that Homer δηλώσας ὡς καὶ τοῦτον ἐπίσταιτο τὸν λόγον (116.1), as he was aware of Alexander's journey to Egypt (δηλοῖ ὅτι ἠπίστατο 116.6). «What Herodotus is looking for is (...) certain elements in the text that are slightly out of place, that do not quite fit, but suggest a correspondence, however tenuous, to the Egyptian version. (...) These may be strained connections, but that is precisely the point; they are not meant to stand independently as convincing proof, only to raise suspicions that there is more going on than meets the eye. (...) Homer has placed them in the text in order that skilled readers like Herodotus, so accustomed to scanning visible objects and *logoi* for traces of the distant past, would notice them and realize that the poet did in fact know the truth about Helen, Egypt, and the Trojan War» (Kim, 37-8). Herodotus was merely spinning a tale on a then-popular theme in Athens. For the tradition of Helen's Egyptian sojourn is firmly posthomeric (not earlier than the Hesiodic fr. 358 Merkelbach-West, itself of dubious authenticity); her presence there for the duration of the Trojan war might possibly be an 'old legend' known to Homer (so F. J. Groten, "Herodotus' Use of Variant Versions", *Phoenix* 17, 1963, 79-87 at 83-7; a 'syncretisme gréco-égypto-phénicien' for Froidefond, *Mirage...*, 67), but both it and the tale of the Helen-shaped εἴδωλον taken to Troy by Paris seem intrinsically unlikely to predate the sixth century, see J. Schwartz, *Pseudo-Hesiodica. Recherches sur la composition, la diffusion et la disparition ancienne d'œuvres attribuées à Hésiode* (Leiden, Brill, 1960), 552-5.

<sup>168</sup> He is only 'apparently proficient in a variety of difficult languages' (Jasanoff-Nussbaum, apud *Black Athena Revisited*, 201), thus error-laden and prone to philological frauds. Consider «a more general sense of 'high' is found in the Aramaic and Syriac *šōmayi* and the Arabic

This conjunction of a silly writer and a queer topic is not enough to condemn the latter as barren; it needs the protracted failure of scholars for this verdict to be iron-clad. Since Gordon, Astour, Bernal, Griffith, busily ransacked the whole Semitic lexicon with no success whatsoever in any of the areas they attempted to cast light upon, Egypt and the alphabetic Levant have no business being a part of the agenda of philologists or Greek lexicographers in the times to come. They will do only if those experts seek to emulate the excellence of Lloyd on Herodotus' second book or Griffiths, on the *De Iside et Osiride* and write equally large commentaries on those compositions. To repack the Egyptomania of the fifth century B.C. à la Haziza and Moyer demands no exceptional aptitude for seeing the wood for the trees<sup>169</sup>; now such a high capacity is indispensable for whoever attempts to examine afresh the writings of Herodotus and Plutarch on Egypt<sup>170</sup>, if he wishes to avoid

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*samā* 'was high, lofty'» (183). This pellucid-looking sentence will mislead a Semitic-free reader into believing that these two Semitic cognates mean one and the same thing; actually Aramaic-Syriac *šamay(y)ā* is a substantive, 'sky, heaven(s)', like Ge'ez *samāy* (Sokoloff, *Dictionary of Jewish-Palestinian Aramaic*, 557; idem, *Syriac Lexicon*, 1572-3; Payne Smith, *Thesaurus Syriacus*, II, coll. 4208-9; Leslau, *Comparative Dictionary of Ge'ez (Classical Ethiopic)* [Wiesbaden, Harrassowitz, 1991], 504), while the Arabic verb *samā* means 'hoch, erhoben sein, hoch emporragen, aufragen; erhaben sein, sich erheben (über); zu stolz sein (für); zu hoch od<er> schwierig sein (für), übersteigen (das Verständnis); aufsteigen (zu, über); höher sein (als); streben, trachten (nach); emporheben, hinaufführen (zu), j-m, e-r S. Aufschwung verleihen' (Wehr, 600-1= Wehr-Colwan, 432, with Corriente, *Dictionary of Andalus Arabic*, 263, under \**√SMW*; Bernal's preference for the past infinitive in his glosses is arbitrary). Needless to say, *šamay(y)ā* embodies no 'more general sense of "high"' than its homonymous cognates: Akkadian *šamū/samū*, *šamā'ū*, *šamāmū* (*CAD* Š.1, 340-6; *AHW.*, III, 1160; W. Horowitz, *Mesopotamian Cosmic Geography* [Winona Lake, Eisenbrauns, 1998], 223-4, 228-33), Ugaritic *šmm* (Del Olmo Lete-Sanmartín, II, 826-7; Rahmouni, *Divine Epithets in the Ugaritic Alphabetic Texts*, 245; Smith-Pitard, *The Ugaritic Baal Cycle II*, 109), Canaanite *šmym* (Hoftijzer-Jongeling, II, 1160-2; Halayqa, 328), Hebrew מַשְׁמָה (*Theological Dictionary of the Old Testament*, XV, 204-36; etc). An intelligent undergraduate equipped with the right dictionaries would do better than Bernal.

<sup>169</sup> Certainly the most difficult task: «les matériaux soigneusement vérifiés s'accumulent à pied d'oeuvre, tandis que l'édifice paraît à peine progresser. C'est sans doute que la tâche des architectes est la plus difficile, et leurs constructions souvent hâtives et provisoires. Mais c'est surtout qu'à force de porter son attention sur le choix des pierres, on n'a plus le temps de les assembler en assises», states G. Rodier at the close of his doxography of Greek philosophy (*Revue de synthèse historique* XIII, 1906, 191-362 at 361). Yet the great linguist V. Henry went so far as to speak of «cette minute suprême de synthèse qui est la récompense et le couronnement d'une vie entière d'analyse» (*Antinomies linguistiques* [Paris, Alcan, 1896], 44); there is no magnificent tree without sturdy roots.

<sup>170</sup> As for Herodotus' Egyptian book all the update we got to Lloyd — both his large commentary and his contribution to the edition of the Lorenzo Valla foundation — has been the fuzzy, theory-obsessed T. Haziza, *Le kaléidoscope herodotéen. Images, imaginaire et représentations de l'Égypte à travers le livre II d'Hérodote* (Paris, Belles Lettres, 2009), cf. *Aristarchus antibarbarus*, 162 note 11, 165-6 (in profound disagreement with Burstein, *Classical Review* 61, 2011, 41-2, over the quality of this attempt to assess the judgementality of fifth-century Greeks and Late Egyptians). No large-scale treatment of the *De Iside et Osiride*

came out since G. Griffiths and Hani (on the relationship of *La religion égyptienne dans la pensée de Plutarque* with Plutarch's *De Iside et Osiride*, cf. the former, p. 23); Froidefond's Budé édition appeared in 1988 but was already outdated by at least a decade (the author tells us thrice that he could not use Griffiths' *The Origins of Osiris and his Cult* [1980]: pp. 132 note 2, 141 note 2, 176 note 1) and contains too much falsehood to be trusted — S. Schröder in *Gnomon* 62, 1990, 485–98. Not only is the editing erratic and the apparatus misleading; constant inaccuracy of reference mars the lengthy 'Notice' and the derivative 'Notes complémentaires'. For example, on p. 308 note 1, where «τὰ μὲν semble reprendre τὰ ἐν οὐρανῷ (cf. J. Festugière, Deux notes sur le *De Iside* de Plutarque, *C.R.A.I.B.L.*, 1959, p. 312–316)», read 'A. J. Festugière' and 'p. 312' (these generic references are a scholarly bane whenever they recur too often, as here, pp. 33 note 1, 52 note 2, 70 note 6, 75 note 1, 79 note 1, 83 note 3, 84 notes 2–3, 91 note 3, 113 note 1, 118 note 2, 129 note 3, etc, cf. E. Laughton, *Classical Review* 6, 1956, 38); on p. 261 note 2, Froidefond cites an article "Platon in Ägypten" by K. Nawratil in the *Zeitschrift für philosophische Forschung* 28, 1978, but he has the pagination so wrong ('p. 421–439'; *vere* 598–603) that one suspects a second-hand reference — indeed, his reason to quote this study is too remote from its contents to evidence a direct knowledge of it ('il est probable que Platon visita en effet la vallée du Nil', whereas Nawratil marshals eidetic resemblances of a vague stamp between some Platonic doctrines and a few Egyptian texts). On the Egyptological side, which the Plutarch scholar Schröder could not assess, the Budé is far outshined by G. Griffiths; though he utilized Hani, Froidefond embodies very little progress on his predecessor, and his individual contribution lacks solidity. He relies on dated scholarship (Erman's old religious handbook, p. 75 note 1; Lefebvre on the Egyptian 'novels', p. 92 note 1; Maspero, *ibid.* note 2; etc), whereas, of the work posterior to 1970, he mainly plundered F. Dunand's theses; small wonder his bibliographies, when he bothers to give them, provide weak guidance (an excellent instance on 133 note 3)... Finally, beware of his contradictions; in the section of the 'Notice' on the Osiridian myth (132–40), we are successively told that the origins of Osiris, Isis, Horus, and Seth «se prêtent bien aux exégèses "évhéméristes" qui, comme celles de K. Sethe, par exemple, prétendent en déduire les événements principaux de la protohistoire égyptienne. Ce sont là de pures spéculations, et on ne peut en vouloir à Plutarque de les avoir écartées» (132), then that «on admet à présent qu'Osiris fut à l'origine un roi mort par noyade et c'est sans doute le séjour du cadavre dans le Nil, mis en rapport avec la crue fertilisante, qui avait fait d'Osiris, dès l'Ancien Empire, le dieu de la fertilité agraire et, en particulier, du blé» (135; by 'à présent' Froidefond means two studies from 1909–1915, cf. his note 2 — on the drowning of Osiris, read rather Te Velde, *Seth, God of Confusion*, 84–6; Hani, 48–51; and Griffiths, *Origins*, 9–10, all of whom envision it as his assassination by Seth rather than the natural, or fitting, death of a Nilotic figure; Froidefond's view is tacitly discarded by D. Delia, "The Refreshing Waters of Osiris", *Journal of the American Research Center in Egypt* 29, 1992, 181–90, cf. 182–8 for a survey, down to the Imperial era, of the Osiridian funerary beliefs associated with the liquid element). Froidefond was at least cognizant with Egypt; whether this applies to M. García Valdés, *Plutarco, De Iside et Osiride. Introducción, texto crítico, traducción y comentario* (Pisa–Rome, Istituti editoriali e poligrafici internazionali, 1995), is far from certain, owing to her preference for Wortphilologie and textual criticism over Egyptological, philosophical or religious analysis in the (thin) commentary and the notes to the translation. In fact, these interests were determined by her editorial policy: a very conservative editor who seems to relish in casting off long-accepted emendations, her text is often so strained as to demand exegesis going beyond the help of a crib. At 359 B grammar has been elicited out of the gibberish of the copyists by the clumsy arrangement πρὸς Πύλαι (πύλαις, πύλας mss.) νίστιτα (νισιτιάνην, νισιτά μην mss.) ἄλλως, despite an unattested spelling and the weird geography entailed by this mention of the gates (of Memphis); parallels in Strabo and Diodorus strongly support πρὸς Φιλαῖς (Squire) νησιδα (the Adina; or νησιτιδα) ἄλλως, as advocated by Griffiths (365–7; the slight awkwardness of the unanswered μὲν which immediately follows he justifies on



lapsing into the recrudescence of trendy verbiage which diminishes in my eyes the value of the *Kaléidoscope* and compromises the dialogical impact of the priestly knowledge on Greece *Egypt and the Limits of Hellenism* aims to uncover. Provided the details are never allowed to obscure the cardinal points raised by the Greek of Herodotus or Plutarch, and that the latest news from Egyptology are reviewed with proper independence of mind by the would-be interpreter, replacements of G. Griffiths or Lloyd are sure to prove a boon to knowledge while driving the last nail in Bernal's coffin<sup>171</sup>. A vast, new treatment of Diodorus' first book is another great desideratum, albeit one diminished by this compiler's clumsiness (T. W. Africa, "Herodotus and Diodorus on Egypt", *Journal of Near Eastern Studies* 22, 1963, 254-8 at 254-5) and his astonishing gaps (O. Murray, *Journal of Hellenic Studies* 95, 1975, 215). The non-Egyptological A. Burton, *Diodorus Siculus Book One. A Commentary*, Leiden, Brill, 1972, generally turned a blind eye to these defects; «it provides a thoroughly useful primer to the many problems raised while rarely making more than the most tentative effort to solve them» (Lloyd, *Journal of Egyptian Archaeology* 60, 1974, 289), and has cast long shadows on the issue of the sources and the detail of the text<sup>172</sup>. Forty years

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p. 366), though the possibility of further corruption ought not to be ruled out too quickly — something might be missing after νησιδα, as seen by Bernardakis; Babbitt suggests νησιδ' <ἀγνή>, Froidefond νησιδα <την>άλλως. At 376 B Valdés exchanges the acquiescence in such unintelligibility for a signal instance of aberration, by reverting to the transmitted ἀπορίαν (again in defiance of Griffiths, 522): this word has no bearing on the behaviour of a magnet being repelled, for, despite her commentary (281), how on earth can the lodestone, or any metal for that purpose, 'reverse <the movement> and plunge (or plunge <it>) into difficulty / trouble', ἀνέστρεψε καὶ κατέδυσεν εἰς τὴν ἀπορίαν? To plead that this is metaphorical parlance does not do; Xyländer's ἀπειρίαν, 'infinity', restores both sense and sequence of thought at minimal cost, compare *De defectu oraculis*, 428 F-429 A, for Plutarch's own view of ἀπειρία.

<sup>171</sup> This is perhaps the best place to spell out a troublesome concern. Reference treatises whose narrative would have shed light on Bernal's big picture and which double as treasure-troves of data, like *Le mirage égyptien...*, *La religion égyptienne dans la pensée de Plutarque*, or *The Gift of the Nile*, only appear in *BA III* for details, nearly always of secondary importance, and in very low numbers (Froidefond elicited three mentions, Hani four, Vasunia one); Lloyd's major commentary was somewhat more useful to Bernal due to the exalted position Herodotus occupies within his Ancient Model, yet its presence in the linguistic demonstration barely reaches the double figures (thirteen mentions by my reckoning). On the other hand, G. Griffiths on Plutarch is cited thrice, whereas none of the commentaries on Plato's *Timaeus* (or on any other classical text) attracts a mention in the half-dozen places where this work features.

<sup>172</sup> Burton tends to eschew the traces of Diodorus' sloppiness except when they are huge, like the chronological discrepancy at 1.4.6-5.1 (39-42; either emend or plead guilty, *pace* P. Green, *Diodorus Siculus, Books 11-12.37.1* [Austin, University of Texas Press, 2006], 237-9, whose own scenario is baseless: «the historian (...) changed his mind at the last moment, subsequently forgetting — or not living long enough — to rework his chronological figures (...)» [p. 239]). Her Quellenbeitrag, pp. 1-34, extends to the entire book the conclusions reached by W. Spoerri for chapters 7-8, 10-13 (*Späthellenistische Berichte über Welt, Kultur und Götter*, Basel, Reinhardt, 1959, savaged by O. Gigon, *Gnomon* 33, 1961, 771-6, as an effort to dissolve into bland generalities the personality of the authors who peer through Diodorus' rewriting;

after, considering its errors, many of them wide-ranging (e.g. Henrichs, “The Sophists and Hellenistic Religion: Prodicus as the Spiritual Father of the Isis Aretalogies”, *Harvard Studies in Classical Philology* 88, 1984, 139-58 at 147 note 32); its inclination to naivety (e.g. Burstein, *Graeco-Africana*, 27 note 30)<sup>173</sup>; and its low level of bibliographical engagement, this synthesis is toothless — no more «l’analyse approfondie d’un commentateur qualifié (qui) décèle un effort de synthèse par confrontation des sources» (Chamoux, p. XXIX) than the Egyptian chapters of Diodorus I have any right to be called «une monographie très complète, la plus complète et la plus intéressante qui soit intégralement parvenue jusqu’à nous depuis le livre II d’Hérodote» (*ibid.*, 8, ‘Notice’ by Vernière)<sup>174</sup>. No other project ought to command the same level of obsession with Egypt as the one seen in *Black Athena* than the writing of fresh commentaries on Herodotus II, Diodorus I, and the *De Iside and Osiride* especially now that we have Rosól’s book.

Of course there will be scholars who will raise their hands in horror at such a minimalist piece of advice, as the outcome of a massive disquisition no less. Did I only slay beasts to come up with the obvious? Yet I believe this appeal to be the child of necessity. In an already darkened and erratic world, no great flood of light is likely to be forthcoming unless it springs from the re-examination of our most extensive literary witnesses conducted in accordance with strict philological guidelines. When the task has been done, we shall use these new tools to reap knowledge in quarters which, for the

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that 9 sqq. can still originate in Hecataeus of Abdera was shown by A. D. Nock, *Classical Review* 12, 1962, 50-1): for Burton, 37-41 and perhaps parts of 30-36 draw upon Agatharchides or Artemidorus; 7-13 reflect Spoerri’s philosophical *koine* of the first century B.C.; and the remainder of the book rests on a mosaic of informants, among which Hecataeus is by no means prominent, the personality of Diodorus showing through the repetitions and contradictions. Cf. rather Murray, “Hecataeus of Abdera and Pharaonic Kingship”, *Journal of Egyptian Archaeology* 56, 1970, 141-71 at 169-70, Burstein, *Graeco-Africana*, 3-18, and compare the debate over Zosimus’ use of his sources in books II-V 25 of the Ἰστορία νέα.

<sup>173</sup> P. 118 «but whereas it was natural for a Greek colony in a foreign land to attempt to establish a firm connection with the strangers by claiming that the colony had been founded by some indigenous hero, the Greeks would be unlikely to invent at a later date a barbarian founder for their own famous cities in Greece unless tradition actually suggested that such was the case»; p. 127 note 1 «(...) the name Ἀγυπτος is conjectured to have come from *hwt kꜣ Pth* (...). Montet, *Géographie*, I, 32 disagrees on the grounds that *hwt* becomes in Greek *á* or *ᾶ* (e.g. *Hwt-Hr* becomes Ἄθουρ [sic]), but clearly the borrowings belong to completely different periods. Ἀγυπτος being borrowed much earlier than the rest, and therefore quite possibly following a different phonetic progression»; etc.

<sup>174</sup> The apologetic tendency of the *Introduction générale* by F. Chamoux (in idem - P. Bertrac - Y. Vernière, *Diodore de Sicile. Bibliothèque historique*, I, ‘C.U.F.’, Paris, Belles Lettres, 1993) has justly been castigated as the result of faulty judgement informed by prejudice: J. Moles, *Classical Review* 44, 1994, 272-4 at 273, states that «failure (...) adequately to contextualise D. within ancient historiography exaggerates his distinctiveness (...). Many of C.’s judgements seem too favourable». There is another cause to this leniency: Chamoux was entirely under the spell of his adviser (p. VII note \*) Spoerri, cf. XI-XII, XXXVII note 95, etc.

moment, remain uncharted or inadequately mapped out. Perhaps the most burning topic is the Egyptian opus of Hecataeus of Abdera (*FGrHist* 264 F 1-6 plus the monstrous F 25 = Diodorus 1.10-98; not even its title has been preserved: Αἰγυπτιακά? or Περὶ τῶν Αἰγυπτίων?). This work buttressed the dependence on Egypt not only of Greece but, in a move not normally given its fair share of attention by Hellenists despite a concordance with the mindset of the Third Intermediate Period, of the entire world (per Diodorus 1.28, 1-29, 6)<sup>175</sup>. The Egyptian treatise of Hecataeus also appears to have been an influential source of Diodorus' first book, in which the Hecataean 'fragments' loom large<sup>176</sup>, and of Euhemerus, who followed his predecessor in

<sup>175</sup> Especially 28.1 οἱ δ' οὖν Αἰγύπτιοί φασι καὶ μετὰ ταῦτα ἀποικίας πλείστας ἐξ Αἰγύπτου κατὰ πᾶσαν διασπαρῆναι τὴν οἰκουμένην, the source of which appears to Burton 'unlikely to be Hecataeus. The problem is a vexed one' (p. 18), despite Murray, 'Hecataeus...', 145 (for whom it would have given this ethnographer «an effective claim for the attention of the civilized world on behalf of the nascent Ptolemaic state, and one which provoked answers from other Hellenistic historians under different patronage»), and G. E. Sterling, *Historiography and Self-Definition. Josephos, Luke-Acts and Apologetic Historiography* (Leiden, Brill, 1992), 71-2. An Egyptian source states that Amun has founded all the lands of the world plus Egypt, from which the manual crafts (*mnḥ.t*) came forth: Wenamun, 2. 19-21, in Schipper, *Die Erzählung des Wenamun*, 72-3 (bilingual text with notes), cf. 195-6 (commentary); both translation and interpretation are wide off the mark in Goedicke, *The Report of Wenamun*, 84-153, 85. If Schipper is right to emphasize a relatively late date for the composition of this work, under Shoshenq I (viz. 943-923 B.C.: Hornung - R. Krauss - D. A. Warburton (edd.), *Ancient Egyptian Chronology* [Leiden-Boston, Brill, 2006], 493; ca. 945-924 for Kitchen, *Third Intermediate Period...*, §§ 58-60, pp. 72-6 at 76), the importance given Amun by the redactor mirrors both this god's theocratic status in the 21-22<sup>th</sup> Dynasties and his presence on seal-amulets of the post-Rameside period discovered in Palestine and Egypt (Schipper, 299-324). In any case, Wenamun 2. 19-21 provides more than a passable fit with both Diodorus, 1.28.1 and 69.5 λέγουσι τοίνυν Αἰγύπτιοι παρ' αὐτοῖς τὴν τε τῶν γραμμάτων εὔρεσιν γενέσθαι καὶ τὴν τῶν ἄστρον παρατήρησιν, πρὸς δὲ τούτοις τὰ τε κατὰ τὴν γεωμετρίαν θεωρήματα καὶ τῶν τεχνῶν τὰς πλείστας εὔρεθῆναι (the latter passage already noted by Sterling, 71 and note 68). Hecataeus is the most likely intermediate, if not the only conceivable one so far as we can know, he who appears to have consulted Egyptian materials (priestly lists — e.g. Murray, 'Hecataeus...', 151 and note 3; Moyer, *Egypt and the Limits of Hellenism*, 104 — and perhaps more, despite his ignorance of the language: Burstein, "Hecataeus of Abdera", in J. H. Johnson (ed.), *Life in a Multicultural Society. Egypt from Cambyses to Constantine and Beyond* [Chicago, Oriental Institute, 1992], 45-9 at 49 = *Graeco-Africana*, 19-27 at 24).

<sup>176</sup> Pace Burton, 15-6, reflected by Chamoux (pp. XXVIII-XXIX), and with all due awareness of C. E. Muntz, *Diodorus Siculus, Egypt, and Rome* (diss. Duke, 2008), 9-27; cf. rather J. Campos d'Arca - P. P. Fuentes Gonzales, 'Hécatee d'Abdère', in R. Goulet (dir.), *Dictionnaire des philosophes antiques* (Paris, CNRS, 2000), II, 505-25 at 509-13, and Sterling, 61-75. Perhaps the most conspicuous test for the dependence of large chunks of Diodorus I on a source well-informed of things Egyptian — the issue was not put in those terms by Spoerri — can be sought in the separation of Heaven and Earth, after which the air plays a large role, at 7.1. This chapter and most of the following one (Spoerri, 1-131, particularly 114 sqq.) have been variously given to Democritus, Hecataeus, Epicurus, Posidonius (doxography in Chamoux *et al.*, 185 note 3; I 7, 1-3 + I 10, 2 + I 10, 6-7 = Posidonius, fr. 306 W. Theiler, defended at *Poseidonios. Die Fragmente* [Berlin-New York, De Gruyter, 1982], 183-5); we need not seek any unity of source here (Burton, 44, 46) and the agreement of 7, 1 κατὰ γὰρ τὴν ἐξ ἀρχῆς

building a Greek utopia, or rather a fantasy, out of Egyptian elements<sup>177</sup>. On the whole, Hecataeus cuts a more dignified figure than is currently supposed, cf. Burstein, p. 49 «his revised kinglist was a considerable achievement, an achievement which suggests that a similar analysis of the fragments of those portions of Hecataeus' work that deal with Egypt in his own time would also be worthwhile». The *Αἰγυπτιακά / Περί τῶν Αἰγυπτίων* thus demands the kind of assessment which only a meticulous commentary of Diodorus I may put on a sound basis if it has a firm grasp of both Egypt and the historian's compilatory method<sup>178</sup>. «Having chosen to use a certain author

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τῶν ἄλλων σύστασιν μίαν ἔχειν ἰδέαν οὐρανόν τε καὶ γῆν, μεμιγμένης αὐτῶν τῆς φύσεως· μετὰ δὲ ταῦτα διαστάντων τῶν σωμάτων ἀπ' ἀλλήλων, τὸν μὲν κόσμον περιλαβεῖν ἅπασαν τὴν ὀρωμένην ἐν αὐτῷ σύνταξι, τὸν δ' ἀέρα κινήσεως τυχεῖν συνεχοῦς, κτλ. with the separation of Nut (the Sky) and Geb (the Earth) by their father Shu (the Ether) in R. Parker – O. Neugebauer, *Egyptian Astronomical Texts* (Providence, Brown University Press, 1960), I, 67-8, is in my mind striking enough to preclude a fortuitous correspondence. I repeat: why refuse to identify Hecataeus here, at least provisionally? Cf. Burstein, *Graeco-Africana*, 20 sqq., for the details, and limits, of his Egyptological competence.

<sup>177</sup> Beyond Murray, “Hecataeus...”, 151 note 4, cf. Henrichs, “Sophists...” 150, 151-2; R. J. Müller, “Überlegungen zur ἸΕΡΑ ἈΝΑΓΡΑΦΗ des Euhemerios von Messene”, *Hermes* 121, 1993, 276-300 at 283-6; J. Dillery, “Hecataeus of Abdera: Hyperboreans, Egypt, and the ‘Interpretatio Graeca’”, *Historia* 47, 1998, 255-75 at 269-74; B. Bosworth, “Augustus, the *Res Gestae* and Hellenistic Theories of Apotheosis”, *Journal of Roman Studies* 89, 1999, 1-18 at 9-10; M. Winiarczyk, *Euhemerios von Messene. Leben, Werk und Nachwirkung* (München-Leipzig, Saur, 2002), 69-71; Stephens, *Seeing Double...*, 36-9; B. Garstad, “Belus in the *Sacred History* of Euhemerus”, *Classical Philology* 99, 2004, 246-57 at 247 note 3, 249-50.

<sup>178</sup> That the major strands of his surviving books, if not his entire books, stem from one source at a time — P. Goukowsky, *Diodore de Sicile Livre XVII* ('C.U.F.', Paris, Belles Lettres, 1976), IX-XXXI at XV-XIX ('il reste vrai que le travail de Diodore a surtout consisté à résumer un historien hellénistique' p. XVI, namely Clitarchus); idem, *Livre XVIII* (*ibid.*, 1978), IX-XXIV at XII-XVII (Hieronymus of Rhodes); B. Bommelaer, *Livre III* (*ibid.*, 1989), IX-XXXI at X-XIV (Agatharchides) — is now doubted by the most recent editors in the 'C.U.F.' (for Ephorus, J. Haillet, *Livre XI* [2001], X-XX at XV; B. Eck, *Livre XII* [2003], X-XXVII at XIV-XV). It must be declared that what they replace it by is both complicated and fragile: for instance, Haillet, X-XI, tells us that Herodotus was a / the key source for the Thermopylae episode, which surprises one given the massive discrepancies between the later narrative and the earlier one. More damagingly, P. J. Stylianou, *A Historical Commentary on Diodorus Siculus Book 15* (Oxford, Clarendon Press, 1998), 49-84, showed with strong arguments how Ephorus lurks behind most of book 15, which confirms the old notion that Diodorus actually epitomized him for the Greek and Persian narratives of books 11-5; differently, G. Parmeggiani, *Eforo di Cuma. Studi di storiografica greca* (Bologna, Patrón, 2011), 349-94, who does not dispose of all of Stylianou's evidence. As I hope to have suggested by (seemingly forgotten) Egyptian parallels to Diodorus 1.7.1 and 1.28.1 ~ 69.1, in specific instances the positivist stance retains some merit. Generally speaking, the trendy methods of present-day historians look to me more assertory than probative, in that they demand cogency in the harebrained operations of a compiler (Muntz, *Diodorus Siculus, Egypt, and Rome*, 19-21); turn into a positive virtue Diodorus' efficiency in erasing the stylistic particulars of the one historian he draws upon (e.g. Green, *Diodorus Siculus*, 26-9) while mistaking the position of the Quellenforscher (of course, per Parmeggiani, 392, Diodorus does not *transcribe* the contents of his source; compare F. W. Walbank, *A Historical Commentary on Polybius*, II [Oxford, Clarendon Press, 1967],

because that author's particular interests coincide with his own, [Diodorus] has a tendency to interpolate variant accounts of those aspects which again particularly interest him. Thus the original author is expanded precisely at those points on which he gave most information, and correspondingly perhaps abbreviated most heavily where he was originally weakest; the faults and virtues of the original are intensified» (Murray, 'Hecataeus...', 148). «A mixture of incompetence, lack of care, and ignorance is responsible for the vagaries which abound. (...) A complete list of Diodorus' blunders would be of very respectable length» (Stylianou, 137-8). Unfortunately, once the best of replacements for Lloyd and Griffiths and Burton are available, there is a real chance that even small-scale advances might never occur, so murky is the ancient evidence.

Needless to say, such commentaries would only ever play in the hands of scholars reminiscent of Van Sertima. Afrocentrists are, with the rarest exceptions, plunderers who forever give a twist to what they have read and only dip into the learned literature insofar as it supports their views; the finest commentaries will be lost on them. Those classicists who endorse(d) Bernal look scarcely, if at all, more likely to put to good use a fresh Diodorus I, Herodotus II, or *De Iside et Osiride*: Levine, McCoskey, Rankine possess neither inclination nor technical equipment to dirty their hands with philological subtleties; and the output of Griffith, who has both, exhibits an extravagance which at times reaches romance. Indeed the least that must be written about him is that he throws caution to the wind in order to score a point whether he deals with a problem of classical philology<sup>179</sup> or

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246-7 at 10.36.5); and pillory source criticism as assumption-ridden and hyperspeculative, as if their own ventures were not (e.g. C. A. Baron, *Timaeus of Tauromenium and Hellenistic Historiography* [Cambridge-New York, Cambridge University Press, 2013], 12-4). The whole matter of the sources remains, for Diodorus, an affair of individual, scholarly conviction; I am glad to find myself in agreement with Moyer, *Egypt and the Limits of Hellenism*, 104 and note 68.

<sup>179</sup> Griffith, "Putting Your Mouth Where Your Money Is: Eumolpus' Will, *Pasta e Fagioli*, and the Fate of the Soul in South Italian Thought from Pythagoras to Ennius", in G. Casadio - P. A. Johnston (edd.), *Mystic Cults in Magna Graecia* (Austin, University of Texas Press, 2009), 131-6. The wish of this Petronian character to be eaten after his death so that he should acquire a 'living tomb' serves as the pretext for a Gorgias-centered attempt to trace all Graeco-Roman beliefs in rebirth back to Italy; it compensates its dearth of compelling arguments with a pompous title and displays of largely irrelevant learning. «The point of intersection is the one heir *not* repelled by Eumolpus' stipulation (...). This man (...) is named Gorgias. This cannot fail to recall the "indefatigable stylist" (...) from Leontini, Sicily, who enthralled Athenians at the turn of the fourth century with his verbal pyrotechnics developed as "an analog of the culinary art" (...) — remember that the connection between rhetoric and cuisine is drawn in the very first chapter of the *Satyricon* (1.3, 2.1, 2.8-9 (...))», Griffith contends on 132, yet nothing specifically points to the Sophist there (cf. S. Breitenstein, *Petronius, Satyricon 1-15. Text, Übersetzung, Kommentar* [Berlin-New York, De Gruyter, 2009], 22-7). What is more, he cannot even be trusted upon to mention his predecessors (the suggestion that the Petronian Gorgias was meant to recall Gorgias of Leontini should have been credited to E. Courtney,



attacks an Egyptocentric issue. All the more reason, after *BA III*, to call for a moratorium on the spread of the literary scholarship devoted to the Egyptian influence on Greece and have the Afroasiatic zest bite the dust for good: *mihi persuasissimum est Bernalem recte facturum numquam*. I am not naive; the postmodernism of the eighties and nineties may have elapsed, its extreme disavowal of the clear-cut answers of traditional scholarship is no longer popular, yet the air has not been completely cleared of the spirit of irrationality that allowed Bernal to pass off his counternarrative for legitimate research. The lack of nose for shoddy scholarship out of which Levine and others waxed poetic about *Black Athena* still persists, that no ostracism of their ringleader and containment of Egyptocentrism will remedy. Before a neurotic analogy with Darwin, *On the Origin of Species*, Bernal closed *BA III* by asserting «I am convinced that between cultures known on other grounds to have been in contact, while quality of etymologies is desirable, quantity is also important. I believe that the more plausible etymologies of the kind proposed here one finds, the more additional ones should be accepted» (p. 585). As if the multiplication of possibilities, however protracted, of itself increased probability! The best we should achieve is ensure that such idle talk does not reach posterity; I suggest extinguishing its eventual by-products as soon as they come out and building a shield wall around those tenets of *BA III* and its attending literature that look deceitful enough to grow roots. (Time shall decide whether the present disquisition will nip in the bud attempts at canonization of the series; I merely trust I have made more complicated the task of whoever decides that the world will be the better for a fresh apology of Bernal or another exposition of the need for extreme cultural diffusionism applied to Greece).

The Egyptian impact on the Greek language barely exists (if anything, Greek was more influential on Egyptian through Coptic). Researching that so-called impact is a dead end and a waste of time. West Semitic influences on Greek at the lexical level there were, yet what remains to be brought to light in this field does not allow one to jump the wagon and juggle claims of wholesale Afroasiatic penetration on Greece. On the other hand and in deference to the truth, the reverse stance must be declared equally uncalled for. A Hellenist's obsession with those parts of the Near East which are now demonstrated to have been most influential on the budding Greek civilization too easily degenerates into displays of foolhardiness and insobriety. The will

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“Two Notes on Petronius”, *Materiali e discussioni per l'analisi dei testi classici* 40, 1998, 205-7 at 207, who sees an echo in 116.9), let alone use them to their fullest extent (so H. D. Rankin, “Eating People Is Right’: Petronius 141 and a ΤΟΠΙΟΣ”, *Hermes* 97, 1969, 381-4; Griffith pays lip-service to it yet remains mum on its reconstruction of a genealogy of the topic of cannibalism from the fifth century B.C. Sophists down to the school θεσπεϊς of the first century AD, clearly so that it should not spoil his own, different and much more speculative, descent — this I call scholarly dereliction of duty).

to march with the times determined some classicists to wildly attack with Levantine comparanda what they might without rashness have interpreted along purely Greek lines<sup>180</sup>. They were not entirely misguided, for look-alike at the lexical level<sup>181</sup> from Mesopotamia or the Levant embody a

<sup>180</sup> Even though he wrote prior to *The East Face of Helicon*, which proved to be both a landmark and a game-changer, and looked backwards to the antiquated Frisk and Chantraine or the fragmented articles by Szemerényi, Hemmerdinger, and Fournet, which were still fresh but rest on data that cannot any longer be deemed adequate, Francis was basically right to caution against overconfidence in assigning the origins of Greek lexemes and cultural artifacts to Anatolia, or the Levant, or Mesopotamia: «(...) we must beware of supposing that, even with the great advances in Near Eastern lexicology over the past thirty years, our present impression of the relationship between Greece and the Near East in matters of lexicon will undergo radical change. There are many reasons for such restraint. Even though we are increasingly well-informed about contacts between Greeks and their trading partners in the ancient world, it is often difficult to find the kind of cultural evidence which might render an apparent connection between a Greek and non-Greek word more than merely adventitious» ('Impact...', 486). Suffice it to contrast the stance of the enthusiastic yet ill-informed Louden: «the likeliest scenario for cultural diffusion is Greek contact with Phoenician culture, whether in ancient Syria, on Cyprus, or in the Greek world. Ongoing archeological research affirms how close ties were at times between Greeks and various Near Eastern peoples, the Phoenicians in particular. Since the Greeks obtained their alphabet from the Phoenicians (...), and Greek myth assigns key roles to Phoenicians (Cadmus, most importantly), it is likely that the two cultures also engaged in exchanges of narratives, or specific genres of myth, as well» (*Homer's Odyssey and the Near East*, 10, original emphasis). The proof of the pudding being in the eating, when all that Louden can muster amounts to pleading, in note 10, that «the *Odyssey* makes many references to Phoenicians or Phoenician culture: 4.83-4, 618; 13.272, 285; 14.291; 15.118, 415, 419, 425, 473; cf. *Iliad* 6.290-1; 23.743-4», one feels cheated. Another textbook case of insobriety lies in Kingsley's treatment of Egypt and Mesopotamia in his *Ancient Philosophy, Mystery, and Magic. Empedocles and Pythagorean Tradition* (New York, Clarendon Press, 1995): learning means little, and there is an enormous amount of it in this book, when assertion displaces demonstration in the case of the nature and geographical spread of what Kingsley defines as the Orientalized wisdom Empedocles and the Pythagoreans upheld; too much should not be read for these philosophers into the fact that their homeland, Sicily, was a cultural and trading *plaque tournante* in the Archaic period (242-4, 331-2) unless there exists material evidence apt to be detailed in a less exalted and cursory fashion than Kingsley's.

<sup>181</sup> G. Rubio, *Language* 74, 1998, 656-7 (review of Levin, *Semitic and Indo-European*): «ultimately this book does not deal with etymologies at all but with look-alikes. From the very beginning the author himself declares the kinship between his approach and Alfredo Trombetti's (viii). This book is, thus, a continuation of such a prescientific and merely intuitive way of searching for etymologies. With the same lack of methodological awareness, the author could have broadened his scope by comparing also Greek *potamós* with *Potomac*, Latin *deus* and Nahuatl *teotl*, etc.» To set the record right, one ought to specify that, if Levin's appreciation for the oeuvre of Trombetti, primarily the *Saggi di glottologia generale comparata* (which he was only able to consult at the eleventh hour, pp. VIII-IX), rather seems to belong to the personal level, it will not be denied that *Semitic and Indo-European* seems extremely fond of referencing the wild Italian (as per the index, p. 462). With respect to methodology, both writers can be lumped together as irrationalists, since Levin damages his stance by harking back to the naivety of Trombetti. Does he not posit that «the language of the received texts in those manuscripts» (of Homer, the R̥g-Veda, and the Hebrew Bible) «must be taken to go indefinitely far back for we have no data about the language as it was earlier»: *The Indo-Eu-*

temptation that should be resisted whenever the conditions are not favorable — when staunch linguistic considerations can hardly be adduced on their behalf and too little emphasis lays on the cumulation of evidence and the reciprocal support between aspects of the hypothesis. Yet whoever does a crude postmortem on the Homeric epics, brands them a concatenation of several genres of myths richly present in the Bible or the Ugaritic epics, then recovers a dialogical relationship between the Greek poems and these Levantine forerunners, can be a fine scholar like Loudon, nonetheless he hardly improves on Bernal's magpie-like picking out of diamonds and shards of glass in the primary sources. This cross-cultural researcher is very unlikely to be willing to glance beyond the surface of things when lexical looks-alike in Greek and a Near Eastern language cross his path. (One must be a properly informed and properly attentive interpreter to feel the type of Anstoss which prevents one from embracing causes a West or a Burkert would never throw the weight of their authority behind). The same temptation to forgo method has to be resisted in the case of the common etymological meaning of lexemes which we know to have been current in a roughly similar field or capacity at the same time, in possibly related parts of Greece and the Near East<sup>182</sup>. Both types of commonalities usually turn out to be mere

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*ropean and Semitic Languages* (Albany, SUNY Press, 1971), 11? On the contrary, thanks to Linear B (which Levin strangely discards in his first tome; *Semitic and Indo-European* II makes but restricted use of this evidence, on 128, 242, 257, 375) and the Bronze Age formulaic elements embedded in the Homeric dialect, like ἀσπίδος ἀμφιβρότης, Δὴ μῆτιν ἀτάλαντος, Ἐνυῶλίῳ ἀνδρειφόντηι, we are able to make more than educated guesses at 'the language as it was earlier'. Unsurprisingly, the sheer mass of data made available in both volumes of Levin is the only good thing about them.

<sup>182</sup> So the overseer called ἐπίσκοπος in institutional, fifth-century B.C. Attic parlance and מבקר in the Hebrew of Qumran, backed up by the equivalence (?) between the contemporary verbs ἐπισκοπεῖν and מבקר (Ezra 7: 14). Steiner thus speculated that «the מבקר in the Persian empire was the model for the ἐπίσκοπος in the Athenian empire. After all, the Athenian empire grew out of an alliance of Greek states against Persia. And it is certainly suggestive that the earliest attestations of the ἐπίσκοπος in the technical sense is in a decree from Erythrae in Ionia. The Greek cities of Ionia were part of the Persian empire before being incorporated into the Athenian empire, and Erythrae in particular was a hotbed of Medizers before they were driven out. It seems likely that ἐπίσκοπος was their term for the occasional מבקר sent by the Persian government. One might even theorize that the Athenians learned both the institution and the technical use of the term (and perhaps even a Persian-style uniform) from the Ionians, but this is not essential to my argument» «The mbqr at Qumran, the episkopos in the Athenian Empire, and the Meaning of lbqr' in Ezra 7:14: On the Relation of Ezra's Mission to the Persian Legal Project», *Journal of Biblical Literature* 120, 2001, 623-46 at 628-9). Steiner's discussion, 626-9, has been debunked by D. Janzen, *The Social Meanings of Sacrifice in the Hebrew Bible. A Study of Four Writings* (Berlin, De Gruyter, 2004), 197-8: you cannot reduce the ἐπίσκοπος to its Attic use, and the philology fails to cohere. The Septuagint, which employs ἐπίσκοπος but twice to equate to *√PQD* (so Janzen, 198 note 42, adding H. S. Gehman, "Ἐπισκέπομαι, ἐπίσκεψις, ἐπίσκοπος, and ἐπισκοπή in the Septuagint in Relation to פקד and Other Hebrew Roots: A Case of Semantic Development Similar to That of Hebrew", *Vetus Testamentum* 22, 1972, 197-207, e.g. at 199-204 for ἐπισκέπομαι), never has

scholarly illusions which it would be unreasonable to maintain, let alone multiply. Otherwise, true Levantine borrowings in Greek are likely to pass unnoticed<sup>183</sup>, and all Classical interlopers on the cuneiform or alphabetic

ἐπίσκοπος apropos of √BQR (בקר; cf., e.g., Hoftjizer-Jongeling, I, 187, *bqr* 1), « which would be odd if *episkopos* is the Greek translation of a Northwest Semitic *mbqr* » (Janzen, 198). More damaging yet, Steiner speculates that « לבקרא has the meaning “to exercise the office of מבקר” (just as ἐπίσκοπέω has the meaning “to exercise the office of ἐπίσκοπος” and just as B(iblical) H(ebrew) לכהן has the meaning “to exercise the office of כהן”) and that the מבקר was a “temporary overseer” or “visiting commissioner” sent by the Persian government to subject states to oversee major projects, like the setting up of a judicial system» (628); yet the claim hardly goes beyond a chain of analogies void of Persian evidence. Steiner only documents the Athenian side whereas the plausible Persian loanword in the Aramaic of the *Mishnah Tamid*, 5: 3, חזן, ‘overseer’, was at hand, like the further cases marshaled by C. E. Carter, *The Emergence of Yehud in the Persian Period. A Social and Demographic Study* (Sheffield, Academic Press, 1999), 80. Finally, despite Steiner’s silence, the Attic ἐπίσκοπος is hardly capable of not being proved to be the enigmatic Persian official which the Greeks translated as ὄφθαλμὸς τοῦ βασιλέως or βασιλέως ὄφθαλμῶς, the overseer / observer / spy not yet attested directly outside of the Indo-Iranian mythology, Old Iranian \**spasaka-*, Old Persian \**spaθaka-*, cf. Proto-Iranian \**spas-*, ‘to attend to, serve’ (Cheung, *Etymological Dictionary of the Iranian Verb*, 353-4; also, Bailey, *Dictionary of Khotan Saka*, 436-7, s.v. *spāśś-*, ‘to look’, Mayrhofer, *Etymologisches Wörterbuch des Altindoarischen*, II, 107-8, s.v. *PAS*) and PIE \*√*spek-*, ‘to look around, take notice’. Read further H. Lommel, “Die Späher des Varuna und Mitra und das Auge des Königs”, *Oriens* 6, 1953, 323-33, whose philological part, 324-31, remains unsurpassed; A. L. Oppenheim, “The Eyes of the Lord”, *Journal of the American Oriental Society* 88, 1968, 173-80; J. M. Balcer, “The Athenian Episkopos and the Achaemenid ‘King’s Eye’”, *American Journal of Philology* 98, 1977, 252-63 at 255-62 (the basic study; «the major difference between the two offices is obviously not in their function but rather in their method of selection», p. 262); and Briant, *Histoire de l’Empire perse*, 355-6 = *From Cyrus to Alexander*, 343-4. The \**spasaka-* is a great deal more thorny and elusive than J. Wiesehöfer, *Ancient Persia. From 550 BC to 650 AD* (London, Tauris, 2001), 266, cf. 62, makes it look; cf. A. F. Garvie, *Aeschylus Persae. With Introduction and Commentary* (Oxford-New York, Oxford University Press, 2009), at v. 980, p. 354 (alas with inadequate bibliography and limited understanding, as happens too often in his handling of Achaemenid matters); we cannot even ascertain whether our Greek sources are to be trusted whenever they suppose the uniqueness of the Eye, in which case he must have been of far more exalted rank than an underling like a spy (so Wiesehöfer; Garvie; or M. Brosius, “New Out of Old? Court and Court Ceremonies in Achaemenid Persia”, in A. J. S. Spawforth (ed.), *The Court and Court Society in Ancient Monarchies* [Cambridge, Cambridge University Press, 2007], 17-57 at 30-1). As a matter of fact, whoever considers that the Eye influenced the Attic ἐπίσκοπος in any capacity will naturally tend to diminish the status of this Persian official, as per the comparative evidence, see Oppenheim, 180 «the documentary sources at our disposal hardly mention the day-to-day doings of that host of ever present and interfering “eyes” and “ears.” We learn more about them when their activities are transferred to a supernatural level and they appear either as evil demons, as the swift messengers and servants or the “Eyes of the Lord”». To conclude, whatever the usual qualities of Steiner’s scholarship, not only is his argumentation lightweight and lacking in profundity on both Greek and Semitic sides; there is not much to say for the derivation מבקר > ἐπίσκοπος and decisive objections against it. His failure stands as a warning against reading too much into cross-cultural similarities: the boundary between (more or less striking) coincidences and conceivable instances of derivation or borrowing must not be blurred further.

<sup>183</sup> That is, whenever they are not as clear as Ἄξεινος (Slater, *Lexicon to Pindar*, 58) <Iranian \**axšaina-*, ‘dark-colored’ (Old Persian *axšaina-*, Young Avestan *axšaēna-*, Ossetic *axsīn*

traditions of the Near East will eventually end up mimicking the paragons of intuitive scholarship who inspired Bernal<sup>184</sup>. I see no point in mincing words here: Greek philology and linguistics are rather more difficult to master at an advanced level than Semitists think, lulled as they are into a fake feeling of safety by the ease of the script and the existence of a plethora of sophisticated grammatical and lexicographical repertoires the likes of which hardly exist in their home turf. It follows that experts in the Levantine languages usually tackle comparative matters with methods and an erudition no classicist worth his reputation would deem businesslike<sup>185</sup>. Small wonder one hundred

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with R. Kim, “On the Historical Phonology of Ossetic: The Origin of the Oblique Case Suffix”, *Journal of the American Oriental Society* 123, 2003, 43-72 at 51 note 20, 67 note 58), as was first seen as long ago as 1921. Read F. de Blois, “The Name of the Black Sea”, in M. Macuch - M. Maggi - W. Sundermann (edd.), *Iranian Languages and Texts from Iran and Turan. Ronald E. Emmerick Memorial Volume* (Wiesbaden, Harrassowitz, 2007), 1-8 at 1-2, to whose listing (note 4) add G. Hinge, “Herodot zur skythischen Sprache Arimaspen, Amazonen und die Entdeckung des Schwarzen Meeres”, *Glotta* 81, 2005, 86-115 at 106; Schmitt, in idem (ed.), *Compendium Linguarum Iranicarum*, 93 note 13; and W. B. Henry, *Pindar’s Nemeans. A Selection* (Munich-Leipzig, Saur, 2005), 38.

<sup>184</sup> One has to do them justice: the differences between Levin, Astour, or Gordon and Bernal all boil down to the dichotomy between polymath linguists and an uncontrolled amateur. While the former scholars are basically competent in all of the scripts and languages they compare (Levin being far more philologically aware in Greek than either Gordon or Astour, who in turn dwarf him in matters assyriological), Bernal is not in possession of a working knowledge of the main Classical, Near Eastern, and Afroasiatic tongues. Yet, are not the differences between a professional linguist and a crank abolished when one watches Gordon decide that the Linear A sign L57 (whose phonetic value was very uncertain back in 1966, when *Evidence for the Minoan Language* was released) must be *ja-ne* = Hebrew יין, *yayin*, ‘wine’, for no more compelling reason than that the group of signs L32-L57 were found on a pithos fragment and that other pithoi carry the wine ideogram? The frequency of similar leaps of faith in the works of Astour, Gordon, Bernal makes an ugly smear across the scholarship of the past half century.

<sup>185</sup> Gordon does not know the Doric plural genitive of the *-ā* themes (θυράων, not θυράων); the Doric spelling of μόνη (μόνα, not μύνα); or the grammatical expression of reciprocity (the Greek epigraphic sequences M[YN]ATOAOI and MYNATOAOI both get analyzed as μυνᾶ τῷ ἀ<λ?>ῶι, viz. μόνῃ τῷ ἄλλῳ, ‘one [f.] to another [m.]’; not only is this mere gibberish for ἀλλήλαις, the only correct form to which other archaic Cretan and Driean inscriptions conform, the true readings are MHATOΔOI and MYNAOΔMEN) — all three examples borrowed from Duhoux, *L’étéocrétois...*, 226 (cf. Heubeck, *Gnomon* 39, 708, for the last two). Red herrings crop up in large numbers in Astour, *Hellenosemitica*, 145-7; I shall address only two: «the adjective “Phoenician” was already used by the Mycenaean Greeks as *ponike* (= *phoinika*)» (145) misreads PY Ta 722.1 out of blind reliance on Ventris-Chadwick, *Documents of Mycenaean Greek* (the singular *po-ni-ke* followed by *qe* is an instrumental listed after a horse, a man, and an octopus — *a-to-ro-qa i-qa-qe po-ru-po-de-qe po-ni-ke-qe* — so can only be a material object, ‘palm-tree’ rather than ‘palmette’); this error is repeated and grows to massive proportions on 146-7, where Astour preys on *po-ni-ke*, *po-ni-ki-ja* > φοῖνιξ, φοινίκιος (*DMic.*, II, 138-140) to posit a West Semitic loanword for the Greek names of the purple and the Phoenicians — unfortunately, the semantic-etymological relationship between the toponym and the color is outright muddled, something an Hellenist or a linguist would hardly have ignored (cf., e.g., Beekes, s.v. φοινός; II, 1584-5). Astour further asseverates, p. 169 note 4, that an «inscription of the Phrygian king Midas (...) contains the peculiar Mycenaean



years and some of 'scientific' Semitic etymologizing of the Greek enigmas, from Lewy to Bernal, reaped but little firm result. To add insult to injury, these dealings were seldom met head on by those most qualified to assess the situation from the Levantine side<sup>186</sup>. So we classicists really ought to stop imitating wild Semitists by dragging in the Levant or Mesopotamia on texts where traits from these cultures are not obviously bubbling below the surface. For we should know better than to perpetuate the circles which end up foisting upon the reader a mass of speculations best forgotten. If it means that some of us need to move on to greener pastures, well, this will be a small price to pay<sup>187</sup>.

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titles *lawag(e)ta* (or *lawalta*, Phrygian variant) and *wanakta*, "duke" and "king"», as if Linear B *wa-na-ka* did not mean 'lord' applied to king or god (P. Carlier, *La royauté en Grèce avant Alexandre* [Strasbourg, AECR, 1984], 76-91, *DMic.*, II, 400-1, D. Nakassis. *Individuals and Society in Mycenaean Pylos* [Leiden-Boston, Brill, 2013], 6-7 for the king, 13-14 for the chief producer; Linear B preserves *ra-wa-qe-ta* and *ra-wa-qe-ja* for a kind of leader second only to the *wa-na-ka*: *DMic.*, II, 229-231); Astour also equates *ki-e-u* to 'Ugaritic *Ky*, *Kyn* and Nuzu *Ki-ia'*, 342, in defiance of the crystal-clear \*Χιθεύς (*DMic.*, II, 358) and though the Ugaritic comparanda are mere personal names, not ethnics (Del Olmo Lete-Sanmartín, I, 474); etc.

<sup>186</sup> With the one exception of M. H. Pope (who mauled Gordon's *Before the Bible* in the *Journal of Biblical Literature* 83, 1964, 72-6, underscoring 'an excess of zeal and defect of caution in the search for parallels' [76]), the greatest Semitic scholars, Albright, Driver, M. Cohen, Moscati, or Lipiński, did not distance themselves from their colleagues eager to teach Hellenists how unexplained Greek lexemes, customs, institutions can be traced back to the Levant. These masters must have been keenly aware of the similarity of method between Gordon or Astour and the prescientific Bochart and Bogan, yet they remained mum. Had Bernal's models been assailed by the best of their peers, he would perhaps have thought twice before entering the fray the way he did. The historiographical myth of the distrust of all Classicists and Indo-Europeanists vis-à-vis the Semitic languages, which resulted from the demolition of Gordon and Astour by the only competent readers of their books who decided to assess their case, viz. specialists of linguistics and Greek, has its roots in this abstention of Albright *et al.* (this further weakens the stance of Rendsburg, 'Someone Will Succeed...').

<sup>187</sup> Martin Bernal died of illness on June 9, 2013 while the present pages were in their ultimate stage of revision. It has been judged timely not to delay their publication and indispensable to preserve their outspokenness, as if the old fighter were still among us and would swiftly respond. Truth seemed more important than the courtesy due the dead, the future of classical studies (which have not been unaffected by the cultural war waged around *Black Athena*) takes precedence over a politically correct sense of scholarly loss. — This article is dedicated to Mary Lefkowitz, for her indefatigable commitment to the best historiographical and philological tradition despite the ingratitude of big chunks of the academe (*sunt enim quibus nihil omnino satisfaciat*). Her scrutiny of the many successive drafts of these pages, along numerous comments or suggestions, has been instrumental too; her assistance is acknowledged here with the deepest gratitude. Stephanie Budin, Thomas Römer, and Oliver Simkin have put me in their debt by answering queries over points of detail, by no means always bearing directly on *Black Athena*, and by providing welcome stylistic input. Needless to say these four scholars share no responsibility in my vitriol (and at least two of them strongly dissent from my, politically incorrect, name-calling). It was necessary, though, to confront sloppiness and unmask the instances in which friendship or partisanship had been allowed to override the facts; I might well have been crueller and drawn a list of well-regarded classicists thanked by Bernal in his prefaces — the same ones who happen to rely on insights from *Black Athena III*,

## ADDENDA

- Note 40:

Adams has now revamped his lexicon: *A Dictionary of Tocharian B. Revised and Greatly Enlarged* (Amsterdam, Rodopi, 2013), 2 vol. The complementary project edited by G. Carling in collaboration with G. Pinault and Winter, *Dictionary and Thesaurus of Tocharian A, I A-J* (Wiesbaden, Harrassowitz, 2009), would have warranted a mention had its etymological sections not eschewed, as a rule, all speculations on the Proto-Indo-European affiliations to limit the reconstruction at the Common Tocharian stage (p. XIII). The appendices in M. Peyrot, *The Tocharian Subjunctive. A Study in Syntax and Verbal Stem Formation* (Leiden-Boston, Brill, 2013), provide a parsing of the relevant passages from both dialects, pp. 621-717, and a richly annotated 'Overview of stem patterns', pp. 719-847. It may be worth pointing out that *BA III* is oftener wrong than right as regards this evidence, by dint of dependence on sources both antiquated and second-hand: thus, on p. 371, 'the Tocharian B *ai*' — without the hyphen! — should be *āy-* or *-e A* (Carling *et al.*, I, 41-2) and *ay-* B, *ai-* actually being the root (Malzahn, 542-3, 546) though it is usually considered the spelling of the B dialect (Adams<sup>1</sup>, 100-1; Adams<sup>2</sup>, I, 106-107; it entails the meaning 'to give' only in the active); on p. 391, 'to fall' should be *klāw-* A, *klāy-* B rather than merely *klā-* as reported in Chantraine (Adams<sup>1</sup>, 220-1; Malzahn, 619-20; Peyrot, 743; Adams<sup>2</sup>, I, 238-239); on p. 637 note 99, no less than three unsourced howlers have crept in, put right in *Aristarchus antibarbarus*, XII.

- P. 301:

An infamous instance of cultural contact through a Greek colony in Egypt, the identification of Zeus with Ammon can hardly be 'far older' than Pindar, whatever the «close links between the ancient oracle of Zeus at Dodona and

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whether or not they endorse the book (for instance, Ahl: see *BA I*, XVI; *BA II*, XXII; *Black Athena Writes Back*, XI; and *BA III*, XVI). Finally, I must thank Miryam Librán Moreno for her kind offices, and the staff of *Exemplaria Classica* who dealt efficiently with the demanding manuscript of an opinionated author; without them, it would have proved impossible to treat *Black Athena III* with the necessary *παρηγορία* and *ἀγχίνοια*. I must apologize to the reader before taking my leave: I intended to debunk Bernal with the amount of technicalities in the absence of which such a sore loser is sure to claim to have been malignantly misunderstood, yet his attempt to blur the lines between legitimate, cross-cultural ingenuity and multilingual quackery was so intricate, with layer upon layer of error-ridden special pleading, that the outcome has been a recrudescence of brick-and-mortar detail. I am loath to plead guilty for this state of affairs whilst admitting that a more laconic style and a sterner refusal to digress might have been preferable. What I ask forgiveness for is the inevitable degree of rigidity my piece exhibits in matters ethnographical and historiographical; I confess to being a pragmatist, old school if you prefer, insofar as influences and sources are concerned, so I favored scraps of evidence, however arcane or minute, over theory-ridden superstructures.

those of Am(m)on at Siwa and Thebes» (*BA III*, 478), cf. C. J. Classen, “The Libyan God Ammon in Greece before 331 B.C.”, *Historia* 8, 1959, 349-55, as summarized on 355: «in the third quarter of the fourth century B.C. the Libyan god Ammon, who had originally come from Thebes in Egypt, and his oracle in the oasis of Siwah were well known in many parts of Greece, the mainland as well as the islands and Asia Minor. It was obviously through the (mainly Dorian) colonists in Cyrene that the Greeks became acquainted with the god, first on the Peloponnesos, then elsewhere, especially in ports which had close commercial contacts with Cyrene. And as Ammon came to Greece through the mediation of Greeks he was not, though a Libyan god, regarded as foreign and his oracle took its place beside Delphi and Dodona. The history of its increasing fame in Greece is an interesting chapter of colonial history as it reveals the rôle which the colony Cyrene played as mediator between two civilisations». Consult further A. M. Woodward, “Athens and the Oracle of Ammon”, *Annals of the British School at Athens* 57, 1962, 5-13, for some fourth-century B.C. dedications to Zeus Ammon, along, e.g., I. Malkin, “Lysander and Libys”, *Classical Quarterly* 40, 1990, 541-5, for the marked, politically-coloured favor shown Siwa by Spartans during the fifth century; H. W. Parke, *The Oracles of Zeus. Dodona, Olympia, Ammon* (Oxford, Blackwell, 1967), 194-241, for the sanctuary itself; I. Rutherford, *Pindar’s Paeans. A Reading of the Fragments with a Survey of the Genre* (Oxford-New York, Oxford University Press, 2001), 352-5, for our earliest literary evidence; Lloyd, *Herodotus Book II. A Commentary*, I, 195-8, for the Libyan roots of the cult of Ammon in Siwa; V. Brouquier-Reddé, *Temples et cultes de Tripolitaine* (Paris, CNRS, 1992), 255-66, particularly 257-60, with A. Cadotte, *La romanisation des dieux. L’interprétation romaine en Afrique du Nord sous le Haut-Empire* (Leiden-Boston, Brill, 2007), 158-63, for Jupiter Hammon and its interferences with Baal Hammon (the Roman Saturn); and S. Georgoudi, “Des sons, des signes et des paroles: la divination à l’œuvre dans l’oracle de Dodone”, in S. Georgoudi - R. Koch Piettre - F. Schmidt (dir.), *La raison des signes. Présages, rites, destin dans les sociétés de la Méditerranée ancienne* (Leiden, Brill, 2012), 55-90 at 59-61, for judicious remarks on Zeus Ammon. It should be borne in mind that writing ‘Am(m)on’, as Bernal does apropos of Pindar, allows the purely Egyptian Amun to loom in the background of Zeus; most of the documentary case of *Black Athena* rests on this, deliberate and more or less artfully contrived, kind of ambiguity.

- Note 114:

Bernal’s old etymology of the name of Athena is confusingly endorsed by V. Blažek, “Hēphaistos versus Ptah”, in Van Binsbergen - E. Venbrux (edd.), *New Perspectives on Myth. Proceedings of the Second Annual Conference of the International Association for Comparative Mythology, Ravenstein*

(the Netherlands), 19-21 August, 2008 (Haarlem, Shikanda, 2010), 243-22 at 249-51. This so-called 'Post-scriptum in the light of the *Black Athena* debate' disappoints, in that too much weight is still given to the functional equivalence between Athena and Neith, despite an obvious *interpretatio Graeca*; *BA III's* 'etymons', the impossible  $h(w)t-n\bar{t}r n/Nt$  and the possible but shadowy  $h(w)t-n\bar{t}r nt Nt$ , are tacitly discarded; and the point of Egberts' strictures with respect to the dentals in  $ht-nt$  is made moot through an orogenesis of verbiage and graphemic manipulations allowed by the absence of Coptic and Greek equivalences of this formula. «One of Bernal's weakest arguments is his explanation of origin of the second vowel in the theonym  $*At^h\bar{a}n\bar{a}(i)$ . In the compound  $H.t N(y).t$  there is no space for any middle vowel. (...) Bernal solves this puzzle by the genitive particle  $n$ , correctly  $ny$  in m. and  $n.t$  in f., vocalized as  $*niy-u$  and  $*ni.t-u$  respectively (...). But there are no traces of the prothetic vowel, neither in the genitive particle nor in the divine-name, as Egberts mentions (1996-97, 159). The genitive particle connecting two feminines should also be in agreement with the same gender. So the whole formation could be reconstructed as  $*Ha(yi)t-nit-N\bar{a}zit$  around 2000 B.C., in the 1st millennium B.C. probably  $*Ha(t)ni(t)N\bar{a}yit$ » (p. 250 § 3). This chain of arguments does not merely come tantamount to a series of *non sequiturs*; their linguistic doctrine jettisons the evidence. First of all, the detail of the proposed Middle and New Egyptian vocalizations is anything but firm. Blažek initially hesitates between  $*N\bar{a}zit$  and  $*N\bar{a}yit$  (p. 250 § 2); marshaling the Greek anthroponyms composed with  $N\eta\acute{\iota}\theta$ , he should also have specified that  $Na-$ , though bolstered by  $\Pi\epsilon\tau\epsilon\nu\alpha\acute{\iota}\theta\iota\varsigma$  (-θης), or  $\Pi\epsilon\sigma\beta\omicron\nu\alpha\acute{\iota}\theta\iota\varsigma$  (-θης), may be doubted in the light of  $\Pi\epsilon\tau\epsilon\nu\acute{\eta}\theta\iota\varsigma$  (-θης), and  $\Pi\alpha/\eta\nu\acute{\iota}\tau\iota\varsigma$  (the references, with short discussion, in Quaegebeur - Clarysse - Van Maele, 'Athēna...', 223-4). The vocalic color of  $N.t$  similarly varies in the Akkadian transcriptions of Egyptian anthroponyms entailing her, so provides no safer footing: one finds both  $-Na-$  and  $-Ni-$  (A. C. V. M. Bongenaar - B. J. J. Haring, "Egyptians in Neo-Babylonian Sippar", *Journal of Cuneiform Studies* 49, 1994, 59-72 at 71). Furthermore, the goddess is indifferently written  $N.t$  (with or without the water determinative),  $Ni.t$ ,  $Nt.y$ , and even  $Nr.t$ , in New and Late Egyptian, *pace* Blažek who merely operates with  $N.t$  and the marginal, archaizing spelling  $Nr.t$ , altogether concealing to his reader that the latter came out of a confusion with the vulture and is a talking name ('terrible (one)'). Last but not least, the phonetic realization of  $N.t$ , etc, in these terminal stages of the Egyptian language, cannot be predicted insofar as the vulture sign conjectured by Blažek is concerned: was  $\mathfrak{z}$  dropped? did it weaken into  $y$  or a secondary glottal stop? It is not difficult to fashion an etymological process when someone takes so skewed and superficial a view of the Egyptian phonology. The Czech linguist tried hard to make a name for himself as the one who clarified the origins of Athena (" $At^h\bar{a}n\bar{a}(i)$ . What Was the First: The Name of Goddess or of City?", *Aspects of Comparative*

*Linguistics* 4, 2009 [proceedings of a March 2008 conference dedicated to the memory of Starostin], 125–43; there he validates the Hurrian and Semitic attempts at etymologization, but not Bernal’s old *H(w)t-nt*, cf. 135–6); it is strange that Blažek should forget the solid doctrine of this article so soon after its completion and, succumbing to the *πειθανάγκη* of Van Binsbergen (“Hēphaistos versus Ptah”, 249 note 21), fall back on an etymon that the vagaries of the New Egyptian vocalization prevent from being demonstrated as more than a distant look-alike to Ἀθηνᾶ, Ἀθηναίη.

- Note 154:

Maravelia’s massive volume reads like a study in credulity insofar as the semantic fields of cosmology and astronomy in Egyptian are concerned. Indeed, she uncritically endorses whatever results her great predecessor, the concise yet truly pioneering R. Krauss, *Astronomische Konzepte und Jenseitsvorstellungen in den Pyramidentexten*, Wiesbaden, Harrassowitz, 1997, arrived at, even when this by no means blemish-free work (J. P. Allen, *Journal of Near Eastern Studies* 61, 2002, 62–8 at 63 sqq.) discovers a previously ignored level of astronomical sophistication — read her summary, pp. 21–2. It is thus incumbent on the reader to remain uncertain at several identifications assumed as so many facts in *Les astres dans les textes égyptiens...*, even when the case looks strong, like the assimilation of the Pyramid Texts’ *mr-nhzi*, ‘shifting / winding waterway’, with the ecliptic detailed by Krauss, 14–66, and agreed upon by Allen. «Le Canal sinueux (...) s’inonde des eaux célestes et sur lequel le Soleil et la Lune se meuvent dans leurs barques sacrées» (Maravelia, 135), cf., e.g. Pyramid Texts, 359 § 599b ‘that he might bring Neferkare the ferryboat of the shifting waterway’, or 697 § 2172c ‘let Neferkare board the bark like Re, upon the banks of the shifting waterway’. Despite Krauss’ learning, all that Maravelia musters in favor of this identification is PT, 334 § 543a sqq. (p. 132 note 30, cf. Krauss, 18): this snippet only has Re traverse, *nmi*, the sky and cross, *dzi*, both Nut and the *mr-nhzi*, quite a common motif in the PT (263 § 337a; 265 § 351a; 266 § 358b; 334 § 543a; 473 § 926b; 481 § 999c; 507 § 1103b; etc) and the Coffin Texts (I 18 §§ 53d–54b; I 62 §§ 270g–271c; II 163 § 405c–l; etc) and no instance of what was described by Allen, 63, as «one of the recurrent goals of the deceased in the Pyramid Texts is to cross the Shifting Waterway from south to north in order to reach the Imperishable Stars in the Field of Offerings». Since Maravelia has little philological independence — e.g. she persistently miscopies the reading of Krauss’ proved inadequate by Allen (63 note 2) *mr-ni-hz* as *mr-n-hz* —, her book cannot be used to control the *Astronomische Konzepte...* on either astronomical or lexicographical grounds. To top it off, Maravelia has little scruple about combining notions deemed separate by mainstream Egyptologists. Her discussion of *Nw.t*, 382–92, ends on the following piece of rhetoric: «nous signalons, enfin, que



plusieurs fois, dans la religion et la mythologie égyptienne, l'on rencontre des aspects et des variantes différents d'un même mythe, or le fait que Nūt personnifiait à la fois le ciel *et* la Voie Lactée n'est pas du tout contradictoire, mais au contraire il est absolument compréhensible et raisonnable. Il montre en plus l'ingéniosité des Égyptiens, qui pouvaient combiner des notions archétypiques d'une manière assez ouverte, féconde, et solide» (391-2; this presupposes of course that *mr-nh3i* is not the Milky Way, as was widely believed before Krauss). I find this difficult to reconcile with PT 511 § 1149a, cited at Maravelia, 108, but not otherwise discussed: 'Geb laughs and Nut utters aloud her joy before he (= Pepi I) as he takes to the sky (*p.t*). The sky (*p.t*) roars for him, the earth shakes for him, the storm is broken for him, and he roars as Seth'. (Had a translation been provided for all the Egyptian in the book, this difficulty would probably not have been ignored). An identification of Nut with the Milky Way might seem simpler, per, say, R. A. Wells, "The Mythology of Nut and the Birth of Ra", *Studien zur Altägyptischen Kultur* 19, 1992, 305-21 at 308-9 (Maravelia pays lip-service to that article, pp. 386 note 50 and 451 note 8, but, as is her manner, does not engage its contents); yet it is vital not to juxtapose such distinctive notions as the sky and the visible galaxy under the byword *Nw.t* without a scrap of justification, for this sidesteps the problem in the most unscholarly manner conceivable. A reader-unfriendly work, *Les astres dans les textes égyptiens...* accordingly demands much more than the usual prophylactic caution; the ingenuity of the author runs ahead of her judgement<sup>188</sup>.

<sup>188</sup> I avail myself of this opportunity to explain out the system I followed when quoting from the Coffin Texts (after Maravelia, 31): I furnish first the reference to the relevant volume of A. de Buck, *The Egyptian Coffin Texts*, I-VII (Chicago, University Press, 1935-61), then the number of the Spell he carved out, from 1 to 1185, and as the paragraph(s), the relevant page(s) and column(s) each text occupies in De Buck's copies. These indications, which do not respect the editor's preferences (for he wanted the texts to be cited by volume, page, and letter, which leaves out the unity of each spell), allow one to quickly consult the admirable translation of Faulkner (*The Ancient Egyptian Coffin Texts*, I-III, Warminster, Aris & Phillips, 1973-78), who prints the volume and page / column numbers of each spell in the margin. The usual references found in Egyptology for the CT lack precision (number of spell, with occasionally an indication of page and column). My references to the Pyramid Texts are orthodox: first the number of the Spruch / Utterance, from 1 to 759, in the Sethe copies and commentary (*Die Altägyptischen Pyramidentexte*, I-II, Leipzig, Hinrichs, 1908-10; *Übersetzung und Kommentar zu den Pyramidentexten*, I-VI, Glückstadt-Hamburg, Augustin, 1935-62) completed by Faulkner's *Supplement of Hieroglyphic Texts*, Oxford, Clarendon Press, 1969, then the paragraphs Sethe divided the texts into, from 1 to 2291, finally the line(s) or column(s). The standard renderings are Faulkner, *The Ancient Egyptian Pyramid Texts, translated into English*, *ibid.*, 1969, and P. Barguet, *Les Textes des sarcophages égyptiens du Moyen Empire*, Paris, Cerf, 1986. To end on an unfortunately sour note: the bilingual versions of C. Carrier should be avoided, since they were done at enormous speed and are not very critical (*Textes des sarcophages du Moyen Empire égyptien*, I-III, s.l. (Monaco), Ed. du Rocher, 2004; *Textes des pyramides de l'Égypte ancienne*, I-VI, Paris, Cybèle, 2009-10; the latter has a half-decent introduction).