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## Reviews of Books

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CATALOGUE OF THE BABYLONIAN TABLETS IN THE BRITISH MUSEUM, VOLUME III. By M. SIGRIST, R. ZADOK and C. B. F. WALKER. pp. 239, London, British Museum Press, 2006.  
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This is the third volume in a projected series of eight cataloguing the Babylonian (which here means anything other than Assyrian) tablets in the British Museum. The catalogue covers Ur III tablets from Girsu (Tello); Old Babylonian archives from Kisurra (Abu Hatab), Larsa (Senkereh) and Abu Habbah (Sippar); Nuzi tablets from Nuzi or Kirkuk; and Neo-Babylonian tablets from Borsippa and Babylon. Internal evidence in the texts suggests that some also came from Dilbat, Drehem, Kazalla, Kish, Larsa, Nippur, Sippar, Umma, Ur and Uruk. The presumed provenance for each item is given an entry in the catalogue. These texts come from seventeen different collections in the museum, all acquired by purchase in 1898 and 1899, and amount to more than 7,000 items. A surprise was the number of texts from Kisurra (the site was subsequently excavated in 1903). The vast majority of these texts are Ur III (more than 5,000) with Neo-Babylonian texts accounting for around 1,000 more. As regards the contents, the overwhelming majority are administrative in nature (this includes the bullae), though in the Old Babylonian holdings there are a small number of literary texts (mostly Sumerian), an even smaller number of mathematical texts and a sprinkling of a others genres (e.g. lexical texts, school texts and rituals).

One excellent feature is that the catalogue not only notes the presence of sealings but in the case of the Ur III material gives, as far as possible, the full text of the seal impressions as well; the catalogue also notes when the tablet has an inscription identifying the seal. This attention to sigillography is now a hallmark of the work of the Department of the Ancient Near East and is very welcome.

Another great advance on earlier volumes, much to be applauded, is that for the Neo-Babylonian texts an attempt is made to identify the archive from which they came. The authors list at least forty-six separate archives from Borsippa alone. Of particular interest are Šaddinnu's activities overseeing the work of bakers in the Ezida temple preparing food for the main (*rabû*) and second (*tardinnu*) courses of the meal of Nabû in the morning and evening<sup>1</sup>.

Last but certainly not least the array of indexes is impressive. For the Ur III texts there are indexes of tablets by reign, of the personal names occurring in the texts, of personal names occurring in the seal impressions, of the kings/deities to which seals were dedicated, of technical terms and officials (including owners of seals) and a short general index. The last notes sixty-one forgeries of Ur III documents in the collection. For the Old Babylonian period there are indexes of personal names, seal impressions, provenance, and a general index. For the Neo-Babylonian period the indexes list texts by regnal year, personal names, place names, seal impressions, provenance and again a general index.

<sup>1</sup> For the latest on these archives see M. Jursa, *Neo-Babylonian Legal and Administrative Documents*, GMTR 1 (Münster, 2005).

More compact indexes also treat individually the material from other periods – Early Dynastic (2 seals), Pre-Sargonic (5 tablets, 1 vase, 5 bricks), Old Akkadian (72 tablets, 4 seals, 1 bulla), Lagaš II (3 tablets, 2 dedicatory inscriptions, 1 brick), Post Akkadian (1 cylinder seal), Nuzi (97 tablets), Kassite (2 seals, 1 terracotta figurine), Neo-Assyrian (1 prism) and Sassanian (1 bulla, 6 seals). Finally, there are indexes of divine names, field names, toponyms and month and year names.

In conclusion, the authors are to be warmly congratulated for presenting this volume to the world. The upgrading of the quality of catalogue entries is highly commendable and putting all this material at the disposal of the Assyriological world will undoubtedly set in train a wave of primary scholarship. To all, our thanks!

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THE HEAVENLY WRITING: DIVINATION, HOROSCOPY, AND ASTRONOMY IN MESOPOTAMIAN CULTURE. By FRANCESCA ROCHBERG. pp. xxvi, 331. Cambridge, Cambridge University Press, 2004.  
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It is often lamented that comparatively few books build bridges between Assyriology, a mostly inward-looking discipline, and other fields of study. This volume is a welcome addition to the number of such books. The author has spent over two decades working with Babylonian astrological texts, especially lunar-eclipse omens and horoscopes, and has given much thought to the particular issues that they raise in assessing the contribution of Babylonian civilisation to the intellectual history of mankind. Her chief aim in this latest book is to place Babylonian astronomy-astrology firmly in the context of the history and philosophy of science. The result is a volume that will be compulsory reading for all interested in ancient science.

In Chapter 1 (The historiography of Mesopotamian science) Rochberg examines the evolution of the responses of historians of science to the astronomical-astrological evidence from Babylonia; usually those interested in science as theory have dismissed Babylonian scientific texts as belonging to practical and religious genres and of little consequence for their own field. In Chapter 2 (Celestial divination in context) the Babylonian astrological texts are examined and found, as “systematically acquired corpora of ‘what was known’”, to deserve the status of science.

In Chapters 3 (Personal celestial divination), 4 (Sources for horoscopes in astronomical texts) and 5 (Sources for horoscopes in the early astrological texts). Rochberg returns to the Babylonian horoscope and finds the astrological principles on which they are based to be consistent with Babylonian celestial divination. In this way Babylonian horoscopy is to be differentiated from Greek horoscopy, which came later and had other central concerns. In Chapter 6 (The scribes and scholars of Mesopotamian celestial science) she examines the sociological context of Babylonian astronomy and astrology. Noting the rise of personal astrology as an innovation of the late period, when Babylonia was no longer an independent state, Rochberg finds no explanation in “political or social factors”. It seems to me that the history of Babylonian horoscopy is intimately bound up with the evolution of intellectual life and political patronage. Celestial divination was a tool of statecraft. After the fall of the Neo-Assyrian and Neo-Babylonian states, whose governments made full use of astrology, and the transfer of political power to Achaemenid Iran, the state had little need for the traditional products of the Babylonian astrologers. They found new business by developing their science to cater to the needs of the great and powerful families who ran the country almost as feudal fiefdoms. Thus horoscopy arose in late fifth-century Babylonia. In due course, the language and writing technology of Babylonian horoscopy

became obsolete and horoscopes in cuneiform died out, to be replaced by horoscopes written in the new language of science: Greek.

Chapter 7 (The classification of Mesopotamian celestial inquiry as science) returns to the issues that inform the book's opening chapters. Rochberg presents evidence for the epistemological basis of Babylonian celestial science, exploring the complex relationships between protases and apodoses of omen texts, and examining the nature of Babylonian theories of astronomy.

The book closes with an epilogue which observes that in Babylonia the practice and development of science was carried out by men who believed that what they observed was made visible by divine authority. Modern scientists may scoff at this considering it to be irrational, but what Rochberg reports is not such an alien way of practising science as it may seem. In a popular book called *Straw Dogs: Thoughts on Humans and Other Animals* (2003), the philosopher John Gray takes issue with the current notion of science as rational and disinterested. He points out how many of the great figures of early modern science worked to substantiate religious dogma and observes that science has always been the servant of its time. Scientists, Gray concludes, have never been free of the burden of preconceived ideas: "beliefs that today are regarded as belonging to religion, myth or magic were central in the worldviews of the people who originated modern science". In that respect Babylonian astronomers were scientists comparable with Galileo, Newton and Kepler. Scientific theory and irrational belief have coexisted in the same minds for most of human history.

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OSMAN'S DREAM. THE STORY OF THE OTTOMAN EMPIRE 1300–1923. By CAROLINE FINKEL. pp. xix, 660. London, John Murray, 2005.  
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In her preface, Caroline Finkel explains "the conceit behind the title of my book" (p. xiii). The allusion to Osman's dream is balanced by the 'dream' of Atatürk presented in his famous speech in 1927, an apologia for the events leading to the creation of the Republic and a vision for the future. This "conceit" does not, however, appear on the book's cover where the date given is 1923.

The book traces the history of the empire from the early beginnings of the Ottoman state through into the Republic in a series of nicely entitled chapters such as 'The sedentary sultan', 'The perils of insouciance', and 'The storm before the calm'. The author is most comfortable in the seventeenth century which receives proportionately more coverage than the other centuries. It is here that the book is at its best. Finkel uses many primary sources and the account is convincing and lively. This is less so for the earlier centuries where her account tends to be more a reproduction of a traditional approach. She describes the battle of Kosovo, for example, as signalling "the end of the independent Serbian Kingdom, and confirm[ing] the permanence of the Ottoman presence in the Balkans" (p. 21). The battle, however, was not as clear cut as this, and indeed the earliest extant accounts of it did not see it as a crushing Ottoman victory but rather as an Ottoman defeat. The idea, too, that Ottoman advance was hostile to Genoese and Venetian interests is somewhat of a simplification, while the presentation of Ottoman disinterest in economic affairs is an approach which is now increasingly under scrutiny. That capitulations were bad because they had disastrous consequences several centuries later does not necessarily mean that their use in the fifteenth century did not have sound economic benefits for the state in this earlier period. The weakest section of the book is that on the nineteenth and early twentieth centuries where Finkel's narrative would have been greatly helped, for example, by a use of memoirs of the period.

In contrast to the recent general history of the Ottoman empire by Justin McCarthy (*The Ottoman Turks. An Introductory History to 1923* [London, 1997]) where the author divides the sections into political history and then social and economic themes, Finkel has chosen to incorporate the material into one continuous narrative. Although she acknowledges that readers might on occasion find it easier to follow if “unfamiliar elements like the janissaries or the *harem* were treated separately, outside the main flow of the text” (p. xiii), she argues that “these features are integral aspects of the society which produced them, that they did not exist in a vacuum” (p. xiii). Thus a chapter entitled “Islam” does not “make any sense . . . since religion is a major dynamic force in history, and the way it is practiced at any time or in any place has political repercussions” (p. xiii). For Finkel “viewing history through ‘institutions’ tends to freeze-frame what was dramatic, and obscure the interconnections between related events. It has the further drawback of encouraging the reader to seize upon the very aspects of Ottoman history that have so often been treated pejoratively, without explaining how they arose and why they developed as they did” (pp. xiii–xiv).

This is a very good point and the isolation of history into themes does indeed have the disadvantage of isolating features and smoothing over mutations and shifts, leaving the institution timeless and ‘history-less’. Constructing a narrative which explains as a whole both events and social, and economic, developments is an excellent ideal. It is, however, extremely difficult to do, particularly over such an enormous time span.

This problem is obvious in the chapters dealing with the nineteenth and early twentieth centuries. The Young Ottomans are almost totally absent, and there are barely a handful of references to Namık Kemal. The Young Turks, too, are conspicuous by their quasi non-existence. Mizancı Murad does not appear at all. There is no discussion of identity, despite the title of the chapter, “A crisis of identity”, no Yusuf Akçuraoğlu and no Ziya Gökalp. The intellectual turmoil of the nineteenth century does not therefore come over. This crisis of identity and the search for survival was not limited to ideas but was also driven by the very concrete presence of the Europeans who came to control a large sector of the Ottoman economy after the state went bankrupt. This, too, is not discussed and the Public Debt Administration is only mentioned in passing at the beginning of the following chapter.

Finkel further states that her book is “an attempt to interpret Ottoman history by the same standards as other histories hindered, and that history made to seem unique. There are unique aspects to the history of every state, of course; but to emphasize them rather than the aspects that are comparable to the history of other states seems to me to miss the point” (p. xiv). Over-emphasis of difference, very much a feature of earlier European approaches to the empire, does obfuscate understanding. However, surely emphasis of comparable aspects is also an unbalancing of history and does not necessarily help one understand without any preconceived ideas the society that one sets out to study. It falls somewhat into the very understandable modern-day desire to stress that the Muslim-Christian divide, a construct largely of modern political requirements, does not represent reality, a point to which Finkel alludes in her preface.

In explaining her approach to writing this book Finkel notes that “a strong narrative line seemed desirable” (p. xiii). This is what you get: very much a ‘kings and queens of Turkey’ history. But it does not give a feeling of the life of the empire. It is the view from the palace.

The book ends with a political comment: “The military and their civilian supporters who appointed themselves to share the burden of guarding the Turkish state and the fanning of the flame of Kemalism perpetuate and interpret Mustafa Kemal’s legacy to convince the citizens of the modern republic to conform to the values which he embodied – not only its secularism and forward-looking modernity in public life, but also such authoritarian inclinations as the crushing of dissent and constraints on the freedom of speech. Mustafa Kemal’s actions were informed by the perils of the years during which he

was in power; but times have changed, and solutions prompted by the ideals and fears of the 1920s are not best suited to the problems and challenges of the twenty-first century. The past weighs heavily, however, and many Turks would disagree with the disparaging western view associating Kemalism with 'militarism, authoritarianism and ethnic nationalism': for them, Kemalism is 'synonymous with progress and, therefore, with freedom' [Mango, *A Speaking Turkey*]. "There are encouraging signs today, however, that greater diversity of expression is becoming acceptable and the place of the military in Turkish public life is receding" (p. 554). Here Finkel's book is very much *not* a 'kings and queens of Turkey' account, for it is hard to imagine a history of England from the reign of Edward II to the reign of George V ending with comments on the current approach to politics of New Labour or expressions of approval of trends perceived by the author. In the constant flux of the Turkish political scene, it is also perhaps a rather dangerous conclusion and reminds one of the well-known article by Dankwart A. Rustow predicting in 1959 that the Turkish military was unlikely to become involved in politics. The army took over in May 1960.

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DER ISLAM IN DER GEGENWART. HERAUSGEGEBEN VON WERNER ENDE UND UDO STEINBACH UNTER REDAKTIONELLER MITARBEIT VON RENATE LAUT; FÜNFTE, AKTUALISIERTE UND ERWEITERTE AUFLAGE. pp. 1064. Munich, C.H. Beck, 2005.  
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This multi-authored survey of Islam in the modern world was first launched by the editors in 1984 and quickly earned acclaim as a standard work in Germany, where it is referred to simply as "Ende/Steinbach". Further editions published in 1989 and 1991 were basically reprints of the first, then in 1996 a reworked and expanded fourth edition appeared, incorporating many changes to existing chapters and adding new expertise by enlisting the contributions of additional authors, including some of the best of young German scholarship emerging at the time. The fifth edition, under review here, represents yet another revision and expansion of the material, and it seems worthwhile to comment upon it in a non-German context where its importance may be less appreciated.

The work as presently published now exceeds 1,000 pages and includes 24 chapters organised under three themes. The first of these is "Historical Expansion, Political and Religious History" (pp. 19–148). Here five chapters trace the main developments in Islamic history from the time of Muhammad to the emergence of modern national states, consider the Sunni and Shi'i branches of the Islamic faith, and discuss the modernisation movements in the Islamic world since the eighteenth century. The role of Islam in such areas as anticolonialism and nationalism is also presented, and Part One ends with a useful chapter on the present diffusion of the Islamic faith in terms of statistics and the current situation throughout the world, including, one should note, the present situation of Islam in western countries (pp. 144–148).

Part Two, which dominates the book (pp. 149–751), deals with "The Political Role of Islam in the Modern World" and contains twelve chapters that interpret the term 'political' in its broadest possible sense. There are chapters on the discussion among Muslims of modern theories and ideas of economic and social organisation, on interest-free financial ideas and institutions, and on trends in the development of Islamic law. There are also accounts of Islam in its American and European diaspora, Muslim discussions of secularism, democracy and human rights, the status of women, and Muslim groups and movements. This reviewer was personally very pleased to see that there are also chapters on mystical brotherhoods and 'popular' Islam (pp. 696–711) and on such sects and minority groups as the

Druze, Yezidis, Ismailis and Bahais (pp. 712–732). There are also chapters on the religious minorities in the Islamic world and on international Islamic organisations, though both of these seem to be rather brief.

The centrepiece of this part is a long chapter (pp. 229–559) – a book in itself – on “The Situation of Islam and Islamic Law in Individual States”. This is divided into 24 regional or national sections, each prepared by an expert or team of experts on that country or region. Coverage includes all of the Islamic countries, of course, but also encompasses Russia, the Islamic republics of the Caucasus and Central Asia, and the various parts of sub-Saharan Africa. Ambitious though the coverage is in this part, it seems to this reviewer that some topics of importance have been missed. There is, for example, a lively Muslim discussion today of Zionism and whether it is Islam’s and the individual Muslim’s responsibility to oppose it. And hardly less vigorous is the debate over terrorism and the extent to which legitimate goals may be pursued through means that many would consider reprehensible. Both of these would have been important additions to this edition of the work. Both arise incidentally here and there, but neither receives concentrated attention.

Part Three (pp. 753–857) consists of seven chapters and treats “Islamic Culture and Civilisation in the Modern World”, beginning – appropriately for a work so solidly grounded in the German tradition of Orientalistik and Islamwissenschaft – with a valuable chapter on “Oriental Studies and Orientalism” (pp. 755–767). This is followed by chapters on Islam and cultural self-assertion, Islam and local traditions, the fascinating question of whether Islamic idioms in the languages of the Muslim peoples allows us to speak of an ‘Islamic’ domain of language, Islam as reflected in the contemporary literature of the Muslim peoples, painting and the graphic arts, and architecture and representational arts. All of these chapters are valuable contributions, but it is curious to see that there is nothing here about traditional and more contemporary music, cinema, theatre, radio and television, the role of the press, or popular traditions such as the *hakawati*. And how, for example, is access to the Internet affecting the thinking and behaviour of Muslims today? And what of science and medicine? There is a lively discussion among contemporary Muslim scientists as to whether there can be such a thing as ‘Islamic science’, and in the field of medicine there are broad-ranging debates and controversies over such issues as organ donation and transplants, artificial insemination, and cosmetic surgery. Many medieval debates are still going strong: for example, may a Muslim take required medication during Ramadan, knowing that this will break his or her fast? Finally, what of sexuality? Issues like pornography, pre-marital sex, adultery, homosexuality, modesty (including standards of dress), and the rights of gays and lesbians are also being vigorously discussed, but do not gain attention in Ende/Steinbach.

One could of course trawl through such a book and point out details that could be disputed or viewed in other ways, but in a work of over 1,000 pages representing the work of nearly 50 contributors – *ce va sans dire!* More useful, perhaps, and especially in the present context, would be the more general question of what this work represents and why and how it is important.

Ende and Steinbach themselves refer to *Der Islam in der Gegenwart* as a “handbook”, and that seems to be an apt description. Certainly it is not a textbook for undergraduate students, although one could imagine such students being assigned to read this or that chapter. I doubt that a student would be able to absorb the work by reading it from beginning to end, and in any case it is just too huge and full of information for this to be an effective way to approach it. Ende/Steinbach is a massive and expertly crafted overview of modern Islam that can best be regarded as a reference work. For every student and professional person whose studies or work touch upon modern Islam in any significant way, this is a work that ought always to be within arm’s reach on the desk. In terms of sophistication, organisation, breadth of vision and content, there is simply nothing else like it – in any language. Its approach is encyclopaedic in vision, but without breaking the content down into the manageable but isolated (and therefore problematic) bits that typify an encyclopaedia.

One can imagine the editors in the early 1980s, before PCs became widely available, working with mountains of paper and trying to maintain some level of coherence for their work as the various contributions arrived. Today, more than twenty years and five editions down the road, how successful have they been? So far as this reviewer can see, astonishingly successful. *Der Islam in der Gegenwart* represents the views of many scholars as to what 'Islam' is and how it is important to Muslim peoples, but does that not in fact reflect exactly what comprises a religion? A religious system involves a corpus of ideas and doctrines, but these are meaningful only in so far as they are interpreted and applied by individuals to whom they are important, and these individuals will view these ideas and doctrines in different ways and put them into practice in various forms against the background of their own culture and social circumstances. On the dust jacket of the book, but for some reason not on the title page, Ende and Steinbach subtitle their work as one devoted to "Development and Diffusion – Culture and Religion – State, Politics and Law". That seems to be an appropriate way to frame their broad-ranging engagement with modern Islam, and certainly it seems to describe accurately the areas where Islam is important in the modern context.

As there have been five editions so far, the appearance of a sixth at some time in the future would seem likely – certainly one would hope for this. The editors will cringe at the thought of making the book even larger, I am sure, but I do think that the areas mentioned above really are of such importance that they should be included in any future revision of the book. I would also suggest a different approach to indexing. At present there are separate indices of persons, subjects and toponyms (pp. 998–1064), and users will be grateful for the way in which important page references are set in bold-face type. But indexing has in recent years become an art in itself, and such an important book as this absolutely deserves a first-class professionally prepared index. This would enormously enhance the usefulness of the book by making it easier for the reader to find or return to exactly what he or she needs. The entry 'Fatwa', for example, is usefully subdivided into ten sub-entries, so that the main entry has a string of only eleven undifferentiated page references. But that is already quite a lot, and the entry 'Mufti' has only one sub-entry ("M. von Jerusalem") and otherwise simply lists 26 places in the book where this topic can be found. There is no cross-reference to 'Fatwa'.

As a researcher into the history of our field, and especially in Europe, it occurs to this reviewer that *Der Islam in der Gegenwart* comprises a kind of 'snapshot' of Islamic Studies in Germany at the turn of the twenty-first century. How does the photo look? Very good indeed. This work vividly illustrates how far Islamic Studies in Germany has come since the days of Reiske in the eighteenth century. It is worth recalling that until the time of Becker and Hartman – no less than 100 years ago – modern Islam was not even regarded as a serious academic subject in Germany. But their successors have now produced a magisterial survey that promises to remain a standard work for many years to come. Thanks also go to the Robert Bosch Stiftung, the support of which made it possible to revise and publish this book at a price that is very reasonable for a work of this size and ambition.

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FABLES OF THE EAST: SELECTED TALES 1662–1785. Edited by ROS BALLASTER, pp. vii, 277. Oxford, Oxford University Press, 2005; FABULOUS ORIENTS: FICTIONS OF THE EAST IN ENGLAND 1662–1785. By ROS BALLASTER, pp. xiii, 408. Oxford, Oxford University Press, 2005.

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In the commentary on *Al-qaṣīdah al-ḥimyarīyyah*, his poem recounting the legendary history of the pre-Islamic kings of Yemen, Nashwān ibn Saʿīd al-Ḥimyarī (d. 1178) tells two stories about Asʿad

al-Kāmil, the ruler who above all others attracted fabulous accretions. In the first of these, the young As'ad meets three witches on a lonely mountain. Having subjected him to a number of trials, they prophesy that he will become a great ruler.<sup>1</sup> The second concerns As'ad's son, Ḥassān. During his campaign against al-Yamāmah, Ḥassān "ordered his troops to carry, each one of them, the bough of a tree, and to hold these boughs in their hands so as to cover themselves with them. And so they did, and they advanced towards al-Yamāmah."<sup>2</sup> The resonances of either one of these tales with the story of Shakespeare's Macbeth could be put down to chance; for both of them to appear in the legends of both characters seems to stretch the bounds of coincidence.

Reynold Nicholson, who also remarked on the parallel between Macbeth and As'ad al-Kāmil,<sup>3</sup> did not speculate on its causes. If there was indeed some migration of legend between the highlands of southern Arabia and those of Scotland, the questions this poses have yet to be answered: at what date could such a migration have taken place? in which direction? and by what route? In this particular case the questions may well be unanswerable; but that does not negate the fact that, as Ros Ballaster puts it in her *Fabulous Orient* (FO p. 7), "narrative moves".

Perhaps the classic traceable example of narrative moving across time and space is that of the *Pañcatantra*. Thomas North's translation of 1570 was, according to its nineteenth-century editor Joseph Jacobs, "the English version of an Italian adaptation of a Spanish translation of a Latin version of a Hebrew translation of an Arabic adaptation of the Pehlevi version of the Indian original" (quoted FO p. 345). But it is the narrative imports into Europe – often much mangled in transit and reshaped, sometimes entirely faked – of a later age that Ballaster collects and discusses. Her period, 1662–1785, "falls between two significant dates in the history of the British acquisition of . . . India" (FE p. 3), those of the transfer of Bombay to the English crown and the retirement of Warren Hastings. One feels that many other equally significant dates might have been picked, and that the real reason for the choice is that this, as a specialist in English literature of the time, is Ballaster's own period. Whatever the chronological criteria may be, the "long" eighteenth century was extraordinarily rich in "fables of the East".

In her anthology of that title, Ballaster brings together extracts from familiar works and writers/translators – Galland's *Arabian Nights*, Harris's *Fables of Bidpai* (Bidpai), Addison, Walpole, Bernier, Manucci, Lady Mary Wortley Montagu, Montesquieu and Goldsmith – with some from names that are perhaps now less familiar – Alexander Dow, James Ridley, Eliza Haywood and Giovanni Paolo Marana. (As is clear from the list, a good part of the English literature in question is in fact French literature in translation.) Her first chapter of the anthology, 'The Framed Sequence', proves that good stories, well told, live on. Galland's *Nights*, for example, have what Ballaster calls "narrative energy", and they have it in inexhaustible amounts. Good themes, too, are timeless: questions about the role of specious deceit employed by rulers, examined so deftly in the fables of Bidpai, were as timely to the late seventeenth century of their translator, Harris, as they were to the eighth-century Arabic-reading audience of Ibn al-Muqaffa' and to an even older India; they are no less important today.

Other pieces in the collection have not aged so well. Ballaster is a champion of the prolific Eliza Haywood; whether her enthusiasm is justified the reader may judge from Haywood's 'History of the Christian Eunuch' that appears in her second chapter, 'The Pseudo-Oriental Tale'. To the present reader, Haywood's story of illicit harem passion and resulting castration seems to exhibit a little too much of what Addison called "oriental Extravagance" (quoted FE p. 124). Varthema, the early sixteenth-century traveller, tells a similar tale (without the castration, however) allegedly from personal

<sup>1</sup> Nashwān ibn Sa'īd al-Ḥimyarī: *Mulūk ḥimyar wa aqyāl al-yaman*, ed. Ismā'īl b. Aḥmad al-Jirāfī and 'Alī b. Ismā'īl al-Mu'ayyad (Beirut and San'ā', 1978), pp. 118–119.

<sup>2</sup> *Ibid.*, p. 142.

<sup>3</sup> *A Literary History of the Arabs* (London, 1930), p. 25.



experience, and he tells it better.<sup>4</sup> Equally extravagant is Ridley's 'The Adventures of Urad', "a kind of oriental Pilgrim's Progress" (*FE* p. 93, n. 9) in which the landscape of a gothicised Iraq includes "the forest of Bagdat", "howling" lions and "a dark Hut, with various kinds of Necromantic instruments". Ballaster's anthology also proves, however, that good writing can transcend even the most extravagant imagination. Horace Walpole's Chinese prince, Mi Li – "my lie", Ballaster suggests as a gloss (*FO* p. 234, n. 49) – bounding around a Gothick park near Henley, is as funny now as he was in 1785.

The third chapter of *Fables of the East*, 'Travels and History', "provide[s] evidence that no encounter with the East is ever 'pure' or free of a network of stories about and from the Orient" (*FE* p. 5). The evidence, however, is not entirely convincing. On the contrary, Bernier's minutely observed account of his journey to Kashmir – the longest quotation in the anthology – provides ample proof of what he calls his "philosophical indifference" (*FE* p. 161). The chapter as a whole sits ill with the rest of the collection.

In the last chapter of her anthology, 'Letter Fictions', Ballaster turns to one of the most influential sub-genres of the oriental tale. The conceit of "reverse ethnography" – employing a character from an alien culture to comment on one's own society – has a long history.<sup>5</sup> Marana's late seventeenth-century *Letters Writ by a Turkish Spy* gave it new life. His spy, Mahmut, living in disguise in Paris for over four decades, does more than simply report back to his masters in Constantinople: a man suffering the strain of long-term dissimulation, he is also a character of considerable psychological complexity – "I am a perfect Riddle to myself", he admits (quoted *FO* p. 157). Given that Marana not only had numerous imitators in the eighteenth century but might also be said to have echoes in latter-day spy novelists like Le Carré, it is surprising that the most recent printing of his complete work was as long ago as 1801.

In *Fables of the East*, Ballaster offers brief but useful analyses of the texts quoted, and in particular of the wide range of ends they sought to achieve, didactic and satirical as well as that of pure entertainment; she also identifies some of the religious and political subtexts they contain. In *Fabulous Orients*, she expands both her analysis and the number of her sources. Her aim is to examine "what western readers . . . read into oriental tales" (*FO* p. 5) – for, she says with characteristic syntactical playfulness, fiction not only "moves" in the intransitive sense; it also moves its readers and, in the nominal sense of the word, "makes distinct narrative moves – political, social, emotive – which serve to prompt desired responses" (*FO* p. 8). In contrast to her anthology, after providing an overview of the various genres of the oriental tale she adopts a geographical rather than a generic arrangement for her material – Turkey and Persia, China, and India. Each of these three chapters also identifies underlying themes in the fictions of the area with which it deals.

The first of them focuses on the use of 'loquacious' female narrator and silent male auditor: "Her agency is that of the tongue and of time, his is that of the eye and of the masterful control of space" (*FO* p. 22). This is a theme Ballaster pursues at considerable length. As the book progresses, however, the texts themselves reassert their authority over these loquacious abstractions, and her analysis becomes more succinct and informative. Her comments on the cult of *chinoiserie* and the relations between narrative and mercantile imports from the Far East are illuminating. So too is her discussion of the changing relationship between European writers and India: authors, she says, displayed "a tendency to identify European values with those of the Mughal and other Muslim rulers of Indian states" (*FO* p. 270); in an epilogue looking beyond her period, she explains how such "Enlightenment systems of analogy" were transformed into "Romantic and neo-Gothic explorations of alterity" (*FO* p. 365).

Perhaps inevitably, when an English literature specialist like Ballaster ventures into 'orientalist' territory she will be on risky ground. Captious though it may be to point out the resulting errors, a

<sup>4</sup> Percy G. Adams, in *Travel Literature and the Evolution of the Novel* (Lexington, 1983), pp. 159–160, cites Varthema as a possible source for Haywood.

<sup>5</sup> It goes back at least to the thirteenth century and Caesarius of Heisterbach's trenchant observations on the state of Christendom, expressed by a fictional Muslim, in *Dialogus Miraculorum*.

few of them do stand out: the Abbasids, for instance, would have been bemused to discover that they were a “dynasty of Shi’ite Islam” (*FE* p. 73, n. 2); similarly, the inhabitants of Agra would be surprised to read that their city “was founded in the early 18th c. by Sikander Lodi” (*FE* p. 180, n. 1). (The latter may be an example of the typographical errors of which neither volume is as innocent as it might be.) More seriously, Ballaster is also strangely inconsistent in her expectations of her readers. She provides them with footnotes explaining the words ‘luscious’ and ‘Amazon’ (*FE* pp. 47 & 186); but when in full and enthusiastic flow she can take them into rather more impenetrable territory – writing, for example, of “the encounter with the Orient [that] . . . provides access for the western female to the masculine position of generating models of subjectivity through the objectification of ‘others’” (*FO* p. 90).

These complaints apart, Ballaster’s two volumes are a valuable survey of a surprisingly large and multifarious genre, and of the “lively, complex and troublesome” narrative traffic between East and West. As those tales of Arabian witches and moving woods suggest, it is a traffic much older than Ballaster’s period; and it is one that continues as lively, complex and troublesome as ever.

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RUMI – PAST AND PRESENT, EAST AND WEST – THE LIFE, TEACHINGS AND POETRY OF JALAL AL-DIN RUMI.  
 By FRANKLIN D. LEWIS. pp. xvii, 686. Oxford, Oneworld Publications, 2000.  
 doi:10.1017/S1356186306266471

This is a book to be welcomed both by students and by the general public – a scholarly and sympathetic account of a great poet of the Islamic mystical tradition of Sufism. Extensive historical research underpins this book; the tone and style are approachable; the author takes us through his arguments with exhaustive patience, scrupulous honesty, and also humour. Indeed, the attractive personality of the author comes through clearly, as does his love for his subject – and the reader implicitly trusts this rare guide to new worlds of the spirit and to the history of an unfamiliar literary tradition.

Franklin D. Lewis gives an account of his first encounter as a teenager with Rumi’s work quoted in the mid-nineteenth century writings of Bahauallah, the founder of Baha’ism, notably in his “Haft Vadi”. His sympathy with Baha’ism’s roots in Persian Islamic mystical traditions is evident throughout this book. This sympathy is counterbalanced by an undertone of disappointment and irritation, and no doubt justified grievance, with traditional Ithnaashari Shi’a exotericism and indeed with all religious legalism. He also condemns the Turkish nationalism of some recent writers on Rumi, and shows an amused disdain of contemporary fashionable Californian ‘New-Age’ distortion of Rumi – though he wryly comments that without that fashion there might be only a limited market for his scholarly work. His 1995 doctoral thesis on the twelfth-century Persian mystical poet Sana’i is a basis of sound historical research, which also benefits this book, one of the best Rumi studies in its range of reading, research and critical comparison of sources. The poet is here placed back in his context as a medieval Sunni Muslim of the Hanafi legal school, of Khorasani socio-linguistic and literary background, living and working in Mongol-dominated Seljuq Anatolia, between the remnants of Hellenic Byzantium and the Ayyubid, later Mamluke, territories of Syria and Egypt.

Lewis topples many myths from their shaky pedestals. Rumi’s birthplace in September 1207 was not Balkh in northern Afghanistan but Wakhsh in the mountains of Tajikistan where his father was working as a preacher – though Rumi is actually referred to as “Balkhi” in early sources. As a boy he

did not meet the mystic poet 'Attar in Naishapur. He was not principally influenced by Ibn Arabi's style of mystical theosophy but was rather the inheritor of the Khorasani mystical poetic tradition of Sana'i and 'Attar. His thirty-seven-year-old relationship with the sexagenarian Shams was not a foretaste of Californian gay romance, but must be seen in the historical context of the Islamic medieval structures of learning. However, on page 207, Lewis quotes the beginning of a passage from the *Fihī ma Fihī*, to argue a relationship of ceremonious formality and humility between Rumi and Salaheddin the goldsmith, successor to Shams in Rumi's affections. The continuation of the passage shows that this "humility" involved mutual massage in the steam-bath, which Lewis perhaps left unquoted because in today's secular sex-obsessed West the connotations are so different from those in medieval pious pre-Freudian Anatolia. Here the younger man stops his elder by taking his turn at massage, whereupon the elder thinks "*tavazu' be tadrij beh: avval dast-ash bemali, ba'd az an, pay andak andak; be ja'i beresani ke an zاهر na-shavad o na-nemayad, va u khu karde bovad*", i.e. "it would have been better if I'd taken it more gradually, massaging first his hands and then his legs, reaching a point where he would not have noticed and would have got used to it!" – which begs the question of what exactly was going on! Especially since one of the most tender and passionate of the love poems in the Great Divan is addressed to Salah al-Din, possibly on his death-bed at the end of a painful illness: "*pushide chun jan, miravi andar miyan-e jan-e man*" "Hidden like the soul, you penetrate my inmost soul". It is indeed difficult to reconstruct the emotional and social conventions of distant and foreign cultures long ago – but one can see how those Californian gays could have been tempted to enrol Rumi among their predecessors! Though one should not forget that the parameters of expressible emotion between males are different in Muslim societies, though genital sexual encounters are considered shameful. Salah al-Din's daughter married Rumi's son, and Rumi conscientiously took care of Salah al-Din's other children after their father's death. The sobbing threnody which Rumi extemporised on that occasion – "*ay darigha! ay darigha! ay darigha!*" – is beautifully translated by Lewis on page 215. By returning these poems to their 'Sitz im Leben' (lived context), he restores also their immediacy and raw emotional power.

The main influences on Rumi were his own father Bahauddin Walad, his preceptor Burhanuddin Muhaqqiq and Shams of Tabriz who is given extended treatment on the basis of his rediscovered works, with due credit given to their editor, the Iranian scholar Movahhed. Fritz Meier is also owed an important debt for his pioneering work in this field. This is one of the most valuable parts of the book, the analysis of the works – the *Ma'aref* and the *Maqalat* – of these three great influences on Rumi. There are very useful details of Rumi's life, and a carefully re-evaluated chronology, given in Chapter 7 "Towards a Biography of Rumi", as also a register of historical characters with whom he had dealings. On page 108, the elderly examiner Burhanuddin is related to be kissing his young examinee Rumi on the soles of his feet, a position difficult to envisage – one suspects hyperbole in the original, or mistranslation.

The hagiographies of Sepahsalar, Aflaki and Sultan Walad are critically evaluated and compared to the information discernible in Rumi's own writings, the *Mathnavi* (as edited by Nicholson, 1925–33), the *Divan* (as edited by Foruzanfar, 1957–67), the 'malfuzat' records of conversations *Fihī ma Fihī* (recommended in the edition by Modarres-e Sadeqi, 1994) and also the letters (as edited by Sobhani, 1992) – which show the social machinations of seeking patronage and favours from the powerful political and military elites of the Seljuqs and other power-holders recognised or established by the Mongols after their conquest of Anatolia in 1243. As Golpinarli points out, the text of the *Mathnavi* also gives hints of the process of composition, as when Rumi apologises to his amanuensis Husam al-Din Chelebi for keeping him up all night taking notes while Rumi was reciting. (p. 219) or when his inspired flow is interrupted for a snack and digestion obstructs resumption of the story. Certainly the liberal sprinkling of quotations from the Discourses, *Mathnavi*, *Divan* etc often give penetrating

insight into the course of Rumi's life and thoughts and emotions, but their 'Sitz im Leben', the actual precise biographical context is sometimes not certain – and so these are all too often more educated guesses than hard facts of biography. The myth-making of the hagiographers is unmasked in the process of the later geographical expansion and institutionalisation of the Mevlevi order, with the formalisation of the Sama' ritual, the later Ottoman patronage, the spread to India and Central Asia and the modern West.

The most amusing part of this long (perhaps overlong at almost 700 pages) book is the later section devoted to the entry of Rumi into western literary culture, where the inadequacies of free second or third hand adaptations masquerading as direct translations are lightly reviewed and dismissed, relativising the recent popular successes of free adaptations of Rumi's poetry by poets with little or no knowledge of the medieval Persian cultural and linguistic background. This range of research is presumably made possible by the Internet, which allows ephemera to be dredged up for academic contemplation and presented as an extended shopping-list.

The exploration of recordings of Rumi's poetry is a valuable part of the book – listening to the recording of the great modern Iranian poet Ahmad Shamlu reading Rumi was one of the experiences which influenced the young author – an experience I shared with my students in Isfahan in 1978, which still resonates in the memory.

Lewis, in Chapter 8, presents 50 of Rumi's poems in translation: these are both contemporary and mostly faithful to the spirit of Islamic Mysticism of the originals. He conscientiously gives the transliteration of the first line of the original and reference number in the Divan etc, and also the metre, emphasising the importance of rhythmic declamation in the Sama' ceremonies; useful brief notes follow the translations.

However the treatment is often free (I like the translation of "*dar feraq-e jamal-e u, ma-ra jesm vayran o jan dar u chun bum*" on page 178 as "In separation from his beauty, my flesh is in ruins, my soul hoots like an owl" where the translator's addition of "hoots" makes the image even more vivid), but sometimes a medieval sense or use of a word is ignored in favour of the modern Tehrani version (eg. in poem 18 "*to 'ayn-e ma'i*" as "you're just like me" rather than "you are my ideal identity" – though of course the modernised Rumi reads better!) or the attempt to accommodate to the modern experience leads to cultural anachronism or insensitivity as when Rumi's arrangements for his son's wedding include a "western-style wine-waiter" (p. 212).

Some friends have been moved to tears by the translations of Rumi here presented, other acquaintances have dismissed them summarily – I feel more in tune with the former than the latter opinion, even though the occasional pseudo-French medievalism of expression does jar – Armance and Byzance, delire and delusion, fabliaux, etudes of poetry – as also the occasional malapropism, due no doubt to the absence of Latin from school-curricula – "tribulation" is used where temptation seems to be meant, "sojourner" for traveller, "security" for bribe, and later "amorette" for lovers, etc, etc.

But these are mere quibbles.

There are occasional lapses in translation that should be pointed out, as these versions of the poetry are after all presented as models to be followed. Let a few examples suffice:

In the first poem chosen for Chapter 8, "*kafi afun*" is translated as "a froth of opium", displaying, according to my colleague Mohammad Reza Jozi, the translator's lack of acquaintance with this noxious substance as well as with Persian idiom: here the "*kaf*" is the palm of the hand, so it would be better to translate it as "a fist-full of opium".

In the second poem of Chapter 8, in last verse, "*alam*" a battle-standard is required by the metre and the sense, not "*elm*" knowledge – *mustaf'ilun mustaf'ilun mustaf'ilun mustaf'ilun* "*khamush! ke bas mosta'jel-am, raftam su-ye pa-ye 'alam.*" The metre could also have helped avoid the mistranslation "even in the midst of war", based on wrong insertion of ezafe, of "*v'andar miyan, jang afkani*" which means

You (God) raise up conflict (in their midst), . . . compounded by a further mistranslation “that no one’s ever seen before” for “*fi estena’-e la yura*” which is a quote “*la yura*” from the Quran Sura XLVI, verse 25, referring to the destruction of the tribe of ‘Ad, so that nothing except their empty dwellings was to be seen the following morning.

In poem 5, which hinges on the false illusory identities displayed by sorcery “*jadu*” or “*sehr*”, with “*nemudan*” as a verb meaning to show (here falsely, by slick sophistry), there are also several instances where the meaning could be expressed more precisely: especially the verse “*por bad-e hedayat ast rish-ash, az sehr-e to, jahel-e ghawi-ra*”, where the windy self-important misguided bearded ignoramus (i.e. exoteric cleric!) seeks to guide where he has no knowledge, and the verse “*sofisata’iy-am kard sehr-at: ey Turk nemudeh Hendu’i-ra*” where the poet is turned into a sophist, passing off the blackness of a Hindu as a white moon-like Turk. Sophistry, like juggling and sorcery – “the equivocation of the fiend, that lies like truth” – makes things seem other than they are – which was also a feature of training in rhetoric. The translation also I think gets “*shiri*” and “*ahu’i*” mixed up, the timid gazelle is passed off as being lion-like, not vice-versa; and it also misses the negative connotation of “*ahwal*” – here the eye unable to see the unity that underlies the apparent multiplicity of existence – and of “*alu*” less prestigious than the “*turunj*” golden citron which, according to Khaqani’s famous qasida describing the ruins of Ctesiphon, graced the royal table of the Sasanians.

In the lines quoted from the Mathnavi on page 182, ““*eshq-ra pansad par-ast, o har pari az faraz-e ‘arsh ta taht ath-thara*”, the translation runs “Love has a thousand (not 500) feathers, and each one soars over the Throne *beyond the Pleiades*”: which fails to recognise a quote from the Qur’an, Sura XX, verse 6, where *taht ath-thara* is usually translated as *under the soil*, which has nothing to do with the Pleiades constellation, though the Arabic name *thurayya* could possibly lead to confusion; previously however, on page 24, *thara* had been correctly translated as soil. Just a slip, or unfamiliarity with the basic text contemplated by all Islamic mystics?

The book is full of interesting facts, based on wide reading – and yet there is still so much to do, as Lewis himself underlines in his suggestions for further research, which also serve to define the limits of the book under review. Thus the following examples of desiderata:

page 84, “a new, fully collated edition of Baha al-Din’s *Ma‘aref*”;

page 128, “a meticulous study of Rumi’s letters in conjunction with historical sources and biographical dictionaries . . .”;

page 164, “to periodize specific statements on the basis of internal evidence” in Shams Tabrizi’s *Maqalat*;

page 214, “a careful study of these sources, along with the letters Rumi wrote for him (Salah al-Din) and the supplementary details we can cull from Aflaki, may one day provide us with a fuller portrayal of the man and Rumi’s view of him”;

page 221, “if further study bears out that this or other poems addressed to Husameddin date to the lifetime of Salaheddin”;

page 272, “these hagiographies should also be subjected to a bit more critical acumen, plotting and analysing them in terms of genre conventions and historical facts. A detailed political history of the period is needed that would correlate historical sources and political events with the known facts of Rumi’s biography. The sources Rumi drew upon for his ideas and the terms in which he expressed them require more thorough examination”;

page 297, “a catalogue of all known manuscripts of the Mathnavi and the Divan-e Shams, giving such details as date, scribe, provenance, present location, etc”;

page 302, “a manuscript from the lifetime of Rumi, or even one in his own hand, may turn up somewhere among the 200,000 or so uncatalogued manuscripts of Persian literature still extant in collections in Iran and Turkey”;

page 303, “a definitive edition of the poems awaits the skills of an enterprising and philologically grounded Persianist, but the task is a daunting one”;

page 315, “an effort should be made to periodize Rumi’s ghazals on internal evidence”

page 400, “we still lack a systematic comparative study of these questions – aspiring Rumi scholars take note” i.e. to trace influences and “situate Rumi’s ideas in the context of Islamic scholastic and legal theology”.

The nature of the conquest state of Seljuq Anatolia – a Turkish military and tribal element superimposed on a Greek and Armenian peasantry and urban merchant and craftsman class, with numerous immigrants from Iran and central Asia – remains difficult to grasp. Since the floodgates were opened to the Turks after the battle of Manzikert in 1071, there had been, by the time Rumi was active in Konya in the middle years of the thirteenth century, a considerable degree of acculturation and even conversion to Islam among the Byzantine inhabitants of Anatolia. We find Sufis acting on different levels, as they have done repeatedly in history, as shock-absorbers for disrupted and dislocated societies, as agents of conversion, as elements legitimising a regime, as soul-doctors, as purveyors of emotional catharsis, of pietism, of a subtle aesthetic and a spiritual philosophy.

Rumi’s patronage and promotion of music, especially after his undergoing the influence of Shams Tabrizi led to the formalisation of the Sama’ ritual, which was obviously a powerfully emotive shared experience that could transcend linguistic boundaries, and offer a form of self-transcendence to artisans and new converts as well as to members of the court. But to whom was Rumi addressing his poetry and sermons and letters? Who actually understood the Persian he spoke and wrote? One is left with the impression that the Great Divan and the Mathnavi were addressed to an inner circle of elite students with leisure to learn and an already considerable knowledge of Persian, Arabic and the Islamic tradition.

But what were the relations among different groups of Sufis – say the Mevlevi and the followers of Sadruddin Qunawi or Haji Bektash and the Akhis encountered by ibn Battuta in his early fourteenth century travels – were they friendly or distant or were they even hostile rivals? What was the social morality of followers capable of threatening to murder or actually murdering their master Rumi’s inspiring mentor Shamsuddin of Tabriz or his beloved companion Salahuddin – how far had they evolved on the spiritual path? Was it the repeated suspicion of “*shahed-bazi*”?

The recent “TURKS” exhibition at the Royal Academy in London showed examples of material culture from the Seljuq/Mongol period in Anatolia, partly from the shrine at Qunya/Konya, many of which are not easily tied in to the historical account given here – as so often in the history of Islamic or medieval culture generally, the gap between the surviving written record and actual buildings and objects is often unbridged or no longer bridgeable. So more needs to be done on patrons, devotees and converts and the material culture, for instance the openwork bronze lamp signed by Hasan al-Maulawi/el-Mevlevi, which is said to be contemporary with the poet himself, and therefore brings up the question of how early the *nisba* “al-Maulawi” was actually used – Golpinarli has an interesting note about the scribe of the 1278 manuscript of the Mathnavi, Muhammad ibn Abdullah al-Qunawi al-Waladi, explaining al-Waladi as “a person devoted to Sultan Walad” – therefore the same presumably goes for al-Maulawi, a devotee of Maulawi Jalaluddin Rumi, who might use or be known by that name within the lifetime of the master. Some of the satirical Siyah Qalam paintings (115, 117, 118, 119, 122, 124 in the catalogue) recall Shams Tabrizi’s mention of impoverished Russian peddlars in Konya who wore pelts and conical hats and sold matches in the streets (quoted on p. 162).

For visitors to Konya and the areas of Anatolia once dominated by the Seljuqs, the surviving buildings of the thirteenth century evoke the ambience in which Rumi lived – and we could have had a little more on these and their patrons – notably Sahib Ata Fakhr al-Din who died in 1288, and was responsible for the 1258 Sahib Ata Jami’ mosque as well as the Ince Minareli Madrasa in Konya and the 1275 Tash Madrasa in Aqshahr / Aksehir and the 1271 Gok Madrasa at Sivas, and also Jalaluddin

Qaratai and his magnificent madrasa of 1251 where Rumi passed with his disciples both during the lifetime, and after the death, of the founder.

There is thus obviously an enormous amount of work still to be done, in the field of Rumi studies in particular and Persian literary and historical studies more generally.

But with this book to instruct and inspire new generations of students, let us hope that willing workers will be found – the field is rich and rewarding. I have certainly enjoyed the chance, given to me in writing this review, of plunging once again into the ocean of Rumi's thought and poetry.

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RELIGION AS HISTORY, RELIGION AS MYTH. By DAVID SNELGROVE. pp. xii, 156. Bangkok, Orchid Press, 2006.

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Some subjects matter, and some books. Religion, a label and a condition, needs to be studied comparatively and constructively. Meanwhile “myth” has two senses: “myth (A)” means “an imaginative representation of a historical tradition”, while “myth (B)” means a fiction, Snellgrove's “pure invention” (pp. 25, 39, 45). The Buddha's ladder is myth (B) (cf. Genesis 28:12). Devadatta (p. 77) is myth (A) on the way to being myth (B). Ananda's reprimand (p. 78) is surely myth (B) like his miracle island (p. 88). But the Gerasene Swine (2,000 of them: p. 4, n. 12) is myth (A) not myth (B): its substrata are (i) “spirit possession” and its cure and (ii) news of Jesus's exclusion of non-Jews from his mission.

Here we have a familiar exposal of Christianity both at a primitive stage (when the kingdom was expected hourly) and in its equivocal aftermath. St Luke is admired (p. 13) without his title of “accomplished midrashist” – for midrash used texts very freely (pp. 18, 33). New is the depiction of St Mark as hostile to Jesus's *śrāvakas* (pp. 45–46). Next comes an exposition for western readers (p. 80) of mainstream and Mahāyāna Buddhism such as one would expect from Britain's doyen of Buddhology, a traveller in South and South East Asia,<sup>1</sup> who does not conceal anomalies (p. 103). Next comes a sympathetic, but candid, account of Islam, noting its reaction to the corrupt West (pp. 125–126).

Snellgrove attends to the concept of prophet, which Jesus (an obsessive: p. 21) and Muhammad (p. 9) assuredly were. Gautama the Śākyamuni Buddha may be neither a “prophet” nor a broker for a deity. Snellgrove avoids a spiteful (if illuminating) view of Judaeo-Christianity,<sup>2</sup> but “original sin” is highly obnoxious to him (pp. 3, 37, 50, 56–57, 123). Our author's lifetime's experience of religion tells him that Buddhism is not incompatible with Christianity nor with Islam, if the criterion is *compassion* (pp. 43 [Acts 10:38], 55, 94–96). He notes the intolerance of both Catholic (p. xi) and reformed theologians and their partisans (p. 44), and Islam's destruction of anything savouring of idolatry in Greater India. It recently proselytised amongst Buddhist communities (pp. 48, 125); however a liberal Islam succeeds to other faiths in South East Asia (p. 124).

Summing up he deals with early concepts of God, a god of loving kindness; God as an active principle; personal devotion; and the world as mere appearance. “Allah” may be a handier word than “God” (p. 133). God is not separate from his creation (pp. 131–132). Leaving the painful existence of the hominids (pp. 56, 58), Hindus who anciently had monotheistic perceptions (pp. 129, 132), provide a better image, Hari-hara, combining creation and destruction (p. 134), for viciousness and mercy are

<sup>1</sup> D. Snellgrove, *Asian Commitment* (Bangkok, 2000).

<sup>2</sup> Gunapala Dharmasiri, *A Buddhist Critique of the Christian Concept of God* (Colombo, 1974).

both natural (p. 136). Awareness of a “creative/active principle”, a “subtle inner resolve” (pp. 133, 136, 143) is real, irrespectively of its sometimes perverse uses (e.g. opportunistic missionaries in Cambodia: pp. 105–107).

This book is tastefully presented, well written (if lengthy on the gospels’ contradictions), and one forgives the occasional unusual spelling; BC for AD on p. 111; “Pharisees” for “Sadducees” at p. 23; and the reversal of captions for illustrations 11 and 12. The glossary, bibliography of works used, and index are splendid. But what of the author’s own posture? The aged athlete teeters on the brink of a delectable pool awaiting his plunge (p. 51). Who will give him a shove? It is the reviewer’s privilege.

Nature is concerned with reproduction and evolution – hence every specimen is dear. A forest-dweller will ponder on this. The myth of Adam and Eve (myth (B)) is witty and sound notwithstanding literal-minded sarcasm (pp. 3, 57). Hominids were when *Homo sapien* was not; then *Homo sapien* was; ergo the Creation of Man. Until the discovery of the unconscious heart and mind were wrongly identified. To reproduce *H. s.* genders were required. The mature male is competitive, accumulative, aggressive; the female, designed for pregnancies and nurturing, dominates and manipulates while appearing as subservient. The lineage arbitrates between competing males and females. Nature can be pruned (John 15:1) but not ignored. *Homo sapien* is the only species to monitor its own slavery to her. A prophet who divulges his disconcerting (p. 74) discovery (cf. Genesis 1:27, here at p. 58), elated and self-defining (cf. Matthew 16:17, here at p. 41, cf. pp. 57, 138), becomes an enemy (p. 113) of Nature’s slaves. Socrates (*Phaedo* 66 C-D, here at p. 98) may have been such a prophet. If the unconscious and the calculating organ work together one has a single-minded command of self, fit to show compassion and indulgence.

The hostility to Christ and his loyal disciples (however few) was understood from the first (Mark 3:31–35, 8:34–35; 10:28–30; cf. p. 15). No bodily resurrection is promised,<sup>3</sup> but that oxymoron, the “pneumatic body” (1 Corinthians 15:44–53). Those that “inherit” it are its heirs and may demonstrate it (Luke 12:32, here p. 49), not as an *iddhi* but (cf. pp. 5, 27) as a particular freedom. Such a solution has no quarrel with science (cf. here pp. 119, 123, 145, 147).

Snellgrove, noticing Chr. Lindtner, does not mention his theory that a gospel derived from a Buddhist text; nor bother with John Mackinnon Robertson, who linked Christ with Krishna.<sup>4</sup> His solution is deeper, and this reviewer tries to shove him into it.

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THE WAHHABI MISSION AND SAUDI ARABIA. By DAVID COMMINS. pp. xii, 246. London, I.B. Tauris, 2006.

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This book, as its ‘blurb’ asserts, is the fullest and most authoritative account to date of Wahhabism, the reformed version of Islam which became entrenched in Central Arabia in the eighteenth century, and which became the state religion of Saudi Arabia in the twentieth. It begins by demonstrating how the ideas of Muhammad ibn Abd al-Wahhab emerged in the eighteenth century, how his family became

<sup>3</sup> Richard C. Carrier, “The spiritual body of Christ”, ch. 5 in Robert M. Price and Jeffery J. Lowder (eds.), *The Empty Tomb* (Amherst, N.Y., 2005).

<sup>4</sup> John M. Robertson, *Christianity and Mythology* (London, 1936). See D.N.B. for this Robertson.



the principal sustainers of the Wahhabi tradition, how their link with the ambitions of the Najdi chieftain, Muhammad ibn Saud, was key to Wahhabism becoming the dominant religious doctrine in the region, and how this religious change represented a revolution in eighteenth-century Arabia. It goes on to show how, despite Egyptian and Ottoman attempts to purge Wahhabism from Central Arabia, the remoteness of the region enabled it to become established as the regional religious culture of the Najd. In the early twentieth century, the rise of Abd al-Aziz ibn Saud to impose his dynasty over most of Arabia brought about a massive expansion of the space in which the Wahhabis might operate with ease, but it was one which was strictly limited by the requirements of international relations, as Abd al-Aziz's destruction of the wilful Wahhabi Ikhwan at the battle of Sibila in 1929 demonstrated. The book goes on to show how the growth of the Saudi modern state, fuelled by petroleum revenues, gave the Wahhabi ulama, with their control over law, and their influence over both education and the moral authority of the regime, the opportunity to spread substantially their influence through Saudi society. Then, the book widens its focus to embrace the world beyond Arabia and to demonstrate how the Wahhabis and Islamic revivalists in the world beyond, members of the Muslim Brotherhood and supporters of the Ahl-i Hadith and the Jamaat-i Islami, found common cause in their rejection of the West and its ways which were so deleterious of Muslim piety and values. Arguably, one outcome of this conjunction of interests was King Faisal's foundation of the World Muslim League in 1962 to combat secularism and socialism. In the 1970s and 1980s this alliance continued to develop, though the seizure in 1979 of Mecca's Grand Mosque by the Muslim Brotherhood-influenced al-Utaibi could be regarded as a severe 'blip', and reached its apogee in the jihad against the Soviet occupation of Afghanistan, which did so much to bring the jihadi tendency from the fringe to become a major force in the Muslim world. Then, in the Kuwaiti crisis of 1990–91, we are told, the Wahhabis split from the Islamic revivalists over King Fahd's invitation to the USA to defend Saudi Arabia and its holy places against Iraq. The credibility of the Wahhabi establishment suffered as it aligned itself with the Saudi monarchy against a growing tide of Islamic revivalist protest inside and outside the Kingdom. The huge gulf between its statements and those of Osama bin Laden and al-Qaida, not least over the bombing of the World Trade Centre in September 2001, only served to underline the point. The book draws to a close observing that the historical alliance between the al-Saud and the Wahhabis is now bringing declining returns to the former, who have had to open the "public sphere to a plurality of voices articulating a range of religious and liberal views", including those of the Shias. The future of the two hundred-year old regional religious hegemony of the Wahhabis, as Commins puts it, is "in jeopardy".

This work is based on substantial research in Arabic sources, supported in part by a residency at the King Faisal Centre for Research and Islamic Studies at the King Faisal Foundation, Riyadh. There are one or two weaknesses. No comment is made, for instance, about the distinction which Natana Delong-Bas (*Wahhabi Islam: From Revival to Reform*, [London, 2004]) has made between Muhammad ibn Abd al-Wahhab's thought with its embracing of *ijtihad*, and its apparent real potential to respond to social, economic and political change, and the straitjacket within which those who have managed the Wahhabi heritage have placed it. This is a pity given the growing marginalisation of the Wahhabi tradition by such changes over the past few decades. Then, apart from the Afghan jihad, Commins is rather too cautious in forming judgements about Wahhabi influence beyond the shores of Arabia. He is right to be careful about trying to make too much of the connections of ulama and sufis in linking the jihad movements across the Muslim world in the nineteenth century. Current scholarship often prefers a local origin for the reforming drive. Nevertheless, I would reckon that Wahhabi influence was greater in nineteenth-century India than he would suggest. This said, Commins has given us in a most judicious fashion the best account we have yet had of the rise and decline of Wahhabi power, its interactions both with the needs of the Saudi dynasty and with the impact of outside forces. Most importantly he demonstrates that the Wahhabi tradition is a very different animal from the Islamists

and Islamic revivalists, who have striven to capture the political realm both in their states and across the world.

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MONUMENTS, OBJECTS, HISTORIES: INSTITUTIONS OF ART IN COLONIAL AND POSTCOLONIAL INDIA. By TAPATI GUHA-THAKURTA. pp. 432. New York, Columbia University Press, 2004.  
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## I

To “seize hold of a memory as it flashes up at a moment of danger” was how the philosopher Walter Benjamin described his concept of history in the unsettling months of early 1940.<sup>1</sup> A sense of writing history from an urgency created by a crisis of the present, or what Benjamin called the *jetztzeit*, resonates throughout this extremely well-written and well-argued collection of essays by historian Tapati Guha-Thakurta. The heinous attack on the Babri Masjid by Hindu nationalists in December 1992, and the vandalism that ensued following the representation of Hindu goddesses in the nude by a prominent Muslim artist in the mid-1990s, threw into question the innocence of archaeology as a scientific practice and pointed to the need for urgently decoding the official canon of Indian art and its constructions as much in the past as in the present. In fulfilling this aim, the book has largely been successful. Published by the Columbia University Press as part of a new series called “Cultures of History” that takes history “as the primary text of modern life and foundational basis for state, society and nation”, the *Monuments* is about pasts that come to be imagined around a cumulative corpus of monuments and art objects in modern India, major signifiers of the idea of nation. In fact, the author claims that “no other subject has borne so forcefully the imaginings of the nation as the history of Indian art” (p. xxii). The “primacy of the national”, “the different forms of its insertion and invocation in Indian art and archaeology” and the pervasive authority of the nation in institutions such as museums forms the chief concern of this collection of nine essays, all but two of which have been published before.

The book’s main theme, the shaping of Indian archaeology and art history as modern disciplines, articulated from within institutions such as museums, which determine their relationships between past and present, subject and object and individual memory and collective history, is situated within the ever-growing discussion on ‘the nation’. Consequent to the publication of Benedict Anderson’s *Imagined Communities* (1983) and an upsurge in nationalist movements in various parts of the world, ‘the nation’ has easily been the most debated idea among scholars of post-colonial history and it has gained particular currency within the field of cultural studies. The debates include: the ‘when’ and ‘why’ of nationalisms, the meaning of cultural difference, whether or not there are different kinds of nationalisms, whether ‘the nation’ is imagined, fabricated or invented and nationalism’s link to modernity. Whatever the terms of debate, there is no doubt that a belief in the primacy of ‘the nation’ deeply influences our understanding of society today.

With the violent attacks on monuments by nationalists, best exemplified by the demolition of the Babri Masjid in India, these objects could no longer be seen as silent relics or inert traces of the past. Gone are the days when we could be innocently hooked on to the ‘aura’ that monuments exude. Archaeologists and historians have begun to explore seriously the relationship between their own

<sup>1</sup> Walter Benjamin, “Theses on the Philosophy of History”, in *Illuminations*, ed. Hannah Arendt (New York, 1969), p. 255

practices and nationalism and also on the presuppositions of this modern 'scientific' discipline. Such interest was triggered however, not only by violent events but also from developments within the discipline of archaeology and discussions on nationalism.

Moving beyond Anderson's theory and the primacy given to the role of print in the spread of nationalism, some historians stress the critical role played by the 'visual' in imagining 'the nation'. Important contributors to this dialogue include E. N. Chernyk, J. A. Atkinson, M. Diaz-Andrew, P. L. Kohl, S. Castro-Klarén and Lamberg-Karlovsky to name but a few. These names however are surprisingly absent from the book's bibliography. Can this be justified by the author's claim of being an 'outsider' to the professions of archaeology and art history? Not really. Neither can it be justified by the author's statement that her interest in the matter stems from a "specific preoccupation with the modernity of our pasts and the modern lives of the monuments and objects in which the past inheres". In fact, the author seems keen on maintaining a distinction between 'insider' and 'outsider' histories but does not explain sufficiently what this means or why this distinction is important. The claim is that the book is "markedly different from the kinds of insider histories of changing methods and approaches that the disciplines have tended to generate" (p. xxi). The reader is provided with but one example of such histories in a footnote (and that from art history alone) but we are left to assume that 'insider histories' means histories that do not question the presuppositions of the discipline. As an alternative to 'insider' and 'outsider' histories, the author attempts to "open out a critical position that lies both within and outside the disciplinary bounds: a position from where we can grasp the broad discursive contours of the field, even as we mark its inner debates, dissensions, and transformations" (p. xxii). A challenging position indeed, not the least because there can be no escape from the fact that the historian is himself/herself an 'insider' to another discipline, that of history, with a history of its own.

## II

The book traverses colonial, regional, nationalist and post-nationalist moments in Indian history stretching from the mid-1850s to the 1990s. I think it is an interesting exercise, albeit unconventional, to read the book in reverse order beginning with the last section, the "Embattled Present", wherein, according to the author, lies the book's heart. The attempt is after all to retrace steps carefully as far back as possible into the past for an insight into the "violation of 'truths'" experienced in the recent times. The essays employ an intelligent structural device that couples or juxtaposes texts, people, objects, institutions and sites from across time, as the context demands, to unfold comparative biographies. This concluding section situated in the "Present", has as its backdrop Hindutva cultural politics, which deeply embroil the disciplines of Indian archaeology and art history in its wake. Two representations of the female nude in Indian art are juxtaposed for comparative analysis, M. F. Hussain's nude goddesses painted in the 1990s and the erotic medieval temple sculptures of Khajuraho. While an exhibition of the former led to violent attacks by extreme right wing groups on grounds of sacrilege, the latter, despite its erotic subject matter, has been hailed as one of the nation's greatest artistic achievements, clearly demonstrating the near impossibility of fixing the boundaries of the artistic, the sexual and the obscene in the context of Indian art. In another essay sharing the same backdrop, the author demonstrates how archaeological data allows itself to be dangerously manipulated by nationalists. Performing an archaeology of two contentious sacred sites of history, Ayodhya (the Ramjanmabhoomi/Babri Masjid controversy) and Bodh Gaya, which in the late-nineteenth century became the site of intense conflict between local Hindu priests and Sinhalese Buddhist monks, the author explores the limits of archaeological jurisdiction, of evidence as 'record', and of the discipline's claims to 'truth'. The same theme has been explored brilliantly in a collection of essays entitled

*Nationalism, Politics and the Practice of Archaeology* (1995) edited by P. Kohl and C. Fawcett, however this important contribution seems to have been overlooked in the book.

In the section entitled “National Claims”, the reader moves back into the 1940s, to a historical moment that saw not only the birth of the Indian nation-state but also its first national museum. On the eve of independence an exhibition of masterpieces of Indian art, described as a “rich allegory of history and nationhood”, was held in the capital of the new nation-state. The author convincingly argues that this was a sign that Indian art history had become a “counterdiscipline”, founded on a privileging of intuition rather than reason, the mainstay of archaeology. With the exhibition, “Indian Sculpture” had transformed itself so successfully into an aesthetic and national entity (from that of its past colonial existence as a mere archaeological record) that it was decided to convert what was a temporary event-related display into a permanent national museum, the first museum to be established exclusively for the display of the nation’s artistic past. From the national exhibition and museum, this section goes on to examine a specific art object, a sculpture known as the Didarganj Yakshi, which became “over time the most canonised of the nation’s art objects” (p. 205). The author traces the travails of the Yakshi in order to deconstruct the official canon of Indian art as regards the female nude, a task which she continues into the ‘present’. Epitomised in the exhibition catalogue as the ideal Indian feminine beauty, a representative of a family of museum objects and as one of the masterpieces of Indian art from the past, the Yakshi is followed through to its provincial, national and international locations over the twentieth century. Brilliantly mapping its transformations from “an archaeological antiquity to a national artistic icon and an endangered art treasure”, the author constructs a richly textured biography of this storied Indian art object.

From the ‘National’ to the ‘Regional’, the story moves further back into the past, spanning the late-nineteenth and the early-twentieth centuries, marking the hatching of the indigenous scholar from within the folds of colonial training and scholarship. The author explores the ambivalences in and encounters with the master discourse that gave birth to the indigenous scholar in the works of Rajendralala Mitra (1822–91) and Rakhaldas Banerjee (1885–1930). Beginning with Mitra’s romantic interests in antiquities, the author traces his break with the past in his momentous ‘nationalist’ attack on the colonial art historian, James Fergusson’s interpretation of Orissan antiquities. While Mitra, based at the Asiatic Society of Bengal, straddled modern and traditional knowledge to establish himself as an amateur native scholar, Rakhaldas working in the early-twentieth century was a formally trained archaeologist. Later he held office with the Archaeological Survey of India and successfully dispensed with the label ‘native’, to redefined himself not only as a modernised and nationalised ‘Indian’ but also as a ‘Bengali’ archaeologist. To him the museum was the natural location of practice and the “catalog the central site for the testing and production of his expertise” (p. 115). The archaeologist who was able to determine provenance and dates of relics had begun to play a major role in inventing a new ‘scientific’ history that could break free of mythical constructions, including that of ‘the nation’. However, there was also an ontological need to explore and recreate popular historical memory around relics, without reducing the scientificity of archaeological practice, giving rise to ‘historical romance’ as a genre. Popular interest in ‘local’/‘regional’ archaeology also began to thrive, a development that has been read by the author as a sign of the maturity of the indigenous scholarship which developed from Mitra’s researches in the nineteenth-century. It was also the historical moment, that saw the theme of “Muslim conquests as the cathartic cut off point in the nation’s autonomous past” and as the “definite trope of nationalist history writing” (p. 138), including that of Rakhaldas’s, both professional and fictional. An over-emphasis on epigraphic evidence and dates in archaeological practice had become an obsession within the art historical scholarship of the twentieth century.

Another essay within this section on the ‘regional’ explains the vernacular turn in the writing on art and aesthetics in Bengal, contemporaneous to the Bengali archaeological scholarship, exemplified

by Rakhaldas in his book. Bengali writings on art expressed the very same yearning for the golden age of the “Bengali race” as in the latter, an essentially declensionist view of race and the nation. The essay takes as its subject, two Bengali texts on Indian art history by Shyamacharan Srimani (1874), a contemporary of Mitra and Abanindranath Tagore (1909), of Rakhaldas. The significance of their interventions rested on the way they situated their indigenous point of view within a predominantly western discipline and practice, art history and art: “They saw the privileging of a new aesthetic disposition in the conjuring of a national art, pitching it against the modes of scientific empiricism” (p.141) that philosophically founded the disciplines of history and archaeology. If the aim of Srimani’s text was to disseminate Indian art history which was the new knowledge, Abanindranath’s strategy was in contrast, a “bounding-in of terrain” through exclusiveness and stress on cultural difference. These two seemingly opposing strategies evolved to shape Indian art history as a new nationalised modern discipline

The last section (the first section in the book) to be discussed here, comprises two essays that relate to the colonial past. The first essay focuses on the works of two pioneering scholars and close contemporaries, James Fergusson and Alexander Cunningham, and the unfolding of their careers in Indian art and archaeology. The second traces the evolution of the Indian Museum, Calcutta from a ‘house of wonder’ to that of a new centre of disciplinary knowledge over the late-nineteenth century. Among other things the essay shows how Fergusson’s architectural (of form and styles) and Cunningham’s archaeological approaches (of excavation and texts) converged by the end of the nineteenth century to give rise to an extensive archaeological programme of excavation, conservation and classification of art objects and monuments in India.

### III

Even though Guha-Thakurta very convincingly demonstrates Fergusson’s self-positioning as a scholar of Indian architecture, replacing the aesthetic of the picturesque tradition with that of a scientific eye, a comparative analysis of the epistemic or philosophical foundations of the practices of the two colonial pioneers, Fergusson and Cunningham, remains a desideratum. A closer reading of a rare document as Fergusson’s travel diary of the 1830s is but a missed opportunity. For the *Picturesque Illustrations*, even when based on the diary notes, does not give the reader the kind of opening into Fergusson’s thoughts and questions regarding Indian architecture as the diary does. A rejection of a search for ‘origins’ and simple classifications in favour of a recognition of what I would call “palimpsest” monuments, expressed by his category, “the modern modified Hindu style”, are the most insightful of Fergusson’s ideas but absent in the book, despite their great implications for the present.

It is difficult to think of the collecting of antiquities or archaeological artefacts in India in the nineteenth century as detached from scientific travels of discovery with a concomitant sample-taking. Like the fauna, flora and rocks, antiquities were ‘discovered’, collected and classified and placed in a private collection or, as in the second half of the nineteenth century, placed in a public museum. What was collected was also visually documented as scientifically or ‘objectively’ as possible, whether it was a plant or a ruin. Fergusson did the same as the travellers before him whether Stirling, Buchanan, Mackenzie, Tod, Prinsep, Elliot or Waugh whose works Fergusson studied closely before he set out on his travels. However for Fergusson, who also contributed to colonial geology (an aspect ignored by the author), his travels (in contrast to Cunningham) were not about ‘discovery’ but of verification and falsification and in his writings on Indian architecture the articulation of a natural history episteme is best revealed. That this natural history episteme was not dissipated completely by the end of the nineteenth century despite museums having been transformed from cathedrals of science to the nation’s heritage keepers, is best evident in the straddling of disciplines by geologist turned art historian,

A. K. Coomaraswamy and the zoologist-cum-art historian F. H. Gravely based at the Madras Museum. (Incidentally, Coomaraswamy is wrongly described as Sinhalese rather than Tamil in this book which is concerned primarily with ‘the nation’)

The author makes a claim for the Indian Museum as “the first museum of colonial India” but this is entirely misleading. There is a need to clearly emphasise the word ‘public’ when one refers to the Indian Museum, the Madras Museum or any one of the museums that were established in the post-1840s. This is not merely a technical qualification but a major historical development, a signifier of public modernity that has gone unrecognised in the book. Even if one is right to speak of the Asiatic Society’s museum as the first museum in colonial India, it must be remembered that that museum was not ‘public’ as the Indian Museum was. Even if all the objects from the former were transferred to the latter when it was established in the 1860s, they remain two different museums, the products of two different historical moments. It may be pointed out that the earliest public museums to be established were the Madras Museum and the Trivandrum museum in the native state of Travancore, and these were established at least a decade before the Indian Museum.

Bengal is today easily the most studied geographical, political and cultural unit of colonial India and there is a strong need for studies on other parts of India, and in particular on princely states, of which we know but little. There is also the problem of monumentalising the nation or an overemphasis on the ontological at the expense of the epistemological, a problem throughout cultural studies scholarship, even in discussions on the shaping of knowledge and to that extent this book is no exception. None of these limitations however detract from the book’s obvious strengths, and there remains no doubt that it is a major contribution to the fields of art history and archaeology in India.

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LEGENDS, TALES, AND FABLES IN THE ART OF SOGDIANA. By BORIS MARSHAK, with an appendix by VLADIMIR A. LIVSHITS. pp. xi, 187. New York, Biblioteca Persica Press, 2002.

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Boris Marshak’s *Legends, Tales, and Fables in the Art of Sogdiana* is based on his Ehsan Yarshater Lecture series, delivered at the School of Oriental and African Studies from 10–17 May 1995. Like the lectures, the book serves as an introduction to Sogdian painting. In spite of the general title, the subject is Sogdiana’s most important site, Pendzikent, located about sixty kilometers east of Samarkand, near the border between Uzbekistan and Tajikistan.

For some thirty-five years, Boris Marshak has been the world authority on Pendzikent. Although Alexander Belenitskii published the results of excavations there in the 1950s, since the 1970s Marshak has been chief excavator. Since then, every major study of the paintings and architecture of this palace-complex in the Central Asian sands has been written or co-authored by him.<sup>1</sup> Yet *Legends, Tales, and Fables* is Marshak’s first book on the site.

Pendzikent refers both to the extensive palace complex that is the subject here and to the city-state of the kingdom known as Sogdia or Sogdiana, where it was located. The Sogdians rose in the sixth century, alongside the more powerful Hephthalites, and flourished amid Central Asian Turkic civilisations and were contemporary to the Sui and Tang dynasties (589–907) in China. The territory fell to an Arab empire in the mid-eighth century. By then, the Sogdians had become a significant presence in

<sup>1</sup> Works by Belenitskii and Marshak are listed in the bibliography of this book.

cities across Asia, including several in north central China. During their period of flourish, Sogdians were merchant traders from Europe to eastern China. Information about the Sogdians has come from records of every civilisation along the Silk Road, as well as inscriptions in their own language. Research on them requires knowledge of all of Asia and most of the Asian languages for a three-century period.

Following an introduction to the Sogdians, Chapter One turns to architecture. By 1995, twenty-nine sectors, some with more than one hundred rooms, had been excavated. No attempt is made at a definitive discussion. A full-length study of Pendzhikent's architecture was published by Marshak's long-time collaborator, V. I. Raspopova, in 1990.<sup>2</sup> An underlying question of that study is raised here, and, indeed, it will be a theme in the rest of the book: can one determine the social class of occupants of Pendzhikent based on the surviving material culture? Before the end of the chapter, it is suggested that the buildings belonged to a wealthy landowning and mercantile class, not peasants, but not aristocracy. Nor, Marshak says he will show, were they great horsemen.

The subjects of Marshak's next chapter are the paintings in the most famous room in the most famous Pendzhikent hall, the Rustam Cycle in the Rustam Room, originally in Sector VI, Room 41 of the site and now in the Hermitage Museum. Although everyone who has written about Sogdian painting has addressed this cycle, including Marshak himself, Marshak tells his reader that this will be the first thorough explication. He begins with a discussion of how the painter painted. Careful observation as a prerequisite for analysis has characterised Marshak's scholarship for decades, and this chapter is no exception. Marshak informs his reader that the artist repeated compositional units but that no two figures are identical. He traces the style to an Ancient Near Eastern principle that prohibited literary repetition. The author then explains the murals scene by scene, the cult scene, the riders, the many combats, between men and men with demons. Marshak's purpose, however, is to understand the artist, and he accomplishes this with sensitivity. He writes, "The painter . . . felt this paradox (between superhuman hero and mortal) deeply and showed it expressively" (p. 38). He explains diversity in four images as the painter's intent to show the hero's development in a like number of stages (p. 49). Marshak demonstrates how his painters move between figures and background, emptiness and immateriality. Literary and historic sources are interwoven in the text that only a mature scholar, fully versed in his subject, could write.

We remain in Room 41 in Chapter Three, in which Marshak discusses paintings above and below the Rustam images. Among them are more battle scenes, but also images traceable to Persian, Ancient Near Eastern, Greek, and Indian tales. These include the Spirit of the Ocean, Stupid Sandalwood Seller, the Wise Judges, the Hero and Three Animals, Elephant and Two Dogs, Bull, Lion and Jackal, Father and Sons, Witness Tree, Fairy in the Hollow Tree, Wolf With Seven Heads, Monkey and Elephants, King and Witty Servant, and others. The chapter demonstrates a mastery of literary sources from all parts of Asia, and, as Marshak concludes, gives new importance to literature known to the Persians, the probable direct sources for most of the images on the Pendzhikent walls.

Literary subjects from eight more rooms are explored in Chapter Four. The girl shown four times on a wall of Room 50 of Sector 23 is identified as one of Rustam's two daughters, given by him in marriage to a hero only after he has slain demons. This story with a clear agenda is contrasted to the many scenes of husbands, wives, suitors, and lovers. This is also the chapter in which one of Pendzhikent's most famous scenes, the Goose that Laid the Golden Eggs, is discussed. Like other scenes in Room 1 of Sector 21, the source is a fable of Aesop, but Marshak uses the painting to point out that the artist becomes comical here, contrasting the well-proportioned men and the enlarged goose. He makes the point that the depiction follows Aesop's fable, not the folk-tale with the same theme in which the man who kills the bird becomes a king.

<sup>2</sup> V.I. Raspopova, *Zhilishcha Pendzhikenta (Opyt istoriko-social'noi interpretatsii)* (Leningrad, "Nauka", 1990).

Marshak's conclusion is just what it should be. The author writes that his discussion of about forty scenes from Pendzikent's walls had the primary intent of informing the reader of their sources in literature and folklore. Thereby, he has shown that the Sogdians not only to have had a rich literature of their own (many of the stories made their way into Sogdian literature), but also through the literary basis of imagery how multicultural the climate of Sogdiana was. Yet he makes another point. The stories portrayed on the Pendzikent walls are secular: Christian, Manichaean, and Buddhist images in and of themselves are not depicted. If a scene appears Buddhist, for example, Marshak suggests it was borrowed for its didactic message rather than for theology.

Marshak makes another point that perhaps anticipates his next book. He calls the Pendzhikent murals the "tip of the iceberg" of Sogdian art (p. 160). In the last decade, as much Sogdian material has been unearthed in China alone as exists in the Uzbekistan-Tajikistan region. China's late sixth- and seventh-century capitals, particularly Taiyuan, Chang'an, and even Luoyang, have yielded Sogdian material that can be understood only through what is known about Pendzikent. Internationally, Boris Marshak has been one of the most active lecturers and spokespersons about sites from West to East Asia trafficked by Sogdians. We await a book by him on Sogdian finds from across the Asian continent, and perhaps beyond.

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MEDIAEVAL MANICHAEAN BOOK ART, A CODICOLOGICAL STUDY OF IRANIAN AND TURKIC ILLUMINATED BOOK FRAGMENTS FROM 8<sup>TH</sup>–11<sup>TH</sup> CENTURY EAST CENTRAL ASIA. By ZSUZSANNA GULÁCSI. pp. xiv, 253 Brill, Leiden and Boston, 2005.

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*Mediaeval Manichaean Book Art* is a carefully researched book about a highly specialised subject: illustrated books with Manichaean subject matter. The author is one of a handful of scholars who works on this material. To understand the subject requires knowledge of the painting and religious traditions of Iran, India, China, and the less major ones at all their borders. Zsuzsanna Gulácsi has done a noteworthy job of introducing the subject, on the one hand, and explaining the most important problems and issues associated with it, on the other.

Gulácsi's introduction is a true introduction, and one of the strengths of her book. Although written from a scholar's perspective, the introduction begins with the assumption that the reader knows little about the subject. One learns the basic data of Manichaean illuminated manuscripts: approximately 100 survive, most believed to have been painted in the Uygur winter capital, Qoço (modern day Turfan). The reasons they are related to the arts of the entire Asian continent are historical. Mani, founder of the faith known as Manichaeism, was born near Babylon in 216. After exposure to the religions of his day, Buddhism, Zoroastrianism, and Christianity, he began to preach his Religion of Light, carried by him and his immediate disciples to the Eastern Roman provinces, India, and Western Central Asia. Mani, himself, taught by drawing pictures. The religion received the support of Sasanian rulers from 240 until it was banned in 276. Mani died in prison. For the next five centuries, Manichaeism was largely a persecuted religion, but from the mid-eighth to mid-ninth centuries it became the official religion of the Turkic-speaking Uygur empire of North Asia, whose capital was at Karabalgasun. In the mid-eleventh century, Buddhism was in the ascendant, giving way to persecution of Manichaeism again and the termination of the great age of manuscript painting.

Next, Gulácsi provides the reader with a brief history of the scholarly study of Manichaeism, beginning with the Prussian expeditions between 1904 and 1914 that brought many of the most



famous treasures from the Turfan region to Berlin, and ending with Chinese research of the 1990s. She then turns to the necessary art historical background for this book. Codicology, she explains, is not taken in the literal sense of the study of the codex, but rather is synonymous with the study of manuscripts, or, even more generally, the study of complete (bound or otherwise put together) books. A brief explanation of the course of the author's research of the last ten years follows, and then, an extremely useful drawing on p. 13, in which the terminology of book illustration is labelled. This straightforward thirteen-page discussion of Manichaean book illustration is recommended for anyone who seeks to become familiar with the subject.

Chapter One begins the technical part of the book. A main purpose is the presentation of the author's list of illuminated manuscript groups and her justification for her identifications. Stating that excavation, alone, cannot be trusted, for sites retain material evidence of many occupants, Gulácsi explains that she has used literary sources, iconography, composition of the books and stylistic analysis to determine her database. By the end, we know how she came up with the number "about 100", among which there are 89 key examples, and that to see each one takes a scholar from London to Kyoto by way of St Petersburg and Quanzhou.

Chapter Two concerns the dating of the manuscripts. In addition to formal art historical analysis, close looking and comparison of details and comparison of images, and a survey of literary or historical sources, primarily in Manichaean and Chinese, as well as some in Arabic, the author discusses her most powerful tool, scientific chemical analysis. Radiocarbon tests confirm the history: the manuscripts date from the mid-eighth century to about a quarter of the way through the eleventh. Impressive to art historians will be the comparative stylistic analysis shown in figures 2/4, 2/6, and 2/7, in which carbon dating confirms the comparisons.

Chapter Three, one of the most technical in the book, is an investigation of bookmaking and scribes. Again, the analysis is based on careful examination. Gulácsi proposes that codices from Turfan were composed of a few thick quires, a quire consisting of at least fifteen sheets. She illustrates her findings on pp. 62–63. We next learn that binding holes are small, but sturdy, occasionally strengthened by gluing strips of paper along the folds. Double sheets were achieved in Manichaean works by splicing. Sizes and shapes of codices, techniques of leather book covers, scrolls, writing, decoration, and oblong folia placed between wooden covers without permanent binding, known as *pustakas*, all are examined here.

Chapter Four returns the reader to the study of paintings. Four sources of Manichaean illuminated painting are proposed and articulated, West Asian, defined by Gulácsi as tracing its roots to Persian, both painted and with the dominant use of outline; and Chinese painted and with dominant outline. The author concedes that the material is more nuanced than these categories, yet she can offer a conclusion. Manichaean book art is more allied with the arts of West Asia than East. Further, she has found little direct relation to Buddhist art.

Chapter Five raises an important issue of the illuminated manuscripts that is easy to miss because Manichaean painting is usually viewed in fragmentary condition. Figures are often oriented differently from words on the page. Design, pattern, and orientation are used to help understand orientation. In this chapter, the author proposes that three formats are among the fragments, textbooks, illuminated textbooks, and picture books. Based on her study, it can be confirmed that the majority were textbooks. Further, she has found that certain books were intended for pictures only. Contrary to previous notions, associated texts were not lost.

The last chapter deals with the art historical problem of the relation between word and image. Gulácsi's aim is to go further than previous studies in which individual fragments have been associated with specific texts, and rather to address the problem for all Manichaean illuminated material. Her first conclusion is that most illustrations are generalised, rather than specifically intended to highlight the narrative that they accompany. In other words, scenes from the life of Mani may accompany his

biography, and those scenes should have been identifiable by a practitioner of the faith through pictures alone, but only rarely is the written scene specifically illustrated. Although the author does not ask it, the question raised is how literate the illustrators were, and whether workshops painted scenes as scenes rather than as illustrations to texts. The twenty-one hymnbooks were even less tied to illustrations. Nineteen of them were decorated with pure ornament. Only one of six illuminated benedictions was decorated. The author concludes that illumination was an important part of the Manichaean book, but its function was primarily decorative. Words were to be read, not read with the help of images.

Beyond the conclusions for each chapter, the author does not offer general concluding remarks about Manichaean illuminated manuscripts. Nevertheless, Zsuzsanna Gulácsi has produced an extremely well crafted book. It is the book of a serious, responsible scholar, who understands both that her work will be read by specialists in her field, and that a book on such a specialised subject should not be superseded for a long time. The charts and diagrams provide important reference matter, for *Mediaeval Manichaean Book Art* is in some ways a reference work, but it is a high-level scholarly study as well that both introduces and explicates a subfield of Central Asian studies.

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HEIDEGGER, RORTY AND THE EASTERN THINKERS: A HERMENEUTICS OF CROSS-CULTURAL UNDERSTANDING. By WEI ZHANG. pp. 127. Albany, State University of New York Press, 2006.  
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This book evolved from a conference paper of the International Society for Universal Dialogue and formed part of the author's continuing research of Heidegger's thought in East-West studies, working specifically on his encounter with a Japanese visiting Professor, together with a discussion of Rorty's correspondence with an Indian 'comparative philosopher', Anindita Balslev. Rorty is there because he challenged the legitimacy of 'comparative philosophy' on the grounds that philosophy was a uniquely Greek concept and its terms of art could not therefore be applied to the East. Indeed he had earlier questioned philosophical 'essentialism' and saw the end of philosophy. But his position changed when he had to consider the possibility of a comparative philosophy and he became an essentialist remarking that philosophy was a form of conversation and exclusively a Greek concept.

In one sense he was quite right. Philosophy ('the love of wisdom') like botany was a Greek concept based on Greek learning. In the western world it started with Plato and persists as 'conversations in Europe'; for as an academic discipline, unlike botany, it has not taken cognizance, except very marginally, of parallel phenomena in other societies. When that does occasionally happen, it employs concepts that have been defined in a basically European context so that its extension elsewhere seems like part of the 'mission civilisatrice' connected with the European appropriation of learning that began with Antiquity.

The limitations of such an approach are well brought out by Balslev. It can perhaps be argued that philosophy more broadly has been confined to written cultures. Certainly they are the ones who are capable of encouraging conscious reflection of this kind but it would be a mistake not to recognise that even in purely oral cultures the use of language enables individuals to reflect upon the problems of human life and the 'love of wisdom'. Philosophy in that sense must be universal (if it has a subject matter at all) even though its practice (like history until recently) is determinately European. So that African philosophy has to struggle to make itself heard, not because Africans are not 'philosophical' but because they have not had writing. With Asia, things are different and if their ways of life have not been reflected upon in the same way as in the West, it is because of a different tradition and more than

a touch of the Eurocentricism built into philosophy. For it is ridiculous to suppose, with Hegel, that those outside the Greek tradition were supposedly without ‘discriminating ability’ or ‘transcendental consciousness’ and did not reflect upon the nature of existence. They did reflect but not as reflexively as is made possible in writing. However, philosophy in the Greek tradition was obviously limited to that very tradition; but ‘philosophy’ as a reasoned contemplation of life was certainly not, despite the contention of Windelband (and Husserl) that the intellectual activities of the Chinese were driven either by mere ‘practical needs’ or by ‘mystical fancy’.

Some recent philosophers have thought it necessary to look more intensively at what others have done – in this case not as ‘exotic’ or ‘colonial’ (even though some, such as Said, have been very pessimistic about any such enterprise). The author selects Heidegger because of his attempt to find a ‘common language’, especially in his conversations with a Japanese visitor, Professor Tezuka, of Kyoto. Heidegger poses a question to his visitor, whether “it is necessary and rightful for Eastasians to chase after the European conceptual systems” (p.50). Meanwhile the visitor suggests that western “metaphysical and technological thought had not taken root in Japanese soil”. Never the twain shall meet. However, Heidegger’s position is phrased in terms of language which he sees, mystically, as “the house of Being” (Dasein, a complicated concept that is elaborated at length), and precedes speaking. But language is tied to a particular culture and therefore cross-cultural understanding is virtually meaningless. European metaphysics originated in ancient Greek thinking whereas Japanese language is “usually perceived as a pictography” (p.54). Let us leave to one side the confusion between language and script and the constant tendency to think of literates as being the most authentic speakers of any language. Heidegger himself proceeds with his interlocutor to discuss Japanese concepts of language, which reveal ‘a cosmic dimension’. That conclusion affects his changing view that language does not simply contain but opens; it is now seen as “the flower of the mouth”, as enabling as well as limiting cross-cultural understanding.

The rest of the book has to do with Heidegger’s view of hermeneutics, of the history of language and its relationship to cross-cultural understanding. While this is interesting enough in elucidating Heidegger’s thought, it does not go far in addressing the problem of East-West understanding. Indeed it muddles any such enterprise by resorting to a Eurocentric version of philosophy (all began with the Greeks) and a Europe-dominated view of the East. The world process is seen as the Europeanisation of the East by its technology (an essential part of the difficulty is the contrasting of western ‘technology’ with the thought of Buddhism and Taoism, with their own spiritual approaches). It is this contrast that is so misleading. Japan had an advanced technology. China is described as the greatest exporting nation until the beginning of the nineteenth century. Needham’s work says a lot about Chinese technology and the invention of paper, of printing, of gunpowder, and the compass. The dichotomy between a technological west and a spiritual east is an invention of Europe of the nineteenth century. Yet for Heidegger (and others) the distinction is of an essentialist character. That is what is mistaken about the whole enterprise, the failure to be historical and to realise the temporary nature of European pre-eminence.

Heidegger speaks of the “Europeanization and technicalization of the earth and man” (p.77). But techniques flowed from one part of the world to the rest. Paper came from China, probably printing. Was this an Asianisation and technicalisation of the world? Film certainly changed the ‘lifeworld’ of the East but so did paper and printing in the West. If there is now an ‘all-consuming’ Europeanisation of traditional Japanese society (which has never been fixed and was earlier subject to Sinicisation) so too there have in the past been similar tendencies as this in the West.

The author, in conclusion, tries to show the possibility of East-West understanding at the level of ‘world views’ by emphasising the aspect of mobility in both the ‘onto-hermeneutic’ view of Heidegger and of the Buddhist, “a moving reality without a metaphysical origin” (p.101). But the philosophers

seem to be embarked upon a largely personal discussion about East–West understanding. Indeed the very phrasing of this problem in terms of East and West seems to be a western version of the problem, derived from a Greek picture that was highlighted by the Industrial Revolution and the temporary dominance of Europe. Where, for example, does Islam fit in, spreading from the north of China to the south of Spain, from East to West, and deriving much of its ideology and its practice from the same roots as Christianity and Judaism, quite apart from contributing so much to ‘modernisation’ in Europe? The Greek dichotomy of Europe and Asia assumes differences even where cross-cultural ties are many. However, of historical or actual ties neither Heidegger nor Rorty takes much account, preferring to work on another level with their own personal concerns. It is doubtful whether readers of this journal will find much to stimulate their thoughts on the comparison of ideologies, ‘world views’ or moralities from the works of these philosophers, or from their commentators.

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T’ANG CHINA: THE RISE OF THE EAST IN WORLD HISTORY. By S. A. M. ADSHEAD. pp. xvii, 233  
 Basingstoke and New York, Palgrave MacMillan, 2004.  
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These days, courses in “World History” are becoming increasingly regular fixtures on the campuses of universities in North America and elsewhere. While there appears to be a growing consensus among faculty and administrators that a global approach to history might be a valuable one, and while no account of the contemporary state of the world can afford to ignore the rapidly expanding economy of China, it has proved harder than one might expect to assess the historical role of China in world history with regard to economic factors. This new book by Professor Adshead, who has written extensively on China, Central Asia and material culture from a global perspective, offers some interesting points of discussion that may help to move the conversation forward.

André Gunder Frank, in his influential book of 1998, *Re-Orient: Global Economy in the Asian Age*, advanced a view of the world in which China was constantly central to global civilisation from 3000 BC onwards. In this volume, Adshead (who first reviewed Gunder Frank’s book in 1999), offers an alternative scenario: he argues that China rose to pre-eminence among major centres of civilisation only after 500 AD; that this rise was not only economic, but also political, social, and intellectual; that it peaked under the reign of the Tang emperor Xuanzong (713–756) and that from 1000 AD China was again gradually superseded by the West.

In focussing largely upon the Tang dynasty (618–907), Adshead paints a lively picture of a period that has long been acknowledged as a “golden age”. He also throws himself whole-heartedly into his critique of Gunder Frank, seeking to establish his point of view by evaluating the politics, economy, society, and intellectual currents of Tang China against those of contemporary India, the Islamic world (he actually uses the term “Islamdom” coined by Daniel Pipes, a controversial polemicist and director of a think-tank whose intent is to advance US interests in the Middle East), the Byzantine Empire, and “Latin Christendom”. But, while Adshead makes a strong case for Tang China’s multiplicity of strengths, it is hard to be certain whether he is drawing on the best available evidence for the civilisations against which he wishes to compare China. He relies on a rather narrow assortment of secondary studies, and in the case of the Islamic world, for example, those that he chooses are perhaps not the best available.

This book is clearly not intended as a work of world history based on new research, but rather as an extended essay that will encourage its readers to appreciate anew the contributions made to human

civilisation during a remarkable time. In particular, Adshead deserves to be credited for the stress he places on the crucial role of Buddhism in the rise of Tang China. As he demonstrates, the influence of the religion was vital in both internal matters (politics, society, economy and intellect) and in China's dealings with other political entities. Buddhist ideas and institutions (most notably the monastery) encouraged commerce and the accumulation of wealth, they introduced new modes of prestige and status, and in the intellectual realm they supplied a new vocabulary and iconography for thinking about the world (p. 86). In Adshead's estimation, it was Buddhism as both content and conduit of diplomacy that made medieval China the centre of a new East Asian order (p. 43).

In addition to emphasising the role of a foreign religion in driving medieval China to pre-eminence, Adshead also points to the significance of the reign of China's only female monarch, Empress Wu, in the bureaucratisation of government. Because, he claims, she was unable to rely on the personal – literally, “man-to-man” – management techniques of her male predecessors, she was obliged, almost unwittingly, to employ more impersonal methods: entry to the civil service by examination, promotion on the basis of merit, and government by due process (pp. 46–47). Adshead argues that these innovations were taken up and further developed by Empress Wu's successor Xuanzong, that their legacy lives on in contemporary consensus politics and that they account for the ability of Xuanzong's China to control both internal and external violence (p. 67).

Adshead's bird's-eye view of the religious scene in medieval China allows him not only to point to the intellectual complexity of Buddhism and the native traditions of Taoism and Confucianism, but also to perceive the interactions between these greater currents and the lesser streams of thought in medieval China (Nestorianism, Zoroastrianism, etc.). The profundity and richness of the resulting intellectual stew can be seen (for example) in Tang poetry, as he indicates. It is certainly refreshing to see a historian devote so much attention to the world of ideas, especially what might be considered the more abstruse religious doctrines of the period.

Those interested in world history will certainly enjoy this book, but others should read it as well. Historians of China, especially of the medieval period, will be interested in the way he constructs his argument, and the use he makes of his evidence. One hopes that it will convince a wide audience of the particular importance of medieval China to world history, and the significance of Buddhist ideas and practices in shaping the world we all live in today. If it encourages some young scholar to take up a closer investigation of the medieval religious world, then clearly all of us will benefit from a having a clearer picture of this phase of human history that, as Adshead shows, was so crucial to the story of global civilisation.

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ZHONG PU GUANXI SHI (HISTÓRIA DAS RELAÇÕES SINO-PORTUGUESAS), 1513–1999. By HUANG QINGHUA. 3 vols. pp. iii, 1430. Hefei, Huangshan shushe, 2005.  
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Huang Qinghua's work, published in October 2005, has received much attention in the Chinese press. There are several reasons for these reactions: the preface, by Zhang Haipeng, claims that this would be the most “complete” account ever written on the history of Sino-Portuguese relations. He praises the author for using not only Chinese and “foreign” but also Portuguese language sources, adding that during the last five hundred years no other historian had acquired such linguistic skills. Furthermore, the history of Macau would be placed into some sort of greater context, and the two Luso-Chinese agreements of 1862 and 1887 would figure as the key issues in the whole account. He then refers to

several visits to Portuguese archives, including the Ajuda library, where the so-called *grande rolo amarelo*, a well-known letter by the Qianlong emperor, was shown to him. The conclusion is that Huang – after an initial period of language training and some years of research – brought out a path-breaking *opus magnum* that will open a new chapter in Luso-Chinese history-writing.

Apart from the fact that the famous Qianlong document has been presented in several printed works during recent years (which may have escaped Zhang's and Huang's attention) and was often shown to foreign visitors, some of the other statements in the preface – and in Huang's own introduction which follows that preface – call for short remarks. First, there are many earlier works whose authors made efficient use of Portuguese and Chinese materials alike. The many Chinese books and articles by Jin Guoping and Wu Zhiliang are excellent examples of such scholarship. Second, the multi-volume edition directed by A. H. de Oliveira Marques, published by the prestigious Fundação Oriente and comparable to a “Cambridge History of Portugal's Presence in Macau and the East” (if there was such a thing), certainly takes a broader approach and is much richer in content than Huang's *oeuvre*. But strangely, neither Jin Guoping's publications, nor the Oliveira Marques collection, appear in the bibliography and notes of Huang's work. This is difficult to understand because Huang was well-treated by his Portuguese hosts (as he himself seems to suggest in his own introduction) and because he was also in touch with several members of the small scholarly community working in his field. Third, reading through Huang's text, one gets the impression that certain details were selected from modern secondary sources without a thorough control of the relevant printed primary material (or archival documents). Further below, I shall cite one or two examples.

By and large, Huang's account follows a chronological order. The first volume narrates the early history of Luso-Chinese contacts, embedded into the larger frame of Ming foreign politics; it then turns to the second half of the Ming period and finally to Sino-Portuguese relations under the early Qing (to c. 1840). Interestingly, the Ming part takes up more room than the Manju section, although the last segment covers a longer period. This ‘disbalance’ could be related to the author's neglect not only of the Oliveira Marques set, but also of G. B. Souza's *The Survival of Empire . . .* (Cambridge, 1986), A. Martins do Vale's *Os Portugueses em Macau (1750–1800) . . .* (Macau, 1997) and certain other works rarely cited in the notes (Vale's monograph also appears in the bibliography, with the subtitle missing). Another problem arises from the fact that Huang is mostly interested in the history of political relations and Macau's status, and not so much in commercial contacts (the term *guanxi shi* in the title of his work should normally imply a wide range of topics, including trade and cultural relations). However, he appends a whole chapter on the opium business and the so-called ‘coolie trade’ to his early Qing section, while there is no separate chapter on the silk-and-silver trade in the Ming section. Two explanations are likely for this structural “peculiarity”: first, he had difficulties in filling the pages of the last segment; second, the opium and coolie themes are more easily exploited for propagandistic purposes. The coolie theme, I may add, should have appeared in the second volume because in terms of chronology it mostly belongs to the post 1840-period. Moreover, important secondary works with relevant statistical evidence are not cited, while such one-sided accounts as a book by Huang Hongchao (quoted on p. 493, n. 2) should no longer be recommended to a modern (Chinese) readership.

The second volume summarises the Opium War period. Special attention is given to Macau's role during that conflict, the Nanjing treaty, and the years following it. There is also a long section on J. M. Ferreira do Amaral and his activities. These have been viewed very differently in China and Portugal, as is well known. No doubt, the opinions presented by Huang echo the Chinese perspective. Yet, M. T. Lopes da Silva's authoritative monograph should have been used; it is missing in both the notes and bibliography, as is certain information recently found in a genealogy related to the Zhao family in Macau. Furthermore, the collection *Guangdong Aomen dang'an shiliao xuanbian* (Beijing, 1999), pp. 45–46, might have been consulted to avoid a very misleading remark on p. 629 n. 1 (not to be commented

here). Be this as it may, the volume then proceeds by summarising the complicated diplomatic and other events preceding the agreements of 1862 and 1887, the negotiations of these agreements and, finally, the so-called ‘border talks’ aimed at fixing the boundary between the Manju state and Macau. This is mostly based on published material. The *Aomen zhuandang* collection and several sources edited by António Vasconcelos de Saldanha are frequently cited, in other cases – surprisingly – no publication is mentioned, only the code numbers of the original documents appear in the notes.

The third volume opens with a segment on the Republican period. It looks at the confused situation inside China, the clashes between rival warlords factions in Guangdong, and the implications of these unhappy circumstances for Macau. The famous strike movements of the 1920s and the Luso-Republican agreement of 1928 are described as well. As expected, the views presented in these sections mostly follow “standard” interpretations. There is also a section on Macau and the Anti-Japanese War. This part is very short – and disappointing because it fails to throw fresh light on the role Macau was forced to play during these dark years. The next chapter presents the history of Luso-Chinese relations in the post-war period (1949 to 1999). It is heavily based on Moisés Silva Fernandes’ voluminous work. This is followed by a brief and somewhat emotional conclusion. Finally, there are three “appendices” (two hundred pages in all): a chronological summary of important events (from the twelfth century through to 1999), a bibliography, and an index.

As was indicated above, several details presented by the author raise questions. Here, I shall only mention some examples drawn from the first volume. One point concerns the use of Ming and Qing sources: very often a particular text is based on earlier work. This pertains to a number of local chronicles, *lishi dili* records and other such texts, but the transmission of specific data and their validity were rarely considered. Furthermore, at times no page numbers are provided, as for example in n. 3 on p. 80, which contains a ‘simple’ reference to Huang Zuo’s (?) *Guangdong tongzhi*. Note 2 on p. 93 presents a different case: a page number does appear (probably because a modern text version with punctuation marks was used), but Huang forgets to tell his readers that the passage which he quotes from Zhang Xie’s *Dongxiyang kao* indicates an earlier source itself, namely the aforementioned *Guangdong tongzhi* (which edition?). Here Huang might have tried to locate the original citation. Other modern works on Sino-Portuguese themes, in my impression, are often more advanced and more reliable in these “technical” matters.

Next, some references to modern western works are inaccurate. Examples are found on p. 82 (n. 3: J. P. Oliveira e Costa), p. 89 (n. 1), and p. 91 (n. 2). Moreover n. 1 on p. 91 reveals that Huang misunderstood the relevant passage in Vieira’s famous letter, or failed to look up the text he cites (R. M. Loureiro’s edition). Another *faux pas* occurs on p. 150: Manuel Teixeira is heavily criticised. The problem seems to stem from a simple misprint in his *Toponímia* . . . (not *Toponimia*), but Huang inflates the story. Paradoxically however, he himself gives a wrong page number (see n. 3; p. 36 would be correct) – apart from the fact that quoting from the *Toponímia* in this particular context was not very elegant!

Be that as it may, the discussion related to Teixeira’s work concerns the name “Oquem”. Huang equates that name with Fujian province and not with Haojing (an old name for the Macau peninsula/region). To support his view, he quotes a relevant passage from Pires’s *Suma Oriental*, where it is stated that ships from Ryūkyū and other countries would sail to “Oquem”. This, he concludes, should definitely point to Fujian, because tribute missions from Okinawa nearly always entered China by way of that province. Obviously, Huang did not consult Tang Kaijian’s *Aomen kaibu chuqishi yanjiu* (Beijing, 1999), which lists evidence for the presence of Ryūkyū and other vessels in the Xiangshan/Haojing region – prior to the coming of the Portuguese (see pp. 67–68 there). One of the sources to which Tang refers in that context, is the Jiajing version of the *Guangdong tongzhi*. Since

Huang himself frequently cites this important work, one wonders, why he overlooked the relevant details.

On p. 167 Huang turns to the issue of ambergris. Wu Zhiliang and Jin Guoping have published a number of papers, arguing that Portuguese merchants were welcome at the China coast because they offered this precious substance that was badly needed in the imperial bedchambers. Earlier works also include occasional references to the ambergris “story”, one example being the old thesis by Lam Chee-shing (Lin Zisheng), originally completed in 1970, but only published in 1998 (as *Shiliu zhi shiba shiji Aomen yu Zhongguo zhi guanxi*). This monograph, I may add, was based on Chinese, Portuguese and other western sources alike (for ambergris, see pp. 25–26 there). Strangely, none of these works were cited by Huang Qinghua, neither in the ambergris section, nor anywhere else.

The question of whether or not Macau was given to the Portuguese in compensation for military services against pirates threatening the Pearl River estuary, has caused a never ending debate in scholarly circles. Of course, Huang arrives at conclusions similar to those propagated by such ultra-leftist writers as Dai Yixuan. Once again, he does not acknowledge the more interesting (and more conciliatory) views proposed by Tang Kaijian (*op. cit.*, especially pp. 114–116). Another work that one might consult in the context of Macau’s foundation is Tang’s *Weiliduo “Bao xiao shimo shu” jianzheng* (Guangzhou, 2004). Probably this book came out too late to be considered by Huang (?). Additional suggestions are found in a number of special studies, as for example in an important article on “Tchang Si-lao” written by Jin Guoping.

On pp. 176–177 Huang briefly deals with the name “Chincheu”. Again, this falls short of modern scholarly standards. Readers are referred to an old note by Charles R. Boxer, but not to the relevant essay by Jin Guoping in his collection *Xi li dong jian* . . . (Macau, 2000). Giovanni da Empoli is mentioned under two different names (pp. 85, 92); M. Spallanzani’s and L. A. Noonan’s authoritative books should have been cited here. The early history of Sino-Portuguese contacts just after the conquest of Melaka cannot be understood without looking at the pepper trade; this has been analysed by Jorge M. dos Santos Alves and others, but was almost completely ignored by Huang. Unbelievable as well: even R. M. Loureiro’s celebrated *Fidalgos, missionários e mandarins. Portugal e a China no século XVI* (Lisbon, 2000) is not listed in Huang’s bibliography!

Several modern Chinese books on the history of Sino-Portuguese relations contain useful lists of the Xiangshan officials, the Macau governors and the local *ouvidores*. One would expect to find such an appendix in a major survey, but there are no such lists in Huang’s account. On the other hand, Huang presents the names and particulars of twenty-five commercial vessels registered in early Qing Macau, which seems like “drawing a snake with legs” (to put it in Chinese terms), given that he is mostly interested in the development of political relations and not so much in the history of trade. Moreover, he fails to fully acknowledge the absurdities of the relevant shipping regulations that were imposed in 1725 (see pp. 352–354). For this he could have looked up the documents in the collection *Sinica Lusitana*, vol. 1 (Lisbon, n.d. [preface 1996]), pp. 76–89.

A long section is devoted to the famous case of Li Tingfu and Jian Ya’er. This also concerns the role of Zhang Rulin and certain other personalities. However, I did not find a single reference to the book by Vale (see above) and the relevant studies in Tang Kaijian’s *Ming Qing shidafu yu Aomen* (Macau, 1998) and Jin Guoping’s *Xi li dong jian* (see above). There is also something wrong with certain quotes from Beatriz Basto da Silva’s well-known chronology and Teixeira’s *Toponímia* . . . (see, for example, p. 412 n. 1, p. 421 n. 1, and p. 422 n. 1 in Huang’s account). For more details on the “belligerent” Bishop H. de Santa Rosa (see pp. 440–441) and his strange proposal to undertake a military move against the Qing, the author might have drawn attention to some works by A. Graça de Abreu, M. Teixeira and, once again, A. Martins do Vale. Regarding the many missionaries mentioned by Huang,



readers are occasionally referred to the old handbook by L. Pfister, but rarely to the modern standard monographs completed in more recent times.

The above list of *monita* could be continued, but I shall restrict myself to a few final observations instead. At times, Huang does not indicate his sources. An example is found on p. 823, where he discusses and translates some very special legal terms into Chinese. The Portuguese versions and their Chinese equivalents were probably taken from an article in *Aomen falü xuekan/Revista jurídica de Macau* 3.2 (1996), especially from pp. 50–51 of the Chinese version, and from A. Vasconcelos de Saldanha's *Estudos sobre as relações luso-chinesas* (Lisbon, 1996). Next, the bibliography carries a number of inconsistencies; this concerns the translation of Portuguese book titles and other aspects. Furthermore, on pp. 6–7 Huang comments on important secondary works; unfortunately he leaves out many titles (some of which were indicated above) and thus gives the impression that he is not very well informed about recent academic developments. It would have been more elegant and more opportune to 'simply' list a number of recent research tools and bibliographical guides in lieu of just giving an arbitrary selection of secondary titles. These and other facets do make Huang's account very vulnerable, indeed.

*Zhong Pu guanxi shi* leaves a bad aftertaste. During the last two decades Chinese scholars have made enormous progress in finding out more about Macau's past. The Aomen jijinhui in particular has promoted Chinese research on Sino-Portuguese themes. A number of excellent publications were also brought out by Zhonghua shuju and other respected publishers in the People's Republic. Moreover, to a certain extent, Chinese and Portuguese scholars have succeeded in overcoming "ideological gaps". Huang Qinghua, it is my feeling, did not only ignore many of these positive results – worse still, he has turned back the wheel of history-writing by insisting on certain 'traditional' patterns of interpretation. His work, I am afraid, will be rejected by an enlightened Chinese readership and most specialists in the field. In short, there is a wide gap between the preface and the account as such – *ming bu fu shi*.

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GATEWAY TO JAPAN: HAKATA IN WAR AND PEACE, 500–1300. By BRUCE L. BATTEN. pp. 183. Honolulu, University of Hawaii Press, 2006.  
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This book is partly an academic work and partly a popularisation. Batten uses colloquialisms ('sound bites') as well as more academic discourse. This mixing of genres does not lead to a fragmented book and, in fact, provides a valuable introduction to a subject not often addressed in English-language sources. Its specific focus is the port city of Hakata on the island of Kyushu, but the general theme is Japanese foreign relations.

Batten asserts that Japan was heavily influenced by foreigners and foreign practices and institutions and that Hakata was the principal historical venue for such interchanges. Hakata's proximity to the Asian mainland made it the conduit for contact with Korea and China. On the one hand, Japan was exposed, via Hakata, to Confucianism and Buddhism, the Chinese written language, and Chinese institutions. On the other hand, the Yuan or Mongol dynasty targeted Hakata as the landing spot for a projected invasion of Japan in 1281. Batten offers a judicious assessment of the controversy surrounding the scale of this invasion. Hakata was also central to Sino-Japanese trade until the Tokugawa era. Chinese textiles, medicines, and spices reached Hakata during the Tang and early Song dynasties. The early Tokugawa period witnessed a diminution of such relations because the government limited contact with the outside world, resulting in a decline in the importance of Hakata. Matthew Perry's expeditions

in 1853–54 compelled Japan to open ports on the main island of Honshu, leading to a further decline in Hakata's significance as the main centre for such foreign contacts and influences.

Although much of this story is well known, Batten's book is a compelling account and eminently readable. It has also been provided with excellent maps and illustrations, contributing immeasurably to a reader's interest and enjoyment.

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THE ROLE OF BODHICITTA IN BUDDHIST ENLIGHTENMENT INCLUDING A TRANSLATION INTO ENGLISH OF THE BODHICITTA-SASTRA, BENKENMITSU-NIKYORON, AND SAMMAYA-KAIJO. By KENNETH R. WHITE. pp. 504. Lewiston, New York, The Edwin Mellen Press, 2005.  
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The length of the title of this book is indicative of one major weakness that makes unnecessarily complicated reading of what is otherwise a useful text. Lengthy and convoluted sentences do not assist comprehension and can produce negative reactions in readers' minds.

The book is a study of how the concept of *Bodhicitta*, defined as the “mind that seeks and reflects enlightened insight”, developed and was employed in the thought of Kukai, the Heian period (785–1184) founder of the esoteric school of Shingon (literally ‘true word’) Buddhism in Japan after his lengthy visit to China. The central question reduces to how the process that leads to true enlightenment can be accomplished, “‘in this very body’, based on appropriate and directed dedication to that end. This was to be realised by means of the execution of rituals designed to provide the conditions necessary for man–Buddha integration, facilitated and characterised by the integration of the practitioner's actions of body, speech, and mind and the subsequent union with those corresponding and reciprocating functions of the three secrets of the Dharmakaya Mahavairocana”. (p. 177) This is expressed in Japanese by the term *soku-shin-jo-butsu* (即身成佛) meaning literally “to become a Buddha in the flesh”. This approach is explained and discussed through texts written by Kukai that deal with aspects of the theme. This is supplemented by an intellectual biography of Kukai and how his understanding of the idea matured. The discussion is detailed and interesting and is supported by texts in which Kukai develops his ideas. Chapter VII is the Chinese text of the *Bodhicitta Sastra* with an English translation. Chapter VIII is the text of the *Benkenmitsu-nikyoron*, a text that compares the esoteric (Shingon teachings of Kukai) with the semi-esoteric teachings of Tendai thought. Chapter IX is the Chinese text of the *Sammaya-Kaijo*, a primer in Shingon thought attributed to Kukai that deals with the stages through which the mind passes to reach the highest level of understanding. Having these texts with English translations and extensive supporting notes is extremely valuable for any students of Kukai, and this work is a major contribution to the study of a tradition of Japanese Buddhism that merits much more attention than it has received.

I am left however, as a reviewer, with three questions that, while not ultimately damaging to the book, do suggest that it lacks completion in certain respects. The first relates to the general issue that the author makes it harder to read than it might have been. For example, the order of appearance of Chinese characters and the reasons for insertion are not consistent. Inserting Japanese *kanji* for technical terms makes sense. But on page 66, there is a reference to the Todai-ji, superfluously called the Todai-ji Temple, since the character *ji* means temple. The name appears again two lines below, but this time with the *kanji*. A minor and pedantic point perhaps, but wouldn't Todai-ji (東大寺, Great Temple of the East) have been more helpful as the initial entry? I see also no need for the Chinese characters for the

name of Emperor Kammu to be included, especially in double brackets with the superfluous addition of 天皇 *tenno* after his name. Likewise the use of *kanji* for the Meiji period, usually dated 1868 to 1912 when Emperor Meiji died, is really superfluous. Also the texts and translations might have been more usefully interfaced, although I accept that this could have raised production difficulties. The end result appears, however, a little bit more like a collection of class materials than an integrated work.

The second question concerns the point that, even from its early stages in Japan, Buddhism had to coexist with the local cults of the *kami*, what Joseph Kitagawa referred to as the amorphous tradition known as Shinto. Most mountain temples were amalgams of Shinto and Buddhist rituals. It was not until the Meiji period separation of Buddhism and Shinto under the provisions of *shinbutsu-bunri* that identities were clearly distinguishable. But traces of the earlier traditions are still visible. Mitsumine Jinja in Saitama Prefecture includes in its long history an influential visit by Kukai. Tsubaki Okami Yashiro in Mie Prefecture, which enshrines a *kami* from the mythology, practices *goma* the Shingon fire ritual. The reference on page 84 to the Kongobuji having ceased to have that name after the Meiji Restoration I find puzzling. While it is true that mountain sites were popular for temples in China, the Japanese fondness for mountains is equally primeval in origin. At any rate, there are a number of questions that have been ignored.

The final question I have to raise about the book is that it stops short of a complete discussion of the author's stated hope "that it will serve to further the understanding of *bodhicitta* thought in particular, and enlightenment theory in general, as it is employed in the context of Japanese Shingon Buddhism". (p. 5)

The idea of "becoming a Buddha in the flesh" developed a history of its own within both Tendai and Shingon Buddhism that went far beyond the purely philosophical. The monks of Hieizan still practice a discipline known as *sennichi-kai-ho-gyo* meaning "one thousand days around the peaks". They run the equivalent of 25,000 miles over a period of seven years. Completion receives recognition from the Imperial Household. Shingon developed the more extreme idea of mummification into a *miira* as its spiritual apogee. This was developed especially in the culture of the Dewa Sanzan mountain ascetic centres located in what is the modern Yamagata Prefecture. After a period of disciplined eating to remove oils and fats from the body, self-interment according to a prescribed ritual followed. Bodies removed in a state of perfect mummification were treated as living Buddhas. The most famous *miira* is Tetsumon-kai-shonin, an Edo period monk who is still revered and annually removed from his glass case to sit with his circle of devotees. While these manifestations may seem bizarre and far removed from Kukai's philosophical purity, they were popular practical methods of achieving instant Buddhahood. While Kukai may not have necessarily approved, they cannot be ignored as part of the Japanese transformation of Buddhism. Perhaps the author might consider a follow-up work covering the subsequent manifestations of esoteric Buddhism, particularly where it encountered Shinto practices and combined with them in the great syncretistic movement known as Shugendō?

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UNDER THREE FLAGS: ANARCHISM AND THE ANTI-COLONIAL IMAGINATION. By BENEDICT ANDERSON. pp. 255. London and New York, Verso, 2005.  
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This book attempts to trace the impact and influence of anarchism on anti-colonial nationalism, or as Benedict Anderson puts it in his introduction the "gravitational force of anarchism between

militant nationalisms on opposite sides of the planet". Focussing on Cuba and the Philippines, the most important remaining colonies of Spain's dying Empire, Anderson maps the political current of anarchism in the final decades of the nineteenth century, by moving breathtakingly from the European cities of London and Paris, to South America and Rio de Janeiro, to the East and to Hong Kong, Singapore and Manila, taking in several other cities in between. He views this journey as an experiment in "political astronomy" and devises a method which he describes as a blend of "Eisenstein's montage" and the thrilling uncertainties of an unfolding *roman-feuilleton* novel, enjoining his reader to imagine she is "reading a black and white film" or a novel that revels in cliff-hangers.

Broadly, Anderson follows a two-pronged line of argument. First, he aims to show a "transglobal coordination" between Cuba's nationalist revolution in 1895, the last to occur in the New World, and the Philippine nationalist revolution a year later, the first to occur in Asia. Their "near-simultaneity", Anderson argues, was no mere coincidence. New technology: the invention of the telegram, the widening postal systems and railway networks, for instance, had succeeded in ushering forth a form of "early globalisation" which drew the ends of the earth closer, thus making a wide range of events, conditions and consequences – from scientific discoveries, the movement and making of capital and profit, to military conquests and defeats – both easy and swift.

Second, Anderson stresses the cosmopolitical nature of Filipino and Cuban nationalism by highlighting the cosmopolitanism of the elite nationalists themselves. Multi-lingual, geographically mobile and impressively cultured, this group of Filipino elite, patriotic intellectuals who are the focus of Anderson's study, formed friendships with and found allies in Europeans (British, French, Spanish and German), whether they were liberals, anarchists or simply sympathisers. Moreover, these patriots abroad kept themselves and their countrymen in the Philippines abreast of world events through reports in newspapers, through letters and telegrams, and, significantly through their travels. In the process, Anderson argues, both Filipinos and Cubans could "learn how to 'do' revolution, anti-colonialism, and anti-imperialism".

By situating nationalism within these larger currents of modernity and cosmopolitanism, Anderson rightly unhinges the late-nineteenth century Cuban and Philippine revolutions from their provincial moorings and often parochial historical interpretations, and analyses them as being very much part of changing global conditions, shaped and pushed to a significant degree by prevailing transnational forces. The fruitfulness of this panoptic approach has been proven by other historians who have, like Anderson, explored the question why social changes occurring in distant regions of the world, with no apparent or obvious connections or ties, should have experienced similar transglobal coordinations or overlapping cycles of change.<sup>1</sup>

Most interestingly, Anderson chooses to foreground three renowned Filipino patriots whose remarkable lives and works serve as an anchor upon which anarchism and its influences are discussed. Anderson's fascination with the astute political campaign organiser Mariano Ponce, the pioneering folklorist and journalist Isabelo de los Reyes, and above all, the polyglot and novelist Jose Rizal are plain. Anderson ranges across their considerable achievements: novels, essays, historical research, while at the same time, he endeavours to capture with intimacy, familiarity and warmth, their individual personalities. The ebullient, energetic, thrice married and virile Isabelo (father to fourteen children), is for example, contrasted with the self-conscious and sensitive Rizal who sired no children. To examine the works of these accomplished men in tandem with some of the most exciting scientific thinking and literature to be produced in Europe in the nineteenth century, necessitates the exploration of not only the development of their "anti-colonial imagination", but the exuberance of their intellectual adventurousness and the rich complexity of their imaginations. In two chapters, Anderson discusses

<sup>1</sup> Victor Lieberman, *Strange Parallels: Southeast Asia in Global Context, c.800–1830 Volume I: Integration on the mainland* (Cambridge, 2003).

Rizal's politically explosive novels *Noli me Tangere* (1887) and *El Filibusterismo* (1891), commendably taking the lively route of examining the novels for their biting humour, or Rizal's "unquenchable laughter" as Anderson describes, and references to sex. Highlighting passages that allude to the sexual lust of a friar, and male and female homosexuality, Anderson speculates on the eclectic sources that may have inspired the young Filipino. Here, Anderson finds correspondences in Rizal's sexual insinuations, which pepper both novels so spicily, with parallel scenes that can be found in works by the scandalously avant-garde half-Dutch and half-French novelist Joris-Karl Huysmans, with Edgar Allan Poe, with Baudelaire and Mallarmé. This novelistic melting pot is not entirely convincing and the links are tenuous. As Anderson himself quickly admits, while Rizal's novels are filled with classical references in Latin and Hebrew, and his vast library attests to his wide reading, there is no evidence to show any of these authors had been read by Rizal. What is significant is that Rizal was at the right place at the right time, and at the right age, to have been both excited and inspired by current, European literary activity. Rizal had made the first of many visits to Paris in 1884; he was an attractive, quietly confident and acutely observant twenty-three year old, and had arrived just over a year after Huysmans' *À rebours* had been published, a novel that had succeeded in outraging *bien-pensant* bourgeois society with its seductive and exotic sex scenes. What occurs, Anderson suggests, might be attributed to the magic of alchemy. "Rizal's originality", Anderson writes, "lay in the manner in which he transposed, combined, and transformed what he had read". It is the experience of Europe, Paris in particular, that provided the stimulation in an already fertile mind for ideas to take shape and crystallise.

It should be remembered that Rizal's novels are serious critiques of colonialism. But it is his mocking laughter and, at times, gossipy tone, that give the works the flair and sophistication found lacking in say, Benito Galdos's monumental *Doña Perfecta* (1876), a work scholars habitually compare to Rizal's *Noli*. A novel concerning a decaying colonial society and the depredations of imperialism, Anderson points out, has never been more enjoyable to read than Rizal's *Noli*. Anderson, as he is only too aware, has built a case for anarchism based on circumstantial evidence. In the Philippine case, his argument, he must surely admit, is largely unconvincing. Proving the political influence of anarchism in the formation of an anti-colonial imagination amongst a Filipino elite intelligentsia, who in the late 1880s and early 1890s were enamoured by the economic and scientific progress, as well as the bourgeois cultures of western Europe, and more interested in social reforms rather than revolution, was a tough hypothesis. What he has achieved is a stylishly written study that has allowed him to exhibit his considerable erudition – his broad knowledge of world history, his linguistic skills and his abiding interest in Philippine history.

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THE HERITAGE OF TRADITIONAL MALAY LITERATURE. By VLADIMIR BRAGINSKY. pp. xiv, 890. Singapore, Institute of Southeast Asian Studies, 2004.

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The content of this monumental study of traditional Malay literature is eclectic. Some chapters are largely theoretical, some consist mainly of summaries of certain literary works, some include lengthy translations of sections from Malay works, and some include detailed analyses of the literary-historical and philological features of the Malay works consulted.

Braginsky sets the scene by reviewing pioneering studies in the field. He begins with early colonial figures, commenting that "the theoretical level of the majority of these scholars' works was fairly low . . ." (p. 5), and observing that they were heavily influenced by European methods and biases, and

rarely produced a positive appraisal of the Malay literary works being examined. He then considers more sophisticated studies by later scholars, especially those by Hooykas and Winstedt. He identifies positive features of these studies, but is also forthright in pointing to their deficiencies. He observes that “the future builders of historically-focused researches into traditional Malay literature will have to restructure large sections of Winstedt’s foundation before they can raise further the height of the walls that rest upon it” (p. 11).

The author then moves onto scholarship favouring synchronic studies. Their focus on the structure and function of particular works, “allowed researchers . . . to come closer to an understanding of traditional Malay literature as a systemic unity” (p. 12). Nevertheless Braginsky sees particular theoretical problems in studying traditional Malay literature, especially the “problem of revealing the integrated character and artistic value of the literary works being researched” (p. 18). He pays particular attention to Sweeney and Koster, giving credit where credit is due but also identifying methodological gaps. He shows his hand by defining, “the development of traditional literature . . . as the evolution of literary (generic) systems that come to replace each other in the course of history” (p. 22).

Braginsky sees his own study as presenting a “historical survey of Malay literature from the seventeenth to the nineteenth centuries . . . based on a specific theoretical approach which . . . imbues its entirety, and has a definite, historically unfolding ‘plot’ running throughout it” (p. xi). To attempt a study of the broad landscape of traditional Malay literature and see it as a unit rather than a disparate set of works and themes cobbled loosely together is both ambitious and exciting.

The author argues for a three-way periodisation in the history of traditional Malay literature. First comes the Old Malay period, during the Indianised states (seventh century to the first half of fourteenth century). This is followed by the early Islamic period (second half of fourteenth century to the first half of sixteenth century), and then the Classical period (second half sixteenth century to the first half of the nineteenth century). Braginsky then proceeds to survey the literary output of each of these periods in turn.

As Braginsky points out, Chapter One is “the first attempt to outline, by way of reconstruction, the genre composition of old Malay literature” (p. 49). He makes lateral references to typologies of medieval literatures from European cultures, and consults inscriptions on the great temples, such as the Borobudur, arguing that the prototype of the *Hikayat Seri Rama* derived from popular, oral versions from southwest India. Through it, the *Ramayana* had a longer-lasting influence on Malay literature than the other great epic of the *Mahabharata*. Braginsky’s conclusions about Old Malay literature in the Indianised states are “tentative” (p. 49), and his reconstructed system will in due course be proven or disproven by subsequent research. The key thing is that Braginsky has started the ball rolling, making this a very valuable chapter.

Chapters Two and Three address the problem of early Islamic Malay literature and its genre system. Braginsky points out that much debate still surrounds the composition of the earliest Malay literary works. He surveys several of the debates, including those regarding the dates of composition of the *Sejarah Melayu* and the *Hikayat Raja Pasai*. He distinguishes between the functional sphere of literature (works on basic principles of Islam, *fiqh*, Arabic grammar, Sufism) and the non-functional sphere (renderings of Indian epics: *Hikayat Seri Rama*, *Hikayat Pandawa jaya*, Javanese tales about Panji). He points to a “period of transition” characterising this period, with two genres dominating: prose narrative (*hikayat*) and chronicles (e.g. the *Sejarah Melayu*).

Braginsky’s extended study of the Classical period of traditional Malay literature occupies Chapters Four to Eight. He reconstructs self-awareness of Malay literature in this period (Chapter Four), challenging prevailing views that surviving literature does not enable us to explore literary self-awareness of the classical Malay litterateurs, and stating that “prefaces and epilogues to Malay literary works, both in verse and prose, have served as the main material for this reconstruction” (p. 208). He

identifies a hierarchically-ordered system between the Creator at the top and the self-perception of the reader at the lower level, which is helpfully summarised in visual form (p. 270).

Braginsky then uses this discussion of self-awareness to propose a genre system for this literature (Chapter Five), stating ambitiously that “. . . the outlines of [Malay literature’s] hierarchically ordered system provides the researcher with an opportunity to find his bearings in the sea of Malay literary writings, not deviating too far from the general standpoint of their creators and readers” (p. 301). He proposes three functional spheres in the literary system – beauty, benefit and spiritual perfection – stating that “each functional sphere fulfilled its task through a number of genre structures” (p. 301).

The genre structures are presented as Hikayat (prose narrative, tale, romance); Syair (narrative poem); Sejarah/salasilah (chronicle, genealogy); Kitab (treatise); Hidayat (nasihat, “edifying mirror”). Braginsky then relates the genre structures to their respective functional spheres: Benefit (sejarah/salasilah, hidayat, hikayat, syair, kitab); Beauty (hikayat, syair); Spiritual perfection (hikayat, syair, kitab). Throughout his discussion Braginsky draws on multiple works to illustrate his ideas, demonstrating his detailed knowledge of the Malay literary corpus.

Braginsky proceeds to explore numerous prose and poetic works of the Classical period (Chapters Six and Seven) and Muslim hagiography and Sufi literature (Chapter Eight), considering each from the perspective of the various functional spheres and genre structures presented earlier.

The author’s first article (mentioned in the bibliography) appeared in 1969. For the next 20 years the majority of Braginsky’s output was in Russian and, as such, was inaccessible to most scholars of Malay studies (including myself). So the present volume is welcome, as it brings together most of Braginsky’s life’s research thus far.

This study has gone through several previous versions: a 1983 prototype in Russian, a 1993 version published by KITLV Press and a 1998 Indonesian language version. So the author has had ample opportunity to develop his thoughts, test out his arguments, receive feedback and present an authoritative version drawing on his almost forty years of research in the field of Malay literary studies.

This is no book for the faint-hearted. At not much short of 1,000 pages of smallish type, readers need to have a deeply ingrained interest in traditional Malay literature. Furthermore, Braginsky states that the study is “intended primarily to be read as an integral work” (p. xii), in order to grasp the overall system of traditional Malay literature.

Nevertheless, the study is also suitable for dipping into sporadically for those interested in a particular work or genre. The volume is encyclopaedic in its detail, reflecting the author’s energetic involvement in the field of Malay literature over several decades.

As for the validity of Braginsky’s systematised portrayal of traditional Malay literature, he himself points out that another one or two generations of research is needed before the map of this literature approaches the level of understanding of that of European literature. No doubt subsequent researchers will make adjustments to Braginsky’s system. However, his contribution represents an important stage on a journey. As Braginsky himself points out, this volume is valuable for the broader Southeast Asian scene in terms of comparative literary studies, as it helps to elaborate reliable models for the comprehension of traditional Asian literatures in general. The original and creative approach that Braginsky has taken throughout his life’s work, as represented in this volume, makes it an extremely valuable and important addition to the corpus of studies on Malay literature.

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