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On “Theses on Indology” by Vishwa Adluri and Joydeep Bagchee

On my four-page review of *The Nay Science* (South Asia 2016, <http://dx.doi.org/10.1080/00856401.2016.1207281>) the authors have written a seventy-page response they characterize as “a Marxist critique” of “the discipline’s superstructure,” that is, of the superstructure of what they consider “Indology”. Perhaps they intend to overwhelm me – and any potential reader interested in the controversy – with this logorrhea, but I am neither fooled nor intimidated by it. In the following rejoinder I cannot address all the issues they raise in this “critique”; I will only rectify and clarify a few essential and striking points.

The definition of Indology: Indology is invariably defined by its scope or subject matter. The OED’s definition is “The study of Indian history, literature philosophy, etc.” The Google definition¹ is very similar: “the study of Indian history, literature, philosophy, and culture.” Alternatively, the term may refer to “the academic study of the history and cultures, languages, and literature of the Indian subcontinent,” if one prefers the Wikipedia definition, and, even broader, to “the study of India and its people” as the Merriam-Webster Dictionary² has it (obviously conceived too broadly). Indology, or German Indology for that matter, does not “largely define itself in terms of a specific method.” as the authors claim. To justify this unusual and untenable assumption they say (n. 26): “It is the Indologists’ own view, found in many writings either as explicit methodological statements or in application to texts.” However, they fail to provide a single source that substantiates their repeated claim. No one would dispute that “references to the historical-critical method can be found in many works and manuals” (n. 25), but none of the quotations adduced in n. 25 demonstrates a definition of the discipline in terms of a method. So until proven wrong, I will continue to maintain what I said in my review: “This is a pure fabrication on the part of the authors.” Even if the authors might find a stray quotation from an Indologist who held a position similar to their view (and so far they have not), this would hardly make their strong claim true and would not represent a generally accepted view among Indologists, not even German Indologists.

Furthermore, it is highly improbable, or indeed impossible, that the various references to method(s) that the authors have collected in n. 25 all relate to method(s) in the strict sense of the method conceived by Johann Salomo Semler. It remains to be demonstrated that the various quoted expressions, such as “historical-philological method,” “a historico-critical approach,” “the *primary method* of a philology,” “historical understanding and critical methods” (note the plural), “modern philological-critical method,” “critical-historical research,” “historical-critical research” and “historical-critical orientation,” all refer to the same unique method, as the authors seem to believe. At least some of these expressions simply relate to Indology as a philological discipline similar in its methods to classical philology, that is, methods that cannot possibly be reduced to Semler’s method or any other single method.

If the authors would have paused to think for a moment, I am sure they would have realized how unlikely their claim is. Is it imaginable that all “German” Indologists used a single method over a period of some 150 years in their research on Vedic literature, Buddhist

¹ <https://www.google.at/search?q=indology&oq=indology&aqs=chrome..69i57j0l5.2917j0j4&sourceid=chrome&ie=UTF-8>

² <https://www.merriam-webster.com/dictionary/Indology>

literature, inscriptions, poetry, theatre plays, astrological and mathematical literature and works of other traditional sciences such as āyurveda, vyākaraṇa, dharmasāstra and alaṅkāraśāstra, philosophical literature, such as nyāya literature, and so on.? Or take grammars and dictionaries of Sanskrit and other South Asian languages, surely cornerstones of the scholarly production of “German” Indology: is it conceivable that identical methods were used in their composition? And even when a scholar works on a single task, he/she would use several methods, e.g., editing a philosophical text, he/she may use one method, another one when translating the text, and a third, fourth and fifth method when studying and interpreting it, e.g., by providing philological, structural and analytical analyses. None of these methods would involve Semler’s method. If the authors were also actual practitioners of Indology, they would perhaps not have fallen for this untenable position. Part of the problem lies no doubt in the extremely limited scope of their book which considers only Mahābhārata and Bhagavadgītā studies. I invite them to take a look at the entire impressive oeuvre of a scholar like Hermann Jacobi, not just his study of the Bhagavadgītā, and see for themselves the absurdity of their thesis. And if the authors leave it open whether their claims can be extended to other scholars working in fields such as the Vedas and Purāṇas and state that this “remains a subject for a separate study” (n. 7.2), why did they give their book the subtitle “A History of German Indology” and not, e.g., “A Selective History of Mahābhārata Studies”?

The authors seem to wonder why I, as an Israeli and Jew and as someone who has written on the relationship between Indology and National Socialism, do not approve of their “central contention, namely, the historical-critical method’s anti-Judaic agenda.” (n. 27) First, I find their book spectacularly uninformed and sloppy in details, manipulative and tendentious. Second, I think that the authors made a categorical mistake in this regard. A method does not have an agenda, nor is it potentially or even per se “anti-Semitic” (nn. 32 and 48). The Semler method may possibly have been conceived with ulterior motives and used by some racists and anti-Semites, but it was also used by Jewish scholars and by non-Jewish scholars who can in no way be accused of antisemitism without any concrete evidence. The authors’ claim amounts to saying that a knife mutilates. One may mutilate something or somebody with a knife, but also may carve beautiful things with it or cut apart things that do not belong together from one perspective or another. It is clearly unwarranted to claim that whosoever uses a knife is *eo ipso* harmful or a murderer. However, this is the simplistic level of the authors’ reasoning. Furthermore, the blamed method is not necessarily exclusive as the authors presuppose; one can use it and still be interested, say, in exploring the Mahābhārata or the Bhagavadgītā as a whole, as unitary and unified cultural products as they existed at a certain time with a certain form (for even in the modern period, we have, e.g., the Bombay, the Calcutta and the Kumbhakonam versions of the Mahābhārata). Their statement that “German Indologists never cared to understand the texts as they existed” (n. 38) is simply not true. Why would “German” Indologists bother to edit and translate entire texts if they were not interested in them “as they existed”? And why would they be interested in *Rezeptionsgeschichte*, if they were not interested in the (changing!) perspectives of the Indian tradition towards texts “as they existed” at a given point in time?

Concerning the use of the word “Indologie” in the nineteenth and early twentieth century, the authors say (n. 20): “The claim that Indologie ‘only came into wider use in Germany after World War II’ is historically inaccurate.” A Google search, however, confirms my general and qualified impression (see the specification “wider use”), though it seems that

compared to the earlier decades the term began to gain currency already in the second half of the 1930s. See the graph produced by the Google Books NGram Viewer under

https://books.google.com/ngrams/graph?content=indologie&case_insensitive=on&year_start=1800&year_end=2008&corpus=20&smoothing=7&share=&direct_url=t4%3B%2Cindologie%3B%2Cc0%3B%2Cs0%3B%3BIndologie%3B%2Cc0%3B%3BINDOLOGIE%3B%2Cc0.

The same holds good for the use of the English word “Indology.” See

<https://www.google.at/webhp?sourceid=chrome-instant&ion=1&espv=2&ie=UTF-8#q=Indology>

(Click on “more” if you do not see the graph.)

In other words, during the period covered in the book, the word is either not used at all or only quite rarely. The authors themselves underscore this point when they mention that the word *Indologie* appears only twice in Volume 1 of Ernst Windisch’s *Geschichte der Sanskrit-Philologie und indischen Altertumskunde*, and not at all in Volume 2 where the adjective *indologisch* appears once.³ Furthermore, I do not understand why the authors expect me to “address the issue of substantial continuity between ‘Sanskrit Philologie’ [sic] and ‘Indische [sic] Philologie’ and ‘Indologie’” (n. 20). I did not write a book on the history of German Indology.

On being German: As the authors frankly admit (n. 12), their definition of a “German Indologist” would include American scholars such as E. W. Hopkins, James L. Fitzgerald and Kevin McGrath. Further, Indian scholars such as C. V. Vaidya, S. P. Gupta and K. S. Ramachandran would have to be added (see n. 30). And John Brockington would be the most German of them all (n. 34, and n. 35: “his work is a pastiche of German views from the past two centuries.”). But surely one may go beyond that. Following the authors, one may consider further areas of Indology and turn many unsuspecting scholars into “German Indologists.” For instance, all Vedic scholars who hold that Maṇḍala 10 of the Rigveda is later than the others or at least the so-called inner Maṇḍalas would have to be designated “German Indologists.” Similarly, all those who maintain that books 1 and 5 of the Nyāyasūtra originally formed a manual of debate and are earlier than books 2 to 4 would have to reconcile themselves to being dubbed “German Indologists” no matter where they come from. Of course, the opposite should be true as well. German Indologists who for whatever reason did not use the Semler method would have to be excluded from the definition and thus cease to be “German Indologists” (in spite of having been born in Germany, having studied there, and having spent their entire career there). If this is the

³ To call Windisch’s *Geschichte der Sanskrit-Philologie und indischen Altertumskunde* and the brief bibliographical notes in Valentina Stache-Rosen’s *German Indologists: Biographies of Scholars in Indian Studies Writing in German* “hagiographies” (n. 4) is a telling example of the arbitrariness and untrustworthiness of the authors’ judgement. Here is only one sample statement in Windisch’s work that would not at all fit a “hagiography”: „Eine gründliche Kenntnis des Sanskrit hat Fr. Schlegel nicht besessen, wie auch vielfach aus seiner Schreibweise der Sanskritwörter hervorgeht ...“ (Windisch, Vol. I, p. 58). To be sure, no one claims that Windisch’s *Geschichte* is perfect, but it is still the best history of Sanskrit philology and “studies of Indian antiquity” and, some 100 years after its publication, still unsurpassed. One can only regret that the planned third volume of this work, which would have treated the contribution of Indian scholars to Sanskrit philology and the study of Indian antiquity, could not be completed. I believe that the authors strongly delude themselves when they boast that their own book “is the richest resource on German Indology to date” (n. 55). And, of course, the authors are right when they state that necrologies are laudatory. What do they expect them to be? Defamatory and denigrating à la *The Nay Science*?

intention of the authors when they claim that they address the question of who those “German Indologists” are (n. 12), I am happy to take back my statement that they do not bother to address this question and replace it with the following statement: The authors arbitrarily define the expression “German Indologist” in such a way that it would include Russians, French, British, Italians, Americans, Japanese, Indians, Austrians, etc., and would exclude Germans working as Indologists in Germany.⁴

Furthermore, the authors’ arbitrary definition is not innocent or candid. Suppose someone were to define Indian nationalists as persons who subscribe to a Fascist ideology, write a (partial and distortive) history of Indian fascism and present it as *A History of Indian Nationalism*. When a reviewer would object to this cheap manoeuvre, he/she would then protest: “The reviewer has not read my book. I have dealt with this problem and clearly admit that some Americans are also Indian nationalists and that there are Indian nationalists who are not Indian nationalists.” Precisely this is the tactics of the authors.

The confidence with which the authors deny simple and well-known facts is astounding. Thus, they contradict me flatly in n. 39.2: “Madeleine Biardeau does not hold that the text [of the Mahābhārata, EF] was composed by a single person.” I did not provide a reference because I assumed that her position is well known, especially to Mahābhārata experts such as the authors. Let me point out to them, for instance, Biardeau’s *Le Mahābhārata* (Paris 1985) vol. I, p. 27: “... le MBh n’est peut qu’être l’oeuvre d’un génie et je ne vois pas ce que l’on gagnerait à le pluraliser.” What were the authors thinking? That I made this all up? Or that I was unable to understand what I had read? So much for their boastful statement in n. 4: “We know the Indologists’ writings and their histories better than they themselves.”⁵

In the same vein, they point-blank deny (n. 57) that either of them “accused Hahn of ‘standing in the tradition of Nazi scholarship.’ Franco refers to the article ‘Pride and Prejudice’ (Adluri 2011b).” Again, do the authors really assume that I would make such an accusation if it were not well documented? I did not refer to Adluri 2011b, but to Hahn 2011 where he says (p. 136): “It seems that he [Adluri], in the same vague and inaccurate manner rightly criticized by Jürgen Hanneder, wishes to place me in the *paramparā* of ‘Nazi Indology’ whatever this *śaśaśṅga* or *vadhyāputra* may be. Not only in view of my published writings, but also in view of my family background this is the most absurd and baseless allegation I can think of.” (Of course I did not refer to Bagchee with this accusation, but to Adluri alone.)

With astonishing confidence the authors also claim (n. 28) that “Origen could not have applied the historical-critical method because it emerged in the eighteenth century, specifically with the work of the Neo-Protestant theologian Johann Salomo Semler.” Do

⁴ Furthermore, if we consider things not from the perspective of persons, but from that of Indological publications, and if we keep to the above definition of “German Indology” as the study of India with the application of the Semler method, we would have to exclude most of the German Indological work from the definition; the PW, for instance, perhaps the greatest monument of “German” Indology in the nineteenth century, would not count as a German Indological work, nor would editions and translations of entire texts, etc.

⁵ The authors state that Biardeau explicitly refers to “groups of Brahmins” as authors of the Mahābhārata in two later publications, but do not provide exact references. In her introduction to Péterfalvi’s translation of extracts from the Mahābhārata (1985) she continues: “S’il le faut vraiment, j’imaginerais un père, un fils, un oncle maternel du père ou du fils travaillant ensemble et dans un coin à l’écart, juste hors de portée de voix, une femme, épouse du père, mère du fils et sœur de l’oncle.” That is, at most she would be willing to assume the authorship of three related authors closely working together (ibid.).

they really think that Semler’s method had no antecedents in antiquity? What was Origen trying to achieve with the Hexapla juxtaposing six textual versions of the Old Testament? Was this perhaps also merely a piece of “mechanical” work? I did not state, of course, that there was nothing new in Semler’s method.

I would not say that to write a history of Indology one necessarily has to be a practising Indologist oneself, but sometimes the authors are clueless precisely because they are not Indologists. Perhaps the most surprising remark in their response concerns the critical edition of the Mahābhārata and specifically their understanding of the reconstruction of an archetype: “the archetype—that is, the latest common ancestor of all extant manuscripts reconstructed using an objective and mechanical method” (n. 39.1). This only shows that they have never edited a text themselves and have no idea what they are talking about. One may argue about the objectivity of the stemmatic method, but what about it being “mechanical”? Sukthankar and his team would turn in their graves if they knew that their strenuous astute work over decades turned out to be considered merely “mechanical.” The above statement is not a slip of pen. The authors repeat this wisdom from their response to Andrew Nicholson’s review.⁶ In fact, the authors do not even know what kind of difficult and highly demanding work an edition based on a single manuscript (especially when partly damaged and containing numerous scribal mistakes) may involve. (n. 11.5)⁷

But there is worse. The authors’ depiction of German Indology borders on delusional paranoia. Thus we read in n. 32: “The Nay Science was written to demonstrate that the Indologists formed a powerful clique that practiced arcane ritual methods, produced flawed and tendentious interpretations of Sanskrit texts, and limited access to these texts in the interests of their own authority.”⁸ A powerful clique? Indologists mostly saw themselves as the poor cousins of classical philologists waiting in vain for the Oriental Renaissance to happen, so that they would finally gain some broader recognition. In any case, having read the entire book, I have found no demonstration of the reasons for this claim. Perhaps in their next response the authors will care to tell us where exactly we can find it. Further, they claim against all likelihood that German Indology had a “coherent ideology” (n. 31). They also claim to know that “Germany made concerted investments in Sanskrit, viewing Indology as an instrument of cultural domination and theological dissemination” (n. 31). Do they mean cultural domination over India? Or over France and other European countries? Probably both, but apparently primarily over India, for they maintain, again

⁶ See “The Real Threat to the Humanities Today: Andrew Nicholson, The Nay Science, and the Future of Philology,” p. 4: “There is a good reason why we defended the work of the Bhandarkar Institute scholars: textual criticism is mechanical, rigorous, and follows objective and explicitly stated principles.” What they probably confuse is that *once a hypothetical stemma has been established*, the choice or rejection of many, but by no means all variants may be largely mechanical. However, the establishment of the stemma itself is hardly “mechanical,” and the final choice of the reading assumed to be have been that of the archetype often requires much reflection, weighting and imagination, especially in the case of stemmatic dichotomies or polytomies when one also has to construe a reasonable scenario to account for the origin of rejected variants with the same probability in terms of the stemmatic position of their manuscript witnesses.

⁷ This is not to glorify my edition of the *Tattvopaplavasimha*, which the authors mistake, or misrepresent, to have given me “the disciplinary entrée.” As I said in the introduction to my extensively annotated translation of this difficult text (p. 66), I could hardly improve on the excellent work of Sanghavi and Parikh and only re-edited the text because their edition had been out of print. Besides, the series in which the book appeared is not my *Doktorvater*’s series, as the authors claim (p. 10) in this connection. I feel honoured to have been Charles Malamoud’s first doctoral student.

⁸ See also n. 11, p. 14: “German Indology is primarily an institutional hegemony.” On p. 3, they accuse Indologists of “polarization of the public,” though it is not clear whether they mean German Indologists here. It is also not clear to precisely what public they refer.

with no evidence whatsoever: “Historically, German Indology’s primary function has been to exercise critical oversight over the Brahmans”(n. 32.3). They further state: “Not only did Germany create publications to disseminate this anti-Brahmanic ideology inspired by Protestant anti-Judaic rhetoric; it also provided a market for these publications by creating departments of Indology” (n. 31). Independently of this funny allegation, how many copies do the authors think were and are bought of any Indological publication by German departments of Indology?

One is left to wonder how German Indology can have a “coherent ideology” and “theology” and at the same time be nihilistic and without meaning: “The title [Nay Science] was thus intended to underscore Indology’s nihilism and its absence of meaning and genuine intellectual value” (n. 68).

With less than four pages, my review is not excessively long and it is also not written in an obscure language; yet the authors succeed to misrepresent and distort more than a dozen of my statements. If they did so on purpose, they are guilty of intellectual dishonesty and of attempting to manipulate their readers; if not, they are guilty of sloppiness and poor scholarship. I invite them to substantiate the following claims or take them back.

- 1) Where did I “invoke traditional hagiographic claims” that Indology is an “unproblematic ‘philological science’”? (n. 9)
- 2) Where did I argue that “only histories written by insiders and practitioners like Windisch are acceptable”? (n. 9)
- 3) Where did I accuse the authors of “self-publishing”? (n. 11, p. 8)
- 4) My wife and I did not “reissue” (n. 11, p. 14) Frauwallner’s *Philosophie des Buddhismus*. We wrote a lengthy introduction to it in which we also dealt with racist aspects of his work, and provided some further supplements to the newly set text of the out-of-print book.
- 5) It is less than accurate to say that I “defend” the work of the early Mahābhārata scholars (n. 36). I said: “One can sympathise with the authors’ claim that the various attempts to stratify the text, and more specifically the Bhagavadgītā, tell us more about the stratifiers than about the stratified” and that “it is easy to point to the weaknesses and prejudices of these pioneering, often over-confident studies.” But even if I would have defended them, how does my “career” as a tenured full professor some three years before retirement “depend on the narrative of a great German tradition of Sanskrit philology”?
- 6) Where did I say that “traditionally the Mahābhārata had an earlier and perhaps more clearly epic version in the Bhārata”? (n. 36) They must be confusing me with Brockington (see n. 34).
- 7) Where did I “argue for purging the [MBh] text”? (n. 44)
- 8) Where did I suggest that one of the *parvan* lists has to be “eliminated”? (n. 45). All I said is that the existence of several such lists makes the assumption of a single author improbable.
- 9) Where did I attempt to associate the book with “Nazism literature”? (n. 58) And what do they mean by “Nazism literature”?
- 10) Where did I “inform” the authors that they “must read Windisch to comprehend German Indology”? (n. 64)
- 11) Where did I say that “*German* Indologists helped Indians rediscover the Bhagavadgītā”? (n. 66) (*italics added*)
- 12) Where did I assert that Indology is a “timeless” science?

- 13) And where did I assert that it is a “value-neutral science”?
- 14) And where did I assert that “the issue [sic] of perspective and privilege do not apply”? (all three in n. 71.1)
- 15) I neither said nor insinuated that the authors “recommend [. . .] reading and using a text like the Gītā as a justification of the abominable concepts and practices of caste distinction, Hindu nationalism, Brahmin supremacy, Right-wing militarism and fascism” (n. 72). They actually quote me here, but by leaving out a few words clearly distort what I said. I wrote: “I am not sure whether the authors realise that what they recommend amounts to an open invitation for reading and using a text like the Gītā as a justification of the abominable concepts and practices of caste distinction, Hindu nationalism, Brahmin supremacy, Right-wing militarism and fascism, to mention but a few possibilities.” Do they really fail to see the difference?

The above samples show the authors’ sloppy, distortive and manipulative way of reading and interpreting a text of less than four pages. As it is improbable that the reading habit of the authors suddenly changed when they wrote their response, one has to assume that they read the thousands of pages of text that form the basis of their book in the very same way and thus can more easily understand the outcome.

There is also some sheer nonsense that crept into the authors’ response which I quote without further comment:

“In our extensive research, we did not encounter a single source where a non-Indologist acknowledged Indology as part of the humanities.” (n. 63)

“Once we discount the Indologists’ fictional histories, we do not know significantly more about the Indian past than before.” (n. 65) “Before” means here “before 1800.”

“India developed historiography in the form of the epics and the Purāṇas.” (n. 65)

Then there are a number of unproven and clearly false statements, such as “We showed that the majority [of German Indologists] were theologically committed Protestants from pastors’ homes” (n. 60). I submit that they did not and would request them to name the pages.

The authors also seem to experience some difficulties in understanding plain English. Thus, they ask (n. 61, p. 46): “Exactly how is this essay [i.e., A.W. Schlegel’s essay; EF] supposed to refute *The Nay Science*?” Very simply: if Indology is modelled after and uses the principles of classical philology, it is neither rooted in Protestant theology nor based on a single method.⁹ Or do the authors think that classical philology is also rooted in such a theology and based on a single method?

In their partial view of German Indology and in their extrapolation from the little they know to Indology in general, the authors can be compared to Chuang Tzu’s frog, for example when they say (n. 38): “In truth German Indologists never cared to understand the texts as they existed. They preferred instead to create fetish texts corresponding to their identity longings.” How is this compatible, for instance, with Jacobi’s detailed summaries of the Mahābhārata and Rāmāyaṇa chapter for chapter? Or with his ground-breaking

⁹ This does not mean that Schlegel’s work was devoid of any national pride, but his nationalism exists side by side with a pan-European vision. For instance, Schlegel thinks that Sanskrit texts should not be translated into German, but rather into Latin. It is also true that later generations of German Indologists felt superior to their British colleagues, but Schlegel himself is painfully aware of how much his knowledge of the Sanskrit language and Sanskrit texts is dwarfed by that of Henry Colebrooke.

translation of Ānandavardhana's *Dhvanyāloka*? Or with Paul Deussen's translation of the Upanishads and of Śāṅkara's entire commentary on the *Brahmasūtra*? Similarly, how is the statement that German Indologists "ignored the commentarial tradition" on the *Gītā* (n. 38) compatible with Hacker's statement that when investigating the meaning of a specific verse he checked nineteen commentaries? See his *Kleine Schriften*¹⁰ p. 542: "Ich habe neunzehn Kommentare durchgesehen, von denen fünfzehn den Vers erläutern." The bibliographical references to the nineteen commentaries are given in n. 31 thereon.

I hardly need to defend my own scholarly work here. However, since the authors mention my wife and mentors, as usual showing much ignorance (n. 11), I would like to inform them that *Alt- und Neu-Indische Studien*, which was not my *Doktorvater's* series (see above, footnote 6), is (or rather was) a refereed series: printing costs were funded by the DFG upon an anonymous evaluation of the manuscripts submitted by the authors themselves. The *De Nobili Series* does not have the referee system the authors seem to be familiar with, but each manuscript is evaluated by several, so far up to seven scholars (the three series editors and five board members, with some overlap) before it is accepted for publication. And since the authors are specifically asking, yes, I did publish in non-Indological, philosophical publication organs, in Hebrew and in English (e.g., in the series *Poznan Studies in the Philosophy of the Sciences and the Humanities*). However, do the authors seriously believe that the Anglo-Saxon referee system guarantees the high quality of all publications reviewed in this way? Or that by definition publications "merely" refereed by highly competent series editors are not worth anything? The system is particularly unsuitable for a small discipline such as Indology. For instance, a look at the names of the publishing houses (with peer-review or not) of books mercilessly criticized or highly praised by the late Willem de Jong, an extraordinary scholar of Buddhism, in the *Indo-Iranian Journal* may be instructive. A blatant example can be seen in a recent publication on Vasubandhu by Jonathan Gold, praised to the sky by four renowned referees. Speaking of praise, none of the reviewers quoted by the authors in their own praise is an Indologist. I am yet to read (or hear, for I have been talking to several Mahābhārata specialists in the meantime) a single favourable review of this book by a Mahābhārata specialist. Are they all in bed with German Indology? Or is this a case of "those who praise do not know and those who know do not praise"?

I did not confuse the two Holtzmanns (not Holzmann, as the authors correctly point out), but referred of course to Holtzmann Jr. with whose work the authors deal extensively; I should have made this clearer. It is true that they deal also with Holtzmann Sr.'s work, but only very briefly (could it be that his study of the epic as a whole does not quite fit their preconceived idea about German Indology?). It is also true that they make some comments on the work of Georg von Simson and Angelika Malinar, but this would hardly turn their book into a history of Indology up to the present time, if this is what they wish to imply. Even the appreciative blurb clarifies that the authors explore "nineteenth- to early twentieth-century German Indology."

Referring to my comparison with Hamlet, the authors say (n. 8): "The real question is: does a review of German Mahābhārata and Bhagavadgītā studies reveal something about the way German Indologists viewed themselves? We contend that it does." Of course it does. Or rather, it would have revealed something if the authors' review were not so one-sided and unreliable. It would have revealed a fascinating chapter in the history of "German" (or

¹⁰ One wonders whether the authors ever opened a volume of this valuable series. In any case, the *Kleine Schriften* are not "transcripts" of published articles (n. 3).

German-language) Indology. But the real question is: Does this chapter constitute a history of “German” Indology as a whole? Definitely not. Is it then at least representative for “German” Indology as a whole? The obvious answer is that it is not.

As I said in my review, *The Nay Science* is a sad book. Sad, one would think, also for the authors who spent many years of their lives to produce such a great deal of nonsense. However, looking at the concoction of abusive and vicious political propaganda that make up this book, one wonders whether they aimed to produce a work of scholarship in the first place. And it is not only the authors’ demonstrated lack of scholarly qualities that I object to, but also their politics which I find ridiculous and repulsive.