

Chaos in Methodology
Reflections on “*Cyrus the Great: Life and Lore*”

(a conference held at UCLA, Oct. 28-29, 2013)

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Bewildered by the inconclusiveness of the stories pertaining to Cyrus’s origins, a member of the audience asked Maria Brosius: “What are we to tell our children? Where can they turn to for anything concrete?” Brosius’s response highlighted two principles:

A : in seeking the truth, one should always go back to the sources, and

B : scholars are supposed to make the sources accessible and understandable

(A) is as sound as a principle can be, and (B) sounded as reassuring as possible. The problem, however, is that at the rate scholars are stamping the sources with a forgery mark, there will soon be no sources left to study, and if some of the methodologies that scholars use nowadays get acceptance, the “scholarly” approach shall become a hollow concept. To wit:

I – Stabbing Ctesias

Unfortunately, the elimination of sources through accusations of forgery has become an epidemic, with Ctesias being its latest victim. Referring to a recent conference on Ctesias, Brosius declared that the *consensus* of the participating scholars was that Ctesias had never been to the Achaemenid Court; his account was fictitious and therefore a forgery. Even Plutarch who considers Ctesias’s work as “a perfect farrago of extravagant and incredible tales” never doubted that he had been in Persia.

As Brosius spoke, I envisioned Ctesias stabbed to death and lying on the floor, with no student of Ancient History daring to touch him anymore (more on this below). What then crossed my mind was that the Ctesias assassins were either unaware of the *Vitae Constantini* controversy that David Frendo explained some time ago (BAI 15, pp. 58-60), or didn’t want to heed his advice. Eusebius’s *Life of Constantine* had long been branded as a forgery on the basis of some inconsistencies perceived in the text of imperial documents that he had produced, often with scorching remarks such as: “Constantine cannot have got his own age wrong”! As it happened, the text of one of the contested letters was discovered on the back of a 4th-century papyrus and vindicated Eusebius. Frendo (quoting E.G. Turner) thoughtfully warned: “*It is clear that a papyrological nemesis awaits those who, without good reason, throw away explicit ancient testimony*”.

Unlike the *Vitae* for which we have a copy of Eusebius’s account, poor old Ctesias’ work has been lost. The *History of the Persians* that he wrote some twenty years after he returned to his homeland has only reached us second hand, partially transmitted by authors several centuries removed. Errors were bound to creep into this chain of transmission. To begin with, Ctesias’s memory must have been less than sharp when writing his observations twenty years after his return. And then, the plethora of repetitive names among the Persian and Medes must have been as confusing to him as to later scribes ([Soudavar 2012](#),

66-70). In addition, Greeks had always difficulties in understanding Persians. On Zoroaster for instance, Xanthus of Lydia and Eudoxus of Cnidus report that Zoroaster lived 6000 years before Xerxes's passage of Hellespont in 480 BC, while Hermodorus and Theopompus recount that he lived 5000 years before the fall of Troy (Gnoli 2000, 47-48). Several approaches are possible in assessing these astronomical figures. A belligerent approach, in line with the forgery epidemic, is to declare that nobody in his right mind would have evoked such egregious numbers; therefore all four accounts are imaginary and must be declared forgeries. A second approach is the one taken by Helmut Humbach who, to rationalize his own misguided belief that Zoroaster lived circa 1000 BC, and quoting Xanthos alone, corrected the 6000 figure to 600, in order to place Zoroaster where he wanted him to be in time (Humbach 2000). But how can anybody assume that different scribes, transcribing the account of four different authors, would have committed the same mistake of adding one zero, to two different numbers! A third approach, however, is the one taken by Gnoli who provides a rationale for such a mistake by explaining how Greeks confused the Zoroastrian belief in an early conception of the spiritual being (*fravashi*) of their Prophet, with his physical birth two tri-millenary cycles later. These were notions developed in a situation of extreme tension between kingship and priesthood—dubbed as Magophonia by Herodotus—in which each tried to suppress the adversarial message of the other, and the general population responded by instantly mythologizing dampened realities. A vivid example of this process is being reenacted under the Islamic Republic—almost continuously—as historical facts are dampened from the top and people respond with allegorical stories at the popular level. These allegories and myths often confuse Iranians themselves, let alone a Greek physician who hardly spoke or understood the local language.

Be that as it may, Brosius's statement on Ctesias is a reckless condemnation that will result in turning students away from this important source. A point in case is an article that the late Sandy Morton once wrote to negate the authenticity of a group of *Letters* ascribed to the celebrated Mongol vizier Rashidoddin. In response, I wrote a lengthy rebuttal, in which each of Morton's arguments was demonstrated to be wrong (Soudavar 2003). It was hard to swallow for those who, like David Morgan, had hailed Morton's paper as the last word on the matter. On three separate occasions, Morgan has characterized my rebuttal as simply "unconvincing," without additional proof, any counterargument or counterexample; such a response is a sign of intellectual impotence, and has no place in a scientific debate. The Rashidoddin *Letters* should have been rehabilitated. And yet, at a recent conference in Sarajevo, when I suggested to a bright post-doc from Princeton that one of the *Letters* provided a very good proof for the thesis that he was proposing, he avowed that because of the controversy surrounding the *Letters*, he hadn't dared to look at them. I just hope that Ctesias' *History* will fare better than the *Rashidoddin Letters*, and that people will use the Gnoli approach to explain its oddities, rather than approaches in contravention of principle B.

II- Melting inscribed gold plaques

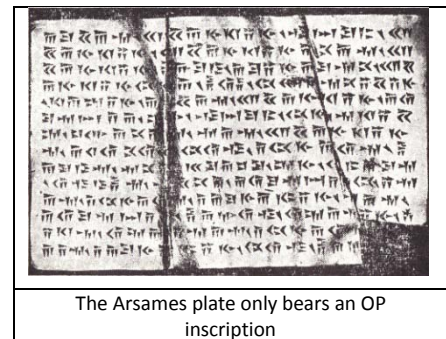
It seems that for lack of finding interesting subjects, scholars revert to unfounded sensationalism. A theory that falls into this category is the "Liar Darius" theory, by which Darius is supposed to have lied about the modalities of his ascent to the throne, and ultimately, about his legitimacy. Imagine: Darius, who says to have repeatedly combated the "Lie" in Bisotun, being a Liar himself! It is truly sensational. A corner stone of this theory was a misinterpretation of the Elamite text *DB L* of Bisotun (also referred to

as *DB §70*), by which Darius has supposedly claimed to be the inventor/initiator of the OP script. Consequently, the OP inscriptions of Pasargadae, in which Cyrus is said to be an Achaemenid, could not be right. To explain this discrepancy, the Liar Darius theory was developed, according to which, Darius had deliberately inscribed false propaganda in Pasargadae in order to create a fictitious bond between Cyrus and himself, through a fictitious Achaemenes. That fictitious bond, so they say, provided him legitimacy and the right to rule.

I shall discuss the Liar Darius theory further below (sec. IV), but for now I shall delve into the question and answer sequence with Matt Waters, whose translation of *DB L* has provided the main impetus for attributing the origins of the OP script to the reign of Darius.¹ Since Francois Vallat has not only refuted this translation,² but also argued that a “determinant” at the beginning of the Elamite version of the Cyrus’ inscriptions is a peculiarity of Neo-Elamite inscriptions and nowhere appears in the Elamite inscriptions of Darius and his successors,³ I asked why is it that the refutation of Vallat is ignored? Didn’t that special determinant provide a proof of authenticity?

His answer, which focused on the latter question, was that there are very few early Achaemenid texts to consider and many of them, such as the two gold plaques in the name of Darius’s forefathers, Arsames and Ariaramnes, were forgeries. I was infuriated by the latter statement, because, as a matter of principle, *an ancient document is valid until decisively proven wrong*. With the dearth of documents available from antiquity, one cannot eliminate primary sources *à la légère*. In further conversation after his lecture, and upon my questioning about the reason for his assertions, he answered that he did not remember exactly the arguments against the authenticity of the plaques, but he remembered that he had found them convincing when he had read them.

Before I tackle this issue, I need to first comment on Waters’ answer in respect to Vallat’s argument about the determinant sign of the Elamite version of Pasargadae inscriptions. Contrary to Waters’ assertion, those two gold plaques are in the OP script *only*, and have no Elamite rendition! Vallat’s observation rested on a repertoire of Neo-Elamite inscriptions that included documents from the Elamite kings prior to Cyrus. As Vallat confirmed to me in a telephone conversation, and as his reference in the footnote shows, the database he used was a syllabus established by M.J.



The Arsames plate only bears an OP inscription

¹ Waters provides the following translation “I (i.e., Darius) made an inscription beside the other(s) in Aryan, which formerly was not”; Waters 1996: 15. The “formerly was not” has been interpreted as “not existing before.”

² Vallat’s argument rests on the relationship between three consecutive words *daae ikki hutta*, for which he demonstrates that that the middle *ikki* should not be the suffix of *daae*, but must be read in combination with *hutta* as “I translated.” He explains that Darius rendered into OP, what was first written in Elamite. This new rendering had to be obviously written where there was nothing before. The key sentence in the Elamite inscription *DB L* is translated as: “...I (Darius) translated this differently in Aryan; it did not exist here before..”. It’s the OP version of the text that did not exist before, and not the OP script as a whole; Vallat 2011, 265-68.

³ Based on the syllabus of Steve, Vallat argues that at the beginning of the Elamite version of the *CMc* inscription (*é ku-raš ...*), appears the determinant *é* that was used in the Neo-Elamite period but never in Darius’ inscription nor in any of his successors; Vallat 2011: 278.

Steve (Vallat 2011:278). To refute Vallat's point, one has to either find an Elamite inscription—from Darius onwards—that has the same determinant, or prove that such determinant did not exist prior to Darius, neither in Steve's syllabus nor elsewhere. If one is unable to do that, then Vallat has produced a decisive argument that cannot be discarded in the cavalier fashion that Waters and Wouter Henkelman are doing.⁴

The discarding of the two early Achaemenid gold plaques reminded me of an incident that happened at the Iran Bāstān Museum of Tehran a few years ago. The Keeper of its treasury, who was in need of money, melted the gold foundation plaque of Persepolis! The story is well known in archeological circles; it was mentioned in the Iranian press but never discussed in the Western scholarly publications, perhaps because eliminating sources is the fashion of the day, by melting a gold plaque or by branding it as forgery. Whether one did it for money or for sensationalism, the end result is the same: a valuable document has been eliminated recklessly. It's rather strange for a scholar like Waters to forget the gist of the arguments pertaining to such an important issue. If he did, it's because there was nothing of substance to begin with.

To condemn an inscribed piece from antiquity, three issues have to be investigated: 1- the philological and grammatical problems, 2- the physical aspect, 3- the provenance. On the first issue (1), the objection was that the plaques had grammatical errors, which were supposedly specific to later Achaemenid texts and do not appear in early ones. Reverend Sharp, Pierre Lecoq, and Vallat have all three demonstrated this to be an invalid and self-serving criteria, since early and late Achaemenid texts were equally prone to grammatical mistakes; more importantly, Sharp demonstrated that in terms of the use of royal ideograms, these plaques conformed to Bisotun but *not* later Achaemenid inscriptions.⁵ In regards to the physical examination of the plaques (2), no scholar or expert that I know has ever seen them, let alone study their physical characteristics. How can a scholar express an opinion about the authenticity of an item that he has never seen? As for (3), these plaques were discovered in Iran in the 1920s, were apparently shown to Ernst Herzfeld who took photos and published them a few years later. They couldn't be forgeries from the 1920s since no Iranian forger had at that time the ability to create a text in the OP script. When I mentioned this to Waters, and asked him who could have possibly written such a supposed forgery in the 1920s, he answered he wouldn't know but there must have been a forger nonetheless. I cannot guess who he had on mind, but since I have elsewhere heard rumors about Herzfeld being the author of these presumed forgeries, I think it's best to bring the issue into the open and out of the domain of innuendos.

⁴ In a dismissive footnote, Wouter Henkelman, characterizes Vallat's refutation as "an adventurous re-interpretation of the Bisotun" without explaining why (Henkelman 2011: 582). He sticks to generalities rather than detailed logical rebuttal, and conveniently forgets to address the two major arguments of Vallat that I explained above. Because he can't argue against, he ignores them. It's a Morgan-type "unconvincing" response and a deliberate sully of Principle B.

⁵ Lecoq 1997:179-80; Sharp 1975: 18-21; Vallat 2011:279. Some had even suggested that the mistakes were typical of the era of Artaxerxes II and that he must have commissioned them to buttress his legitimacy versus his rebellious brother Cyrus the Younger (Sharp 1975: 18-19). But one wonders what legitimacy Artaxerxes II would have derived from plaques in the name of his forefathers that his own brother wouldn't?

III- Maligning past scholars

Ernst Herzfeld was a giant in the domains of archeology, philology, history and art history, all combined. His insight and vision of the historical past is unparalleled to this date. His misfortune was: a) to be a Jewish scholar caught in the entangled political web of intrigues that reigned before WWII in Iran and on the international scene, and b) that his visionary perception of Iranian history is to the dislike of those who dogmatically promote unjustifiable theories such as the Liar Darius theory, or that Zoroaster lived circa 1000BC. As a result, when the Freer and Sackler Galleries decided to organize a symposium on Herzfeld in 2001, his opponents saw an opportunity to discredit him. He was accused of being a collector, smuggler, dealer and an outright thief. In sum, he fared far worse than Ctesias, in his own symposium. These accusations have both a legal overtone—because they are about civil crimes—and bare a moral condemnation. I shall address them both.

One cannot allege legal impropriety without knowing what the laws of the land were in those days. For the record, I should say that Antiquity Laws of Iran were promulgated on Nov. 3, 1930, through the efforts and with the consultancy of Herzfeld himself. He knew the law and acted accordingly. It was a liberal law that concentrated on national monuments and important objects of national interest. Articles 2&3 of the Law wanted the government to create a list of monuments and objects of national interest. As per Article 4, objects in private possession had to be presented to the government and if deemed to be important, they had to be included in the national register. If not, it was pretty much left to the finder or the owner to do as he wished. In confirmation, Art. 6C only prohibited transactions of *registered* items; there was no restriction on non-registered items. Article 10 addresses the issue of new found items. If of national interest, the government had to register them first and *buy* half of them at market value; it could retain the other half or *give it back to the founder*. Articles 14&15 allowed *private* excavations with a government license, and included a procedure to divide the finds. Article 18, even stipulates that items found in licensed excavations that were given back to the finders were exempt of exit tax. As a result, many licensed excavations were active in those days,⁶ and dealers often submitted their finds to Herzfeld for assessment. That is how he was able to take photos of found objects. If he recorded them in his diaries, it did not mean he owned them.

In practical terms the government never registered new found items because hundreds of broken pieces and shards were regularly found and nobody cared to evaluate them or register them. It was too cumbersome. What Herzfeld took for himself were mainly items of this category, items that nobody wanted or knew what to do with them. If he had not taken them, they would have been probably lost or gradually destroyed. And that to me justified his action on moral grounds. If they were to be left on the spot, only to be neglected or destroyed, no moral objective would have been achieved. Many countries are simply not geared to take care of the multitude of pieces found in their territories, even important ones: The famous Lydian hoard of the Metropolitan Museum was returned with great fanfare to Turkey, and a good many of them are now missing. I hope they were not melted as in the Iranian case. A crate of objects that belonged to Herzfeld, and was left in Iran, is a good test case to assess this outlook. The crate was accidentally found in the 1970s. Before Herzfeld is lynched, it is incumbent on his detractors to: a) see whether the items of this crate would have been considered of value in the 1930s, and b) how

⁶ Private excavation licenses were granted up to the 1960s.

they have fared since their rediscovery; are they still around, or have they been discarded? That answer will provide the moral judgment with a perspective.

In a blistering attack on Herzfeld, Oscar W. Muscarella has falsely accused him to have owned four silver plates and later sold them (more on Muscarella further below).⁷ One of these plates is at the Freer and the other at British Museum, and both have conducted tests to ascertain their authenticity. But in an article published a few months after the Freer-Sackler symposium, Nicholas Sims-Williams, has argued that the inscription on these plates—in the name of Artaxerxes—were forgeries perpetrated by Herzfeld (Sims-Williams 2001). In a forthcoming publication, I intend to disprove this contention on physical grounds, on circumstantial evidence and philological considerations, and also show that they were neither excavated in Persepolis, as Muscarella contends, nor did they ever belong to Herzfeld. At this juncture, however, I wish to emphasize a problem of methodology. Sims-Williams' reasoning basically rests on one word, *s-i-y-m-m*, which he considers: a) to be as loan word from Greek, and b) that it first appeared on a Greek papyrus in the 1st century AD. The inscription must therefore be a modern forgery. When it comes to loan words between two Indo-European languages, it is not easy to say who borrowed from whom. The first attestation in one language militates for it to be the lender. In normal circumstances, if the shadow of a "crooked" Herzfeld had not put into question the authenticity of these plates, the appearance of *s-i-y-m* on these plates would have been considered as a first occurrence, and proof that Greeks borrowed it from Persians. But such has been the devastating effect of the attacks on Herzfeld that even a brilliant mind as Sims-Williams can succumb to false propaganda and neglect this second possibility. Sims-Williams concludes that : "It can only be modern forgery, which betrays itself as such by this one demonstrable lapse." I am afraid that the physical evidence alone from the Freer plate will prove him wrong. It is always risky to build a case on the basis of one supposedly "demonstrable lapse."

If I have spilled much ink on the Herzfeld issue, it's because it's not an isolated case and a new trend seems to be developing among modern scholars: to malign those of previous generations. It's one more way to indulge into sensationalism, albeit a very unethical one. A year before the Herzfeld symposium, Muscarella published a book (Muscarella 2000) trying to expose supposed forgeries and the malpractices of past archeologists. Among others, Roman Ghirshman was branded as a thief, a forger and an accomplice of dealers who in the back rooms of the Tehran Bazaar refashioned and reworked Luristan bronzes to make them more valuable. His analysis was so flawed, both technically and logically, and his accusations were so outrageous that I wrote a long letter to then director of the Met, Philippe de Montebello, exposing his methodology and his unwarranted attacks on the reputation of scholars. For those interested, the said [letter](#) is posted on my website, and so is Montebello's [answer](#) expressing his agreement with my conclusions. As it turned out, there was not one "papyrological nemesis" awaiting Muscarella but hundreds of them. The discovery of hundreds of Jiroft items, similar to those he had falsely stamped as forgeries—without ever physically examining them—should have discredited his methodology and exposed his bias. But I have not seen one single scholar criticizing his methodology, his conclusions, or his unethical accusations. To malign past scholars is sensational, but to defend them must be boring.

⁷ <http://www.academicroom.com/article/ernst-herzfeld-and-development-near-eastern-studies>

Muscarella's objective is to discredit every item that has not been found through authorized excavation. The rationale for this approach is that much information is lost when "grave diggers" conduct clandestine operations. But if the preservation of information is the objective, Muscarella is guilty as well. His actions are akin to the anti-abortionist who takes away the life of a doctor to save "life"! Stamping an antique object with forgery is akin to killing it, and results in a loss of information.

IV- The Liar Darius theory

I have extensively criticized elsewhere ([Soudavar 2012](#)) this theory, and presented evidence about Darius's legitimacy and his right to rule within the Achaemenid line of succession. I see no need to repeat them here. But since this paper is about methodology, I would try to point out additional methodological flaws in that theory.

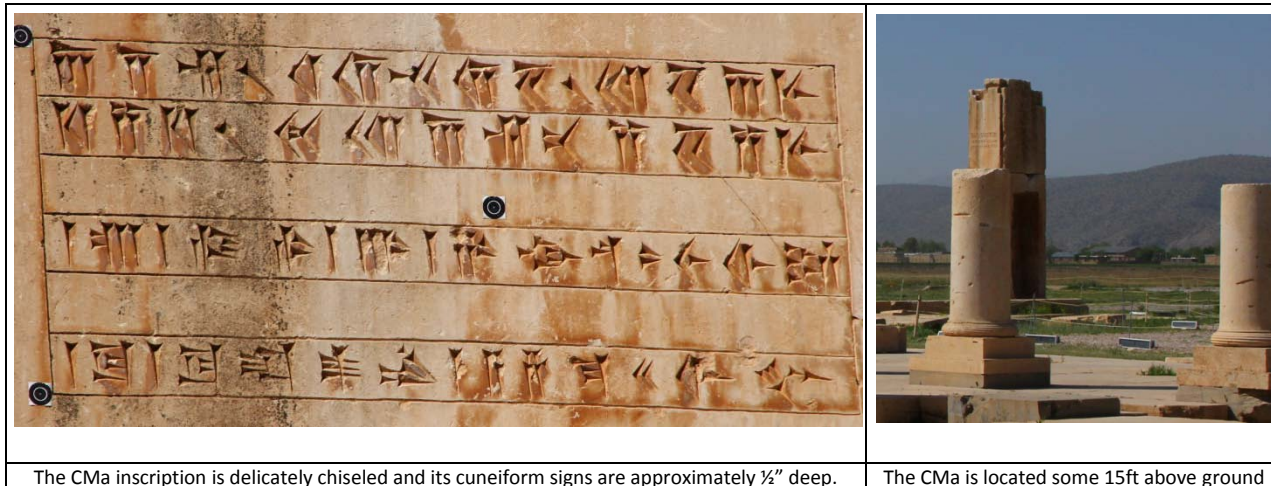
1- The proponent of this theory considers Darius's claim of being an "Achaemenid" as a lie, but his supposed boast to have invented the OP script as correct. Once a liar, always a liar. If Darius had lied about his genealogy there is no reason to believe his other boast. It's almost a Kafkaesque reasoning to claim that his second boast is true, in order to ascertain that his claim to the throne was false.

2- To dupe everybody, Darius supposedly initiated a propaganda scheme that connected him and Cyrus to a fictitious ancestor, Achaemenes. To see it, the supposed addressee of this propaganda scheme had to first go to Pasargadae, and find there on a narrow strip of a figure's robe, or on a corner pillar, only *half* of this propaganda: inscriptions in which Cyrus says he is an Achaemenid. The other half is nowhere to be found in Pasargadae, for Darius's name nowhere appears in there. Like for a treasure hunt, this person must then go to Bisotun (it takes about 10 days on horseback) and climb that vertical mountain, in order to find somewhere up there that Darius too was an Achaemenid. He would then have to connect the *two* claims together, and say: "Aha! I got it, Darius had a common ancestor with Cyrus." What an absurd way to conceive propaganda.

3- When proposing a theory, one has the duty to follow it all the way to its conclusion. What remains unexplained in the Liar Darius theory, is how this young man of 28, recently back from Egypt, persuaded all other contenders about his right to rule. Did Darius say to Persian noblemen and warlords that: "Cyrus and I had a common ancestor that none of you know of, and therefore you must elect me king?" Did they all say: "O wise Darius, please forgive our ignorance, thou art right and thou must be king"? I see only one way for this to have happened: Like Asterix le Gaulois, Darius had a magic potion that gave him power to bang every contender on the head and force them to submit.

4- I have much criticized David Stronach's arguments concerning the Cyrus inscriptions in Pasargadae. At the UCLA conference, he graciously suggested that we should agree to disagree. Absolutely. But my concern is methodology. When one has a theory in mind, one inevitably tries to find supporting arguments for it. But once found, one needs to verify whether it holds elsewhere as well. I find the following technical argument advanced by Stronach to be utterly wrong. He contends:

“Cyrus would have violated standard Achaemenid building practice if he had indeed erected the CMA inscription on any part of the stone fabric of Palace P while that structure remained largely unfinished.”⁸



The CMA inscription is delicately chiseled and its cuneiform signs are approximately ½” deep.

The CMA is located some 15ft above ground

In other words, the engraver would have only added the inscription at the last moment as a finishing touch. But anybody who has seen this inscription in situ knows that the carvings are deep, complicated and needed advanced planning. Contrary to Stronach’s assertion, any building planner in his right mind would have had the carving done while the slab was on the *ground* rather than when erected at a height of several meters above ground, especially since it was a repetitive work that appeared on at least 8 corner pillars, or *antas*.⁹ A serial operation is always easier to work on the ground and side by side, rather than individually and up in the air. His contention is almost like saying that for the bull capitals on top of columns, a block of stone was first placed there, and a sculptor was then sent up to chiseled it out.

I have chosen these four examples to show how a dogmatic belief in the Liar Darius theory has fostered wrong arguments. If proponents of this theory still believe in it, they need to find better explanations.

V- Killing by names

Several speakers projected an image of the famous bas-relief on the doorjamb of Pasargadae and referred to it as the “winged genius,” which has become the standard term for designating this figure. But similar to forgery allegations, inappropriate names can neutralize the meaning that an iconographical composition was supposed to convey. Irrespective of who first developed the winged anthropomorphic figure, throughout the Near East, from the Hittites, to Assyrians, Urartuans, to Babylonians and Iranians, it always represented the supernatural counterpart of man, i.e., a deity. This is a basic concept and if that is not understood, it is futile to talk about the meaning of royal iconography. What’s more, this anthropomorphic figure is wearing a royal Elamite robe. As such, it is either Cyrus’s

⁸ Stronach & Gopnik 2009.

⁹ The height of the *anta* is 6.14 m as per the Unesco report, and the inscription measures 90 x 40 cm; Pasargadae 2004, 14.

most revered deity, or one of the most important ones of his realm, for, whenever you see a deity before the king throughout the Near East, he is clad with the same robe as the king. By naming it “winged genius,” its status is diminished from deity to a mere decorative element. David Stronach argues that both the large bodied guardian sphinxes (found as shattered pieces) and the winged figures of Pasargadae were inspired from Assyrian prototypes and had *apotropaic* functions, but then bends backward to justify that even though the winged figures were facing “inwards and not outwards” they were meant to “prevent malevolent forces to make their way in” (Stronach 2013, 66-69). The fact is that Assyrian winged figures, whether inside or outside, were not apotropaic but always in the company of deities and kings, and even the guardian sphinxes were more likely to project grandeur than scare spirits off. Kings were prone to display their strengths and not their weaknesses, especially in the Iranian context. It was counterproductive to insinuate that kings were afraid of some malevolent force and needed protection. To the contrary, these sphinxes were to project how great was the king whose palace was guarded by such colossal supernatural creatures. If the figure on the bas-relief is recognized as deity, the historian has a duty to identify it. I have proposed that it represented Mithra, and that the fish-clad figure on another doorjamb designated Apam Napāt ([Soudavar 2012](#), 49-52). Whether acceptable or not, it does not relieve historians, as well as art historians, from the duty to identify these deities. By applying the term “winged genius,” they simply evade their responsibility.

VI- Cyrus the Anshanite vs. Darius the Egyptian

In his conference paper, and relying on a recent paper by his former student Antigoni Zournatzi, David Stronach rightly criticized the newest theory in circulation: that Cyrus was an Anshanite and not a Persian. I am afraid that it would take much more than the concerted efforts of Stronach and Zournatzi to reverse this trend, for one must now contend with a politburo that severely criticizes those who err from the Anshanite path, and brings them back into line. It has apparently the necessary means to do so, for I have noticed that Mark Garrison who used to qualify Cyrus as an Achaemenid, was given notice by Henkelmann, and had to apologize for committing such a sin; he rectified his “error” in his next paper (Garrison 2011, 378, n.4). Oddly he does that in the same volume in which appears Vallat’s article contradicting this whole thesis, and for which Garrison is one of the editors. Why be an editor if you can’t read what the contributors say.

Be that as it may, based on the same methodology that Henkleman used, I wish to propose to Garrison that Darius was Egyptian and not Persian, and see if he buys my argument even though I am not a member of the Anshanite Poliburo. Henkelman concentrates on one secure item, the Cyrus Cylinder, in which Cyrus is named King of Anshan, and then pushes aside all sources contradicting his theory. If the Babylonian Chronicles and Berossus call Cyrus King of Parsu, or of the Persians, they cannot be trusted. Neither can be Herodotus, who qualifies Cyrus as an Achaemenid. As for the Pasargadae inscriptions, they were commissioned by Darius as propaganda (Henkelman 2011, 597).

Now, there is in the Louvre an Egyptian plaque with the following inscription: “The perfect god, lord of the two lands, king of Upper and Lower Egypt, Darius; life forever.” Egyptian scribes came in the wake of 2000 years of scribal activity. They knew what they were writing. And besides, the first time we hear about



Louvre plaque

Darius, he is with Cambyses in Egypt. He must therefore be an Egyptian prince! Following Henkelman, I say that the inscriptions ascribed to Darius were actually ordered by Xerxes—who did most of the construction there anyhow—because he wanted to expunge his father’s Egyptian identity. Like Darius, Xerxes commissioned all these inscriptions as propaganda. Therefore, in the name of Amon and Re-Harakhty, I implore Mark Garrison that he should give back to Egypt Lands what belonged to them, and from now on he should call Darius: “the Egyptian.” This way no Persian would be left in Persia, and everybody would be happy.

Final reflection: One can reach any desired objective with false arguments and bad methodology.

Abolala Soudavar
Houston, Nov. 3, 2013

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