

Прикази Reviews Comptes rendus

On criticism, misrepresentation, and logical fallacy in Egyptology: A response to Aaron de Souza's review of *Ethnic Identities in the Land of the Pharaohs. Past and Present Approaches in Egyptology*

Reading the books *Genesis and Development of a Scientific Fact* by Ludwik Fleck (1981 [1935]) and *Laboratory Life: The Construction of Scientific Facts* by Bruno Latour and Steve Woolgar (1986) was in many ways an eye-opening experience. These authors argue that:

- Research is a social process and is therefore not an entirely objective and value-free enterprise.
- Knowledge is constructed within social contexts and through the actions of researchers. Certain ideas are black-boxed, meaning that they are taken for granted and unquestioned.
- Researchers form communities which can be called thought-collectives and they share thought-styles. What they do is often the result of various social influences, including funding, politics, and personal interests.

I utilized these ideas in my analysis of scientific racism in Egyptology and archaeology of Sudan (Matić 2018a) as well as in my tackling of the legacy of culture-historical archaeology in the archaeology of Egypt (Matić 2018b). These papers prompted the editors of the new Elements series of Cambridge University Press titled “Ancient Egypt

in Context” to invite me to write a short review on the approaches to ethnic identities in Egyptology (Matić 2020). After both editorial and external peer-review, the book was published and has since then mostly received a positive reception, as indicated by, among other things, its quotation rate on Google Scholar (28 times since 2020). However, the book also received other responses, such as negative criticism by Aaron de Souza (2021), parts of which I would like to address here.

de Souza starts his criticism gradually and builds up his arguments toward the total dismissal of my work in his conclusion. Firstly, in summarizing the introductory chapter, he claims:

“Apart from the research questions themselves, the introductory section is otherwise dominated by the chronological overview, despite Matić’s claim that ‘chronological boundaries will not play a significant role’ for the volume.” (de Souza 2021, 347)

This is, however, unfair and untrue for two reasons. The first is that the introduction of the book counts 15 pages and is divided into three sections. The dominating chronological overview de Souza refers to is less than two pages long (Matić 2020, 1-2). The second reason is that this is a logical fallacy, specifically a straw man argument. De Souza attacks a different subject than the topic he discusses and gives an entirely wrong and unjustified

perspective of the book to the readers of his review. The purpose of this misdirection using a straw man argument is to make one's position look stronger than it actually is. If he had informed himself of the goal of the Elements series of Cambridge University Press, he would have known that its publications are not intended to be read by Egyptologists only. In fact, I wrote the book while being based at the Austrian Archaeological Institute, Austrian Academy of Sciences in Vienna and de Souza could have simply asked me for this information, since he wrote his review while being based at the same institute, our offices being located in the same hallway. Furthermore, it goes without saying that a chronological overview is necessary in such a context. One thing that the editors of the series insisted on was providing a background without necessarily using Egyptian dynastic chronology, since not many readers outside Egyptology are familiar with it.

De Souza then turns to my discussion of the visual sources and states that the book:

“focuses heavily on Egyptian iconography that was intended to promote and reinforce an image of Egyptian dominance over outsiders. The first words in the subsection on visual sources are ‘subjugating the enemy’ (p. 3), which immediately sets a singular and one-sided perspective that is skewed towards depictions of violence committed by Egyptians against those perceived as ‘others.’” (de Souza 2021, 347)

It indeed goes without saying that the study of visual sources from ancient Egypt focuses heavily on ancient Egyptian iconography! It is unclear what de Souza expects from a section on ancient

Egyptian visual sources. Furthermore, subjugating the enemy is a well-known motif throughout ancient Egyptian history. Stating that this immediately sets a singular and one-sided perspective is untrue. One needs to actually read my book to know this, as I myself indeed state in this subsection that:

“The disadvantage of visual representations is that they provide us neither with the perspective of ancient Egyptian non-elites, which in some contexts could have been different, nor with the perspective of those depicted. These ideologically framed and culturally relative depictions of foreigners were sometimes approached as accurate representations of reality (for details see Section 2). However, several cases demonstrate that this was actually not entirely the case (for details see Section 3).” (Matić 2020, 4; my emphasis)

However, de Souza deliberately fails to mention this. What troubles de Souza next is the explanation of what is meant by ancient Egypt in my book (de Souza 2021, 347), namely, “a civilisation of ancient northern Africa primarily concentrated in the Nile Valley from the Nile Delta in the north to the First Cataract” (Matić 2020, 6; my emphasis). I am not sure what the issue is here. I doubt that any Egyptologists would argue that what we call ancient Egypt was primarily (and I wrote that word with intention) somewhere else, whether in the north, the south, west, and east of the Nile Valley to the First Cataract. Yes, I wrote, and de Souza acknowledged, that Egypt expanded its borders over time, changing what we refer to when we say ancient Egypt, but de Souza again deliberately ignores an important part of that subsection in which I stated:

“Therefore, when talking about ancient Egypt and ancient Egyptians we have to bear in mind that these terms do not refer to a static society resistant to demographic, social, and cultural change (Schneider, 2003). Strictly speaking there is no such thing as ‘ancient Egypt’ and ‘ancient Egyptians’. The land we refer to as ancient Egypt, although having its core in the Nile Valley from the Delta to the First Cataract, either expanded or lost parts of its territories only to regain control of them again. The people we refer to with the term ancient Egyptians were men, women, and children of different class backgrounds living in different towns and regions of the land.” (Matić 2020, 6; my emphasis)

Quite unprofessional is also his statement that I do not define identity although it is the theme of the book, which is even titled *Ethnic Identities in the Land of the Pharaohs* (de Souza 2021: 347). I do, in fact, define it on several occasions and by referring to the seminal work of Sian Jones (1997), among others (Matić 2020, 9–11, 35–36, 52, 60). Any reader can easily proof-check this. Next, de Souza writes: “The discussion of the concept of race is at times unclear and contradictory, in particular how it relates to skin colour and perceptions thereof in ancient Egypt (pp. 8, 12–13)” (de Souza 2021, 348). However, he does not demonstrate what is exactly unclear and where can the contradictory background of the text be noticed. It is also not true that perceptions of skin colour are not discussed in the book (see Matić 2020: 12, 13, 15, 21–23, 52, 56).”

He misrepresents the book once more when he claims that the introduction:

“would have been an opportunity to introduce the complex problems of equating ethnonyms used in Egyptian texts with actual ancient communities and, even more problematic, with material culture traditions. Kate Liszka’s work on the Medjay is a good recent example of how these issues can be addressed.” (de Souza 2021: 348)

In fact, not only is this very issue addressed in detail exactly how de Souza would have wanted (Matić 2020, 27–30), but the book also refers to the work of Liszka, among the works of others. I again do not see what the problem is!

Nevertheless, the misrepresentation of my book continues in his summary of Chapter 2. De Souza writes that:

“The suggestion that past researchers have not taken into account that the term “Medjay” was an ethnonym used by Egyptians and may not have been used by the people it was intended to describe (pp. 27–28) is untrue. This point has been explicitly made by many authors including Kate Liszka, Hans Barnard, and also by this reviewer.” (de Souza 2021, 348)

I never wrote something like this! In fact, I open the discussion by referring to the works of those who used the term Medjay as an ethnonym for Pan-Grave culture. I then refer to the seminal work of Liszka, and even to the work of de Souza. True, I did not refer to the work of Hans Barnard, but I could not refer to every single work I could or wanted to, due to the word limit in the Elements series being around 30,000. It is not true that, as de Souza claims, I aimed my criticism at recent publications that have already addressed the same challenges. In fact, my criticism refers to

other aspects of these publications. De Souza then refers to, among else, his own book on Pan-Grave ceramic tradition and a paper that he published together with Liszka one year after my book came out! His book is referred to in *Ethnic Identities* and a simple bibliography check can prove it. In fact, it was his duty as the reviewer of the book to check this. I, unfortunately, could not have quoted in 2020 a paper that came out in 2021, since I worked on the manuscript from 2018 to 2019. I am puzzled by the fact that the editors of the *Journal of the American Research Center in Egypt* allowed this kind of criticism to be published in such a prestigious Egyptological journal.

Therefore, it is not true that, as de Souza states, this part of the book (Chapter 2) does not acknowledge the criticism expressed by other scholars. Such accusations are anything but serious. They actually demonstrate another logical fallacy in de Souza's review of my book, the one of appeal to authority, or the misuse of an authority's opinion to support an argument. What actually seems to hit de Souza's nerve is my critique of his own work, for which he does not provide any contra arguments (Matić 2020, 28). Before moving on to review Chapter 3, de Souza again misrepresents my book. He claims that by addressing the problem of using ethnonyms such as Hyksos, but not ethnonyms such as Egyptian, I fail to acknowledge "all of the problems, the challenges of working around them, and the difficulties in moving beyond them" (de Souza 2021, 348). This is again simply untrue and can easily be proof checked by readers (see Matić 2020: 6, 11–14, 61–62). Next, in his final comments to Chapter 2 of my book and the closing section dealing with hidden theory in the archaeology of Egypt, de Souza claims:

"While Matić does raise some important interpretational considerations, it would have been more instructive if the criticism had been balanced by positive examples that would give the reader a sense of how such problems can and should be addressed." (de Souza 2021, 348; my emphasis)

Again, this is simply not true. The following chapters of the book provide ample examples of how such problems were tackled by others. Nevertheless, de Souza again misrepresents the content when he claims that:

"Apart from that, this section is dominated by criticism without indications of alternative approaches, as well as a number of inaccuracies and misrepresentations of the selected sources." (de Souza 2021, 348–349)

It is important to address this statement by going into the details of de Souza's criticism. He claims that I incorrectly read the work of Michele Buzon when I wrote that:

"She argues that being a member of Egyptian society meant having less violent experiences during one's lifetime, and assumes that Kerma during the eighteenth to sixteenth century BC was a 'culture of violence' (Buzon, 2008: 180). However, although Buzon builds her argument of a less violent Egyptian society in Nubia based on skeletal evidence of trauma, she neglects numerous Egyptian textual and visual sources indicating just how violent Egyptian society could also be (Bestock, 2018; Matić, 2019)." (Matić 2020, 39)

In fact, after my book was published Buzon contacted me via the Research Gate platform. In her message from June 22, 2021, she stated that she enjoyed the book very much, found it useful in addressing intersectionality at Tombos, and that I made many valid points and criticisms of work in this area, including her own. She then kindly sent me some of her published work and since then we have exchanged our views on other topics as well, some of which we will hopefully tackle together in the future. Clearly, there is no issue here, since criticism based on arguments is what drives research. The only issue is being made by de Souza who is defending a statement he did not familiarize himself with properly. In order to show that I correctly read the work of Buzon, I will provide a quote from her paper:

“Finally, the adoption of Egyptian cultural standards by the Nubians at Tombos may have been partly responsible for the relatively peaceful coexistence at Tombos as suggested by the low level of traumatic injuries indicative of interpersonal violence.” (Buzon 2008, 181; emphasis mine).

The underlying idea is that adopting Egyptian cultural standards meant becoming less violent. Nevertheless, de Souza jumps to attack my claim that Buzon neglects numerous Egyptian textual and visual sources indicating just how violent Egyptian society could be and follows it with his own comment: “This latter statement is not untrue as ancient Egypt was often extremely violent towards Nubians” (de Souza 2021, 349). He contradicts himself here, as he criticizes a point on which we seem to agree. The reasons for this misleading statement are dubious. De Souza then continues by commenting on my

discussion of Pierre Bourdieu’s concept of habitus in relation to ethnic identity and claims:

“While he does make the necessary point that one must be cautious to not immediately connect an object with an ethnicity, the way in which he supports his argument is problematic, and at times contradictory.” (de Souza 2021, 349)

However, he never states what is exactly contradictory and instead jumps to the question of gender, where he seems to be bothered by the fact that this question has been brought up in the first place. He accuses me of oversimplification in criticism of another author, Bettina Bader, who wrote in one of her papers:

“Thus, it seems likely that the 2.5 % of flat based cooking pots represented in the archaeological record of the settlement were most probably used by women with Syro-Palestinian cultural back-ground or their descendants born at Tell el-Dab’a/Avaris.” (Bader 2011, 65; my emphasis)

According to de Souza, the statement of Bader quoted above is “not so definitive” (de Souza 2021, 249), although the emphasis here speaks for itself and is further collaborated by the fact that after my criticism, Bader (2021a, 79; 2021b, 76) changed her view regarding the gender exclusivity of cooking practices at the site. That my critical reflections were indeed fruitful is also demonstrated by the fact that the most recent work on the topic of flat-based cooking pots of the Middle Bronze Age is careful not to assume the gender of producers and users of cooking pots without strong arguments (Prell 2023).

De Souza (2021, 349) then claims that I contradict myself by stating that women have been living in Middle Kingdom fortresses in Lower Nubia, since I am referring to a seal impression with the name Nebet-Kepeny and title Lady of the House from Mirgissa as possible evidence (Matić 2020, 349). According to de Souza this seal impression “by its very nature almost certainly came from elsewhere” (de Souza 2021, 349). In fact, although I find this to be a valid contra argument, I strongly disagree with the implication that a seal impression has to come from elsewhere. People seal goods within their own communities. Thus, we are clearly dealing with an equifinality problem, women could have been there, but not necessarily.

De Souza (2021, 250) fully misrepresented my reference to Amarna letter EA 4 as an example of subversive mimicry of Egyptian authority. In this letter Babylonian king Kadashman-Enlil asks Amenhotep III requests any Egyptian woman to be sent to him in marriage as no one in Babylon would know that she is not a princess. De Souza fails to mention in his review that I provided the context for this mimicry and an explanation of what Homi Bhabha meant with it, namely questioning of colonial discourses and power relations. Egyptian pharaohs avoided marrying their daughters to foreign rulers, although they gladly married foreign women. In fact, Amenhotep III married his own daughter Sitamun. By showing off with an Egyptian woman as “a daughter of the Pharaoh”, Kadashman-Enlil would elevate his status in the Late Bronze Age “brotherhood of rulers”.

In his summary of Chapter 4 of my book, de Souza continues with unjustified accusations which are easy to proof check. He argues that Stantis et al. (2020) have

demonstrated that the presumed “other” (i.e., the Hyksos) had already long been a part of the Egyptian social landscape. Here he fails to realize that this is old wine in new bottles; my criticism was not, in fact, aimed at the result of stable isotope analyses which showed that some of the inhabitants came from elsewhere. I was very clear in expressing my issue with the fact that the authors of this study equated the term Hyksos with an ethnic group:

“in combination with previous archaeological evidence, this research supports the concept that the Hyksos were not an invading force occupying this city and the upper Nile Delta, but an internal group of people who gained power in a system with which they were already familiar (Stantis et al., 2020, 9).”

The inherent mistake here is that Hyksos is a word describing the ruling class and not an ethnic group. None of the skeletal remains Stantis et al. sampled come from the burial of a Hyksos ruler! Why? Because no such burial has been found to date! Relating the analyzed samples to rulers of the 15th Dynasty is a factual error. De Souza should know this, bearing in mind that he is more than ready to critically address the issue.

In conclusion of his review de Souza summarizes his accusations. He makes several daring claims, bearing in mind that he completely and maliciously misrepresented my book, as I demonstrated in this answer in detail. It is however quite unprofessional to write a review that fully misrepresents the book and then claim that:

“The concern here is that a student or beginning scholar not so

well versed in this topic or existing scholarship may take Matic's words at face value and reproduce the mishandlings of the evidence and literature." (de Souza 2021, 350)

Here de Souza makes another logical fallacy, the one of an appeal to pity, which relies on provoking emotions to win an argument rather than factual evidence. De Souza is worried about students and young scholars! Appealing to pity attempts to pull on a reader's heartstrings, distract them, and support their point of view. Unfortunately, although I have shown that the criticism of de Souza misrepresents my work, some authors uncritically refer to both my book and de Souza's review of my book side by side (see Jurman 2022, 8, f. 45; Prakash 2022, 12, 211). They do not specify what is it they agree or disagree with. This is a classic example of another logical fallacy, the bandwagon fallacy, which assumes something is true because someone else argues it or agrees with it. The problem with this type of erroneous reasoning is that the broad acceptance of a claim or action does not mean that it is factually justified. People can simply be wrong, mistaken, confused, or even deliberately irrational in their opinions, so using them to make an argument is flawed.

Therefore, with this answer to de Souza, I also hope that readers will critically engage with both texts. Nevertheless, I fully agree with de Souza when he writes:

"Criticism is important, and disagreements are inevitable, but two things are essential. First, the work being critiqued must be read carefully and it must be accurately represented. Secondly, criticism should have a constructive intent, especially if it is to drive the discipline forward." (de Souza 2021, 351)

If he indeed practiced what he preached, he would have read the book and the works I referred to carefully and represented the arguments accurately. But he failed.

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