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Contents

Contents	3
List of Contributors	5
Editorial. RICHARD GOMBRICH	7
Brahmā's Invitation: the <i>Ariyapariyesanā-sutta</i> in the Light of its <i>Madhyama-āgama</i> Parallel. ANĀLAYO	12
Ambitions and Negotiations: The Growing Role of Laity in Twentieth-century Chinese Buddhism. EYAL AVIV	39
A comparison of the Pāli and Chinese versions of the <i>Devatā Saṃyutta</i> and <i>Devaputta Saṃyutta</i> , collections of early Buddhist discourses on <i>devatās</i> "gods" and <i>devaputras</i> "sons of gods". CHOONG MUN-KEAT	60
Some Remarks on Buddhaghosa's use of Sanskrit Grammar: Possible Hints of an Unknown Pāṇinian Commentary in Buddhaghosa's Grammatical Arguments. A.M. GORNALL	89
The Legend of the Earth Goddess and the Buddha. PAISARN LIKHITPREE-CHAKUL	108
Ven. Walpola Rahula and the politicisation of the Sinhala Sangha. SUREN RAGHAVAN	114
A Buddhist ballad from Bangladesh: the <i>Sambhūmitta Pālā</i> . An apocryphal <i>jātaka</i> from Southeast Asia to an Indian tune. PAOLA G. TINTI	134
An Early Korean Version of the Buddha's Biography. SEM VERMEERSCH	197

Book Reviews

Greater Magadha: Studies in the Culture of Early India by Johannes Bronkhorst. Reviewed by Richard Fynes 212

The Genesis of the Bodhisattva Ideal by Anālayo. Reviewed by Richard Gombrich 216

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Book Reviews

***Greater Magadha: Studies in the Culture of Early India.* By Johannes Bronkhorst (Handbook of Oriental Studies, Section Two, India, Vol. 19). pp. xx, 420, Leiden and Boston, Brill, 2007**

Professor Bronkhorst's book is a valuable contribution that will stimulate debate among scholars and students and encourage them to re-examine ideas about the nature of early Indian culture that are often taken for granted. However, not all scholars will agree with his conclusions. *Greater Magadha* largely consists of Bronkhorst's arguments in favour of his belief that in early India the northwest was the centre of a Vedic culture that was primarily concerned with ritual performance and magical thought and that this culture was distinct from and diametrically opposed to that of the northeast, Bronkhorst's Greater Magadha, which was primarily concerned with world renunciation and empirical thought. Bronkhorst argues that Buddhism, Jainism and the *śramaṇa* movements in general developed out of the specific culture of Greater Magadha, as did the belief in karma and retribution, which subsequently entered mainstream Vedic culture. There is not space in this short review to address Bronkhorst's arguments with the detailed response that they deserve; its purpose is rather to present an overview of the strengths and weaknesses of the book.

The reader of *Greater Magadha* would probably begin by wondering what was the actual geographical extent of Greater Magadha and within what chronological limits was its *floruit*. These are questions that Bronkhorst does not address with sufficient exactitude. For Bronkhorst the entire region east of the confluence of Ganges and the Yamuna was Greater Magadha, and he states that "it serves no purpose... to define exact limits for it". Furthermore, as he admits, Greater Magadha is a term invented by himself and the name Mahāmagadha does not appear to be found in ancient Indian literature. "Greater Magadha" is thus an artificial

construct, one that can be seen as a strategy which enables Bronkhorst to present a wide range of disparate evidence in support of an hypothesis which is basically a simple polarity. But without grounding in a historical context of time and place, the evidence can do little to establish the central thesis.

A study that gave closer attention to geographical particularities and chronological change would doubtless reveal a more complex reality. It is certainly true that the *śramaṇa* ethic was transmitted through the expansion of Magadhan power and prestige and that it received state support under the Mauryan empire, but the *śramaṇa* movements seem not to have originated within the borders of the historic *janapada* of Magadha, since both the Buddha and Mahāvīra appear to have spent their formative periods in countries whose centres were well to the northwest of the historic *janapada* of Magadha. The most important of these centres was probably Kosala, whose more westerly situation would have opened it to Āryan culture at a period earlier than were the eastern *janapadas*, including Magadha. Kosala seems to have become subject to Magadha during the lifetime of the Buddha, but it could well have happened that *Kosala capta ferum victorem cepit*¹ and that Kosalan culture, having initially developed as a result of an interaction between Vedic and non-Vedic cultures, was absorbed by Magadha and was subsequently exported along with Magadhan power.

One feels that Bronkhorst's simple hypothesis leads him to develop arguments in its support that are in places too complex to be convincing. A central part of his argument is that the teachings of the Buddha do not presuppose knowledge of the Vedas and earliest Upaniṣads. For example he argues, *contra* Richard Gombrich, that the passages in the Pāli canon in which the Buddha appears to satirise the Ṛg Vedic *Hymn to the Cosmic Man*, the *puruṣa sūkta* RV.x.90, do not preserve the actual response of the Buddha to the social hierarchy that the hymn seeks to validate. Bronkhorst argues that the cosmic man was a well-known theme of Indian mythology so it is not surprising that there are references to it in the Pāli; he seems to be suggesting that the passages entered the canon at some late though unspecified date. A simpler explanation would be that the hymn was already well known during the period of the Buddha's life and that, as argued by Gombrich, the pas-

¹ Meaning "Captive Magadha took captive her wild conqueror". I have adapted the Roman poet Horace's *Graecia capta ferum victorem cepit* (*Epistulae* II 1). Horace was making the point that the Roman conquest of Greece enabled Greek culture to spread throughout Rome. The same may have been true of Magadha and Kosala.

sages preserve the Buddha's characteristically satirical response to Brahmanical claims.

One serious weakness of *Greater Magadha* is the lack of an adequate assessment of developments in material culture. One important development, which must have had an impact on social relationships in ancient India, was the use of coined money. Bronkhorst does not discuss the evidence provided by numismatics in his section on urbanization, even though coinage in India is a key example of the adoption of a concept that originated outside Brahmanical culture. The earliest Indian coins were produced in Kabul and Gandhāra in the early fourth century BC at a time when these regions were part of the Achaemenid empire, and shortly afterwards coins were being produced in large quantities in the countries bordering the Ganges valley. There is evidence to suggest that the earliest coins of the eastern regions were produced in Kosala. Early Indian coins are now known as punch-marked coins, since they are typically formed from pieces of silver, scyphate or flat, cut from larger sheets, and usually bear from one to five symbols, some of which clearly have a religious significance, stamped separately onto one side of their surface. Punch-marked coins were being manufactured and were circulating in the area Bronkhorst calls Greater Magadha perhaps no later than twenty-five years after the death of the Buddha. Coined money facilitates the redistribution of wealth and it may be that one of its original purposes in India was to enable kings to make payments to Brahmins in return for their performance of Vedic sacrifices. Coined money also facilitated donations by merchants to the Buddhist *saṅgha*, as is evidenced by visual representations from Buddhist monuments dating perhaps from the second century BC. Furthermore, an increased production of coinage may well imply an increase in exchange transactions that are not based on an asymmetrical hierarchical relationship between giver and receiver, but imply equality, since the participants' relationship is often temporary and is determined solely by their willingness to give and accept money in the form of coins. Money facilitates anonymous exchange and thus enables its possessor and receiver to enter into relationships that are based on choice rather than on ascribed status. The growing availability of coined money may well have had a profound influence on social relationships and cultural expression in ancient India.² No study of the culture of early India should ignore the potential of the

²Richard Seaford has discussed these questions with regard to the Greek world in his *Money and the Early Greek Mind: Homer, Philosophy, Tragedy*, Cambridge; CUP, 2004. I hope to join Professor Seaford in a study of the significance of coined money for early Indian society.

evidence provided by numismatics.

Greater Magadha has its origins in papers previously published by Professor Bronkhorst over a number of years, and the book's subtitle, *Studies in Early Indian Culture*, is an accurate description of its contents and structure. The emphasis of the studies is primarily literary and there is little discussion of social and political history. Bronkhorst demands fairly rigorous attention from his reader; his material is densely presented, and this reviewer felt that he could have provided more signposts to enable the reader to follow his arguments. For instance, it would have been useful to have a summary of the evidence at the end of the important section on chronology. Furthermore, the material in the eight appendices, which fill ninety pages of text, could have been more closely integrated into the main body of the book. Overall, *Greater Magadha* is a quarry to be mined rather than the presentation of an argument to be followed. Bronkhorst's knowledge of the literary sources is profound and his presentation of the literary evidence is exhaustive. On almost every page there are ideas and suggestions that will stimulate debate among scholars and students. The book has great strengths and if this review has concentrated on some of its weaknesses this is by no means to detract from Professor Bronkhorst's achievement. All those with an interest in early India should own a copy of *Greater Magadha*, at least in its readily available e-version, if not in its exorbitantly priced hard-copy version.

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