**Discovering and Defending Ancient Indian Elephant Science- Piers Locke**

**Abstract**

*What counts as scientific knowledge and how best to evaluate culturally diverse forms of expert knowledge practice are issues that have generated contentious yet productive debate. Arguing for a plural and inclusive understanding of science unconstrained by the colonising, historical prototype emerging from the European Enlightenment, this paper considers the status of knowledge about elephants in Sanskrit literature. Explored in the course of the author’s ethnographic research on captive elephant management in Nepal, this paper challenges the views of some scholars who have dismissed this literature as mere folklore and rhetorical fancy. Noting continuities and convergences with contemporary knowledge and practice, I draw on the work of Sanskrit literature specialists and elephant ecologists to assert the significance of a tradition of applied, systematized knowledge about elephants that fits well with current behavioural and physiological understanding. Finally then, I ask if we should consider the knowledge contained in this literature as scientific, and why it might matter if we do?*

I would like to begin this paper with a few important caveats. Firstly, I am not a historian of science, secondly, I am not a Sanskritist, and thirdly, this paper is only a very rough first draft. Despite this, I hope though you will find that I have something useful to say regarding knowledge of elephants in Sanskrit literature, and that I use this to raise questions about its status as a form of knowledge that some suggest we might consider scientific.

I am in fact an anthropologist, and my interest in elephants began with ethnographic research among mahouts in Nepal. I commenced this research with an interest in apprenticeship learning, skilled practice, and occupational community. Elephants then, were secondary, a position I had to revise. This very particular study of men who live with elephants in and around the Chitwan National Park not only extended historically to consider captive elephant management through time, but also thematically and theoretically with regard to human-elephant relations more broadly.

This led me to develop a framework for studying the social, historical, and ecological intersections between humans and elephants which I call ethnoelephantology. This entails opening up to multiple forms of disciplinary expertise with which we can make sense of the various dimensions of this interspecies relationship, across space, through time, in practice, and in theory. That includes the question of expert knowledge about elephants, which I contend we do not merely find in the pages of contemporary animal science journals, but also in Sanskrit texts like the *Matanga Lila,* the *Gaja Sastram*, and the *Arthasastra,* in the Assamese *Hastividyanarva* (which is illustrated), in the books of the British colonial ‘elephant men’ George Sanderson and A J Milroy, and, of course, in the lived practice of mahouts. These sources, and their significance, are variously addressed in several chapters of the new book from OUP; *Conflict, Negotiation, and Coexistence: Rethinking Human-Elephant Relations in South Asia*, edited by myself and my NZSAC colleague Jane Buckingham from a symposium I convened here in NZ back in 2013. Most significant is Patrick Olivelle’s chapter *Science of Elephants in Kautilya’s Arthasastra.*

I am getting a little ahead of myself though. I think it important that we appreciate how the questions I wish to pose emerged from the enquiries comprising my ethnographic study of captive elephant management in Nepal. For this is a story that tells us something about the sustained, if somewhat attenuated, influence of a venerable knowledge tradition and the dynamics of its relationship with communities of practice. This is a question that Thomas Trautmann addresses in his chapter *Towards a Deep History of Mahouts* in the *Rethinking Human-Elephant Relations* book*.* There, he argues that the mahout emerges as a state-sponsored professional with the invention of the war elephant around 1000 BCE, through relations between kings, forests, and their human and nonhuman inhabitants. This sets up a crucial relationship for the development of expert knowledge about elephants- a community of skilled practitioners on the one hand, with their own oral knowledge traditions, and a bureaucratic structure for managing elephant capture, care, and deployment.

It is this relationship between local workers and imperial administration that allows for the literary codification of knowledge about elephants (and I might add, in a way that almost totally erases the mahouts themselves). This opens the question of whether this knowledge may be considered in some way scientific, or whether it should be dismissed as folklore (in the derogatory sense of its antithesis to scientific knowledge). While this may seem like an ostensibly empirical question (does it contain knowledge that can be effectively applied?), it is further complicated by the notions of science and of folklore underlying this argument, which have gone unaddressed. If we are not clear as to what counts as science, and how practices produce reliable knowledge, then how can we persuasively adjudicate the status of ancient Indian elephant lore?

How then did my research raise these issues, and how does the work of Patrick Olivelle and Thomas Trautmann figure in this? Here, the happenstance of fieldwork is crucial. During my fieldwork in the government elephant stables of Nepal, at a time when elephants and mahouts have been redeployed to serve the interests of biodiversity conservation, protected area management, and ecotourism, an informant brought me a text on elephant care and management, in Nepali, that we date to sometime prior to 1923. I commissioned a translation, and what was immedicately striking was that this text bore the rhetorical hallmarks of Nilakantha’s *Matanga Lila,* a text translated and discussed in Franklin Edgerton’s *Elephant Lore of The Hindus*, a little book published back in 1931 that I discovered in the course of my scholarly research*.*

The Nepali treatise begins with an encomium praising the elephant, that reads in translation as follows:

“The King who is for the welfare and wellbeing of the elephant will be victorious everywhere. The elephant is equivalent to the soul of the king and so the elephant is to be protected in good ways. There is no other thing in this earthly world, besides the elephant, which has a greater power. To be without elephants is like a night without the moon, or the earth without sun-fed paddy. Likewise, if there is a huge army without any elephants, then it cannot be one of any great importance or grandeur”.

This recalls a bygone era in which elephants were integral to the imperial war machine, the legacies of which persist in the symbolic association of elephants with state power (evident for instance in the elephant-back coronation of the late King Birendra in Kathmandu in 1975, and also in the state use of elephants for managing protected areas that I investigated ethnographically in Chitwan). Significantly though, it is also redolent of Nilakantha’s *Matanga Lila*, and I found the treatise’s concerns with elephant classification by type, by age, and by behaviour, with elephant anatomy, and with methods of elephant capture, to be consistent with it, in form and content, to a notable degree. In 1931, Edgerton argued that the substantive content of the *Matanga Lila* represents a codification of orally-transmitted traditions of practical knowledge. He supported this claim by noting over 130 technical terms without clear Sanskrit etymologies, which he took as an indication of their appropriation from vernacular lexicons.

He further notes parallells between the content of the *Matanga Lila* and the local knowledge reported in George Sanderson’s *Thirteen Years Among The Wild Beasts of India.* Sanderson had been in charge of elephant catching operations in Mysore and Bengal for many years, at a time when British colonial interests were putting elephants to work in the logging industry. Sanderson was one of the few British commentators to pay heed to local knowledge. He was not familiar with the Sanskrit literature on elephants, yet Edgerton claims he reported a modern elephant lore with striking resemblance to it. As Edgerton contends: “in general it seems to me hardly doubtful that we are dealing with a genuine, ancient, and persistent tradition of elephant-lore, which grew up in and around the elephant stables of Indian potentates.”

Several years later, I came across a commentary article in the 2001 issue of Gajah, the journal of the IUCN’s Asian Elephant Specialist Group, with the title *Abusing the elephant: pseudo-specification and prognostication in ancient elephant lore.* Its author, Merlin Peris, takes a very different view, suggesting that “some old manuscripts on elephants which have gathered together folklore once prevalent in India and Sri Lanka… perhaps from sentimental reasons, have so far not been shown up by scholars for what they really are- a lot of baloney!” He does not mince his words!

Peris constructs his playfully polemical argument, without reference to Edgerton, through a very selective use of texts that includes the *Matanga Lila,* but disregards the *Arthasastra,* a crucial omission as we shall see. He also does so through a highly selective focus on classification, and a ridiculing concern with the portentous qualities ascribed to elephants. To be fair, Peris does not claim to be conducting a systematic analysis, and while I think he is excessively dismissive, I do think his provocation is useful, and that his arguments, unconvincing as I find them to be, warrant critical deconstruction.

Let us consider first, that Peris suggests, as with Edgerton, that these manuscripts, I quote; “gathered together folklore once prevalent in India and Sri Lanka”. At this stage the manuscripts go unspecified, allowing for a blanket denunciation. Next, he proceeds to discuss the etymology of elephant in Greek as it leads to the taxonomic categories we are familiar with in the form of Latinized binomial nomenclature (*Elephas maximus, Loxidonta africanus,* and *Loxidonta cyclotis*). For Peris, this is specification that stands as a proxy for scientifically ordered knowledge, against which he contrasts the caste-like specifications of the Sanskrit and Sinhala literature, complaining that the Indians have fallaciously distinguished tusked from tuskless elephants. I’ll come back to the issue of caste. He seems to enjoy scoring points, suggesting that despite their lack of direct experience, the Greeks classified elephants correctly, by which he means distinguishing the African and Asian species, even though he fails to recognize that knowledge of African elephants was not flowing back to inform the Indian tradition, at least not in a way that was evidently documented. Incidentally, this absence recalls Willhelm Halbfass’ concern with the Indocentric character of Sanskrit literature, which tells us so little about the history of India’s cross-cultural encounters. Back to the privileging of Greek classification though - This is where I think he falls foul of a category error. Let me quote:

 *Thus it is with no zoological difference, a distinction has been struck between two animals of the same species on the basis of what is no more than an “accident”- the possession and non-possession of tusks.*

He seems to be treating *species* not merely as the referent of a taxonomic system, but rather as an essential and inviolable type that mirrors the world rather than constructing our understanding of it. To me, this reeks of the positivist fallacy of reification- conflating categories (that serve a purpose within the particular rationale of a system of classification) with entities (in this case, actually existing beings we refer to by the arbitrary linguistic sign of ‘elephant’). For Peris, it seems, the Greek reflections that led to binomial nomenclature are correct and worthy, limited as they are in their explanatory utility, while the considerably more complex specifications by age, type, and behavior, deriving as he implies, and Edgerton contends, from folk experience, are neither correct nor useful. Indeed, he shows no concern with their utility, even as he remarks on indigenous schemata that distinguish elephants by each decade of their lives (which also appears in the Nepali treatise I have mentioned). This is the kind of specification one can imagine to be useful in a culture that harnesses the labour of elephants. Perhaps if Peris had considered the broader significance of classification as an anthropological phenomenon, applied in many ways, in many contexts, by different agents, in different times and places, then he might have recognized binomial nomenclature as but one instance, deriving from a particular situation in service to particular goals (which I would describe as Western modernity and the scientific knowledge supporting it).

My concerns though are not limited to his conceptual logic, but also to his empirical claims. The presence or absence of tusks *is* a matter of zoological difference, which is not the same as the taxonomic difference that concerns him (he has conflated zoology with taxonomy). Nor is it merely the result of “accident”. He seems to have neglected the evolutionary processes that select for tusklessness, including the selective pressure of human predation and exploitation that help explain the proliferation of tusklessness in Sri Lankan elephants. In 1995, the late Fred Kurt and colleagues argued that this is a man-made phenomenon through practices of hunting and capture that preferentially selected tusked males. At this point, you may be wondering if Peris is at the limits of his expertise as a humanities scholar, which indeed he is. This speaks to the necessity of collaboration and cross-disciplinary dialogue, something which I think the *Rethinking Human-Elephant Relations* book testifies to- at the symposium from which it derives, the scholars and the scientists talked together and educated each other.

Now to caste. Peris suggests that while the Greeks classified zoological types as distinct from each other, the Indians merely assigned castes. The irony here though is that the meaning of the Hindi word for the originally Portuguese term *castas* is *jati,* which has been translated to mean type, kind, or even species, alerting some anthropologists to the fact that viewed from the inside, the hierarchical phenomenon of caste also discloses an essentialist logic of types, which I have argued elsewhere makes it well suited for application beyond the human. Peris though, seems to be in thrall to Orientalist understandings of caste long promoted by Western scholars, which tended to view it merely as a kind of social apparatus, despite the role of ingested substances in its differentiating idiom of purity. McKim Marriott is one anthropologist though who took local understanding seriously, pointing to the role of the humoral *triguna* to explain castes as social groups with differing ‘substance-natures’ as he terms it.

Okay, so I have provided some objections to Peris’ wholesale dismissal, noting his failure to engage with the full range of this literature and the substantive range of its content, as well as his failure to fully recognize that it represents the codification of subaltern knowledge. As Thomas Trautmann remarks: “texts attribute the knowledge derived from elephant-men to the mythical past, and to an inspired sage. The mystification of subaltern knowledge serves to exalt it by attributing it to more exalted sources.” I cannot help but wonder if he has been seduced by familiar modernist prejudices- if it doesn’t conform to the recognizable forms of the knowledge tradition that first laid claim to the title of science, then it can’t be science. As I suspect the participants here can appreciate, one can find Sanskrit and vernacular literature testifying to vibrant knowledge traditions that combine the mythic, poetic, scholastic, and scientific in ways that long confounded Western scholars with their categorical assumptions and genre expectations.

Now I would like to very briefly turn to Patrick Olivelle’s analysis of Kautilya’s *Arthasastra,* a famous manual that includes the role of elephants in statecraft. As I have written “Patrick Olivelle’s analysis of knowledge pertaining to the management of captive and free-roaming elephants in the Arthasastra surely represents one of the most significant contributions to our understanding of Sanskrit elephant lore”. While it alerts us to valid knowledge about elephants and their management, it should also remind us of a crucial difference from the majority of texts discussed during this symposium, which attest to scholarly schools of thought that I do not think can be claimed here, for I have argued that this ‘elephantology’ represents an applied form of knowledge largely derived from non-literate expert practitioners.

Olivelle dates the text to the first century CE, claiming that scholars wrongly assigned it to the Maurya period. He remarks that: “The Arthaśāstra, for the most part, is hard-nosed and only infrequently gets into idle speculation”, noting that “it provides the most ancient technical description of the elephant/ human interaction available for South Asia”. This is why Trautmann finds it useful in constructing his deep history of mahouts. Indeed, its coverage is impressive- quote p10/11 of book.

This does not sound like the baloney that Peris attempts to serve us. But is it science?

I shall conclude with an observation from Raman Sukumar, India’s leading elephant ecologist, who has noted that the literature that Peris dismisses as baloney includes a detailed and informative account of musth, a state of hormonal agitation in male elephants, that prefigures the findings of Western science by centuries!

References