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Logbook

Amir Ahmadi, The Bīsotūn Inscription - A Jeopardy of Achaemenid History

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Reviewer 1

The author proceeds from an initial review of current scholarly positions on the ascension of Darius I to the throne of Achaemenid Persia to posing a series of critical questions regarding the general consensus of this king as an usurper to the throne and the supposedly propagandistic nature of the inscription commonplace in the literature.

I found the manuscript a well-researched piece with an extensive and up-to-date biography. While the disposition and manner of the argument is highly specialised, and the writing occasionally idiosyncratic or delving upon very narrow scholarly debates, the overall thrust of the manuscript appears sound to me. The initial critique, that primary sources (the Bisutun inscription) and their contemporary accounts (Herodotus) should not from the outset be considered propagandistic in nature or designed to divert from historical truth, I can only applaud. There are several examples of similar epistemological blunders in e.g. Neo-Assyrian studies, where the initially justified critical reading of primary sources becomes a basis for an argumentation from an imagined and obscured historical truth conjured by the scholar – which of course leaves primary sources logically unreliable.

Then again, I find it a somewhat poor show of style to devote a manuscript solely to the deconstruction and dismemberment of virtually all past significant studies on the subject without offering a more clearly outlined alternative than what is presented here. The author evidently seems to think that the inscription itself and contemporary accounts are more trustworthy than the conclusions that much modern research derive from them, but I failed to find a clear and concise section laying out this position, which would have made the manuscript appear more a constructive piece for future research rather than the mere initial deconstruction of current narratives. The blank slate resulting from the latter surely cannot be in the authors interest.

While the grafting of speculative and directly opposing historical narratives conjured by the scholar onto the frame of primary sources that are then themselves muted should, of course, be critically examined, primary sources of ancient history cannot pose an argument strictly by themselves, although the author implicitly seems to think so here. In several places, the author makes clear that his or her intent is not to explain the driving factors in the ascension of Darius I, but merely to evaluate the soundness of scholarly arguments suggesting that Darius I was an usurper. But this position, logically, also assumes that the inscription carries a message which is not in agreement with the conclusions of other scholars. A more clearly stated outline of the author's own position on the subject would certainly improve upon this manuscript.

Reviewer 2

General Impression

In this paper, the author challenges the commonly held view that the narrative concerning the accession of Darius as narrated in the Bisotun inscription should not be taken at face value, but rather interpreted as the propaganda of a usurper. This undertaking certainly has merit and there are many interesting points brought up in the discussion. The author bases his conclusions, namely that there is no reason not to take Darius' account at face value, on the discussion of three suggested interpretations that are discussed at length: those of Rollinger, Bickerman, and Shayegan.
The author spends by far the most time discussing (and rejecting) Rollinger's interpretation which is certainly warranted, seeing as it is this interpretation that has garnered the largest following. The author discusses, in turn, the interpretation of the term *duvitāparanam,* the use of the number nine in the Bisotun inscription and Rollinger's reconstruction of Darius' predecessors, and potential Ancient Near Eastern precursors for the structure of the Bisotun monument. The author then goes on to suggest that Darius' marrying his predecessors' widows expresses common practice and not an attempt to legitimize his own rule. He further discusses and rejects Bickerman's suggestion of Darius' fabrication of Gaumata as a negative character based on the reputation of the magi and discusses Shayegan's proposal of a connection with the Mesopotamian substitute king ritual.

One point where the paper falls short is in that it considers the evidence from a predominantly text-internal perspective. One of the reasons the Bisotun inscription is considered propagandistic lies in its widespread distribution, from Iran to Babylon to Elephantine in Egypt. One cannot argue for or against the historicity of the account of the Bisotun inscription without also taking text-external criteria into account, including a discussion of the historicity of some of the events referred to in the inscription. A point that might strengthen the author's argument in this context is the historicity of the 'lie-kings' in Babylonia, corroborated through local year dates (see, for example Lorenz 2008 and Bloch AfO 42 (2015)).

While the author discusses the points that have been raised in favour of a propagandistic interpretation at length, he often fails to suggest an alternative interpretation. He rejects Rollinger's attempt at reconstructing a linear genealogy, constructed by Darius and not considered historical, but he does not suggest an alternative explanation for Darius' claim that he is the ninth king in his family – the crux that underlies the discussion regarding Darius' legitimacy.

Likewise, the author accepts Lecoq's suggestion regarding the origin of the Old Persian script without further discussion ('Lecoq's conclusion is sound', p. 12) and in consequence considers the Cyrus inscriptions from Pasargadae (referring to Cyrus as an Achaemenid) genuine and not fabrications by

Darius to legitimize his rule. This may very well be true, but the debate regarding the date of the invention of the Old Persian script remains far from settled.

Frequent references to an undefined entity, 'the historian', make it seem as if there is a unified scholarly consensus regarding the events of Darius' accession and it is not always clear who is actually meant when the author refers to 'the historian'. While most scholars indeed interpret the inscription as conforming to a certain royal ideology, there is definitely a wide range of opinions and interpretations, some of which the author cites himself, and the literature on the subject is far from uniform.

The tone of the contribution is at times unnecessarily polemic, and I strongly suggest a thorough revision of the manuscript using a more factual tone.
There are also several points where the discussion can be shortened and/or lengthy footnotes can be abbreviated or removed altogether, which would greatly improve readability. In addition, more clarity regarding the structure of the argument is needed, as it is not always clear how the issues discussed lead to the conclusions set forth on pp. 32–34, and additional section headings would help greatly in guiding the reader along.
Furthermore, careful copy-editing is deemed necessary, as there are many linguistic, stylistic and formal issues that need addressing, as well as a number of mistakes in the bibliography.

Additional Comments

- p. 1, fn. 1: The author states that he has not had the opportunity to consult Wiesehöfer 1978. However, this work is vital for a thorough discussion of the role of the Bisotun inscription, especially "Die Bedeutung der Inschrift als historische Quelle", pp. 3–8, and the passages regarding the identity of Gaumata (74ff.) and on Darius' genealogy (p. 179ff., 199ff.).
- p. 2 ad point 1) the author is certainly correct in that Llewellyn-Jones overstates the facts concerning Cambyses' purported suicide, a suggestion brought forth by Herzfeld and others, however, the interpretation hinges on the interpretation of the expression 'died his own death' (there is ample literature on the subject) which has not been adequately resolved.
- p. 3: "The historical argument […] *contradicts* all the historical sources of the events in question." This is putting it much too generally and placatively. The narrative found in many modern histories contradicts the narrative given in the Bisotun inscription, not necessarily *all* historical sources. The incredulity of the historians stems on the one hand from the unlikelihood of the narrative itself (a member of the royal family being replaced by an impostor without anyone noticing), but also from the extent to which Darius' narrative was propagated in the Achaemenid Empire.
- pp. 6–7: I cannot follow how the impossibility of a linear line of succession 'shows that the picture the historian has drawn of the supposed 'großkönigliche Propaganda' is untenable'. Again, it is unclear to me who is referred to here as 'the historian'. There are a wide range of reconstructions and interpretations of Achaemenid genealogy in modern scholarship, all of which have in common that the sources do not agree and a degree of interpretation is necessary.
- p. 9: The discussion of the number nine in Indian astrology is only marginally relevant to the discussion at hand and can probably be relegated to a footnote or at least significantly shortened.
- p. 10 "Can the historian seriously entertain the notion that anyone would think that a numerical schematism of historical events operated through historiography is a reduction of the terrestrial realm to the celestial order? Can the historian seriously entertain the notion that Darius thought that his audience would be persuaded to accept that his reign represented the divine order because he made the kings nine in number?" Is a king trying to make his achievements fit the divine order of things really such an unlikely concept, especially in the context of the Ancient Near East? Windfuhr's allusion to astrology of course takes it too far, considering that there does not seem to be any special meaning attached to the number nine, but is it really so unlikely that a king who just quelled rebellions by nine kings made himself the ninth king in his dynasty for reasons of symmetry? And once further rebellions took place, the benefit of adding these episodes to the narrative simply outweighed the loss of the symmetry.

- pp. 10–12: the lengthy quote by Rollinger that spans nearly two pages should at least in part be paraphrased.
- p. 14: While the author's analysis is correct for the Assyrian royal genealogies, the reference to an (often mythical) ancestor or family origin is more prevalent in contemporary Babylonia (see the examples in Frame, RIMB 2, for example p. 255, and also various private documents). While Rollinger's assertion that this is an Assyrian topos is certainly incorrect, the genealogy listing father-grandfather-mythical ancestor/name of the house is very much an Ancient Near Eastern topos.
- fn. 86: is the lengthy quote really necessary?
- pp. 16–21: the lengthy discussion of the possible Ancient Near Eastern antecedents, while interesting, can definitely be shortened.
- p. 23, fn. 106: the author is misrepresenting Kuhrt's point: whether or not the new kings are dynastically related is less relevant, but they do not have a firm hold on power and the marriages are an attempt at strengthening this hold.
- p. 33: I cannot follow the line of reasoning that the existence of one alternative account in Herodotus (not necessarily accurate) means that the entire legitimizing programme failed – there must have been alternative narratives circulating in the Persian Empire, unless we posit an extremely thorough brainwashing campaign.

Summary
The aim of the article aligns well with the subject matter of JAAH. The controversy addressed in the article certainly warrants further discussion and it is my opinion that the article should be published pending edits regarding length, clarity and formalia

Authors' comments

I would like to thank the reviewers for their comments on an earlier version of this article. I have carefully considered their suggestions and criticisms and have made requisite changes to meet them where appropriate. In particular, both reviewers wanted me to supplement my critical analyses of the prevalent view of the Bīsotūn account of Darius's rise to power with my own evaluation of this account. I have accepted this suggestion and have explicitly articulated what was implied in my criticisms of the view of the historians of the Achaemenid Empire.

Response to Review 1

Since all views of the historical facts in question are necessarily interpretations of the sources that rely on argumentation, we must ensure that our reasoning is in order. In the article I claim that the arguments put forward in support of the prevalent view are faulty. If I am right in this claim, it means that this view does not have a sound basis. Historical methodology requires historians to reconstruct their pictures on the basis of sources, of course with due critical vigilance. I argue in my paper that historians of the Achaemenid empire do not present sound reasons for their rejection of the accounts found in the sources. I note that the primary ambition of the article is critical and not positive. The reader can decide for herself or himself whether this (preliminary) critical scrutiny is worthwhile and indeed needful for a more robust historical reconstruction.

Response to Review 2

The reviewer says I do "not suggest an alternative explanation for Darius' claim that he is the ninth king in his family." I do not see any reason to discount Darius's claim that he was the ninth king from his family. Unless one can show why Darius should lie about this, it stands. The reviewer criticizes me for using the generic "historian" instead of proper names. I have accepted the point and now use the generic term only sparingly where the context warrants it. I note, however, that in as much as the view under consideration is shared by many historians of the Achaemenid Empire, and certainly by the most influential among them, use of the term "historian" is a justifiable point of reference in some contexts. Prompted by the reviewer I have removed the long quotes, removed or shortened a number of long footnotes, and better signposted my argument.

In a footnote about the controversy regarding the origin of the Old Persian writing system, I say: “As for the genuineness of Cyrus’s inscriptions at Pasargadae, Lecoq’s conclusion is sound.” That footnote refers to a number of works on this topic and briefly discusses a number of issues. The reviewer writes: “the author accepts Lecoq’s suggestion regarding the origin of the Old Persian script without further discussion.” It is plain that accepting Lecoq’s arguments regarding the “genuineness” of Cyrus’s Pasargadae inscriptions does not imply or necessitate accepting Lecoq’s view about the “origin” of the OP writing system. I think I give due consideration (including up-to-date references) to a topic which is not directly related to my theme – which is why it is in a footnote.

I now respond to the reviewer’s specific criticisms.

1. The reviewer duly stated that Wiesehöfer 1978 “is vital for a thorough discussion of the role of the Bisotun inscription.” I find it puzzling though that it finds virtually no reflection in the works published since the early 1980s; a few refer to it but none discusses it in any detail although it holds a position on the issue that is at odds with the currently favored view.

2. I have removed the discussion of Llewellyn-Jones in the second (new) version of the article. Incidentally, the reviewer’s assessment that the question whether Cambyses killed himself, as Llewellyn-Jones claims, “has not been adequately resolved” is unnecessarily skeptical. There is no question that scholars with any knowledge of Iranian languages universally maintain that the Old Persian phrase in question avers that Cambyses died naturally.

3. The reviewer says that the “incredulity” of historians in the face of Darius’s account is due on the one hand to its “unlikelihood” and on the other its being “propagated in the Achaemenid empire.” The second reason is incomprehensible to me: from the apparent fact that Darius wanted to declare to the peoples of the vast empire, many of whom must have been in a state of turmoil, that he had succeeded in crushing rebellions and that he was in control – from this historians infer that Darius must be lying. As for the historian’s “incredulity”, Voltaire thought the Babylonian practice of temple prostitution by upper-class women had to be untrue because it was incredible. The manner the reviewer formulates the issue shows she or he thinks that it could not even be a matter of investigation: “a member of the royal family being replaced by an impostor without anyone noticing.” There is a significant difference between “without anyone noticing” (which Darius does not say) and not allowing it to become public knowledge (which Darius claims).

4. Rollinger argues that Darius devised an elaborate “program of legitimation” whose mainstay was the “linear linkage” of his own family with the royal house of Cyrus. In the context when I use “historian” I primarily refer to him. I have now made this clear. I name other historians in the article who approvingly refer to his treatment of the issue.

5. The discussion of the number nine in Indian astrology is due to Windfuhr’s vague remark about its importance in “ancient wisdom.”

6. I rhetorically ask whether it is reasonable to think that Darius thought it advisable to make a number (i.e., nine) the “organizing principle” of his account, whether it is reasonable to think that he thought that in this way (i.e., by way of this number) he would make his kingdom correspond to the divine realm. The reader is invited to respond in the negative, of course. The reviewer comments: “Is a king trying to make his achievements fit the divine order of things really such an unlikely concept, especially in the context of the Ancient Near East?” But this is a different question from my (rhetorical) question. Ancient Near Eastern kings great and small claimed they represented the supreme god and were his steward on earth. I would not deny this, of course. But it is not my point.

7. I have removed the lengthy quote of Rollinger.

8. The reviewer writes: "While Rollinger's assertion that this is an Assyrian topos is certainly incorrect, the genealogy listing father-grandfather-mythical ancestor/name of the house is very much an Ancient Near Eastern topos." This assertion is incorrect, or at least it still awaits corroborating evidence. The testimony the reviewer adduces certainly does not bear it out.

9. The reviewer suggests to shorten the discussion of ancient Near Eastern background. I have this discussion because historians of Achaemenid Empire invoke the connection in their account of the Bīsotūn Inscription.

10. The reviewer claims that I misrepresent Kuhrt's point, which is, according to the reviewer, "whether or not the new kings are dynastically related is less relevant, but they do not have a firm hold on power and the marriages are an attempt at strengthening this hold." This is not Kuhrt's position in the text I cite, however. Kuhrt writes: "as part of the formulation of the new Persian royal identity, kingship was presented as having been in essence restored, returned to the bosom of Persia's ancient kingly family, when, in fact, this notion of a clearly defined royal line only begins with Darius himself. Darius consolidated *this claim* by several means. Most important was his marriage of his predecessors' wives and female kin, which bound his line to the family of Cyrus" (Kuhrt, 2007, p. 138, emphasis added). Darius marries the women from Cyrus's house in order to consolidate his claim that he belongs to the same "ancient kingly family" whose rightful rule he has restored.

11. I have now made my point about Herodotus clear.