Reviews of Books

Sancti Gregorii Nazianzeni Opera. Corpus Christianorum. By Hélène Metreveli (ed.). (Series Graeca 45. Corpus Nazianzenum 12). pp. xv, 221. Turnhout, Brepols, 2001.

This volume, a cut-before-you-read paperback, is the third in the series of the Georgian translations of Gregory Nazianzenus' writings that began with a sumptuous hardback in 1998 (see my review *JRAS* April 2000). It is dedicated Oration XXXVIII *In Nativitatem vel in Theophania* and contains on facing pages the 9th-century rendition from the Athos and T'bet polycephala (pp. 2–45, the 10th–11th-century translations by Euthymius the Athonite and Ephrem Mcire (pp. 50–119), and a commentary on the text (pp. 121–219) that is present in almost all the Euthmian manuscripts and which may not merely be a translation by Euthymius but, as no Greek original is known, actually an original work by him – the Euthymian but not the Ephremian version indicates acquaintance with the earlier polycephalic renditions; the main source for the Ephremian text is the 12th–13th-century ms C (Jerusalem 43), as for this Oration ms A comes from an 18th-century copyist's hand. Additionally, there is a short extract (p. 47) of the Oration included in the T'bet and P'arkal polycephala and a five-line dedicatory iambic verse (p. 49) that precedes the Ephremian translation in Jerusalem ms. 15. The Introduction is in French, and, as noted for vol. I, the (to linguists) annoying practice is repeated of leaving ejective consonants unmarked whilst employing the apostrophe (rather than the more usual reverse apostrophe) to indicate aspirates, a convention that, as in my previous review, I do not follow here. Textual notation is of the standard type, as described for vol. I of the series.

The first director of the Tbilisi Institute of Manuscripts, Ilia Abuladze, proposed that the short fragment from the T'bet and P'arkal polycephala will either have come from an earlier lost translation or derive from a compilation based on some other language. That fragment reads thus:

> [Oration/Reading] of St. Gregory the theologian, Bishop of Nazianze, on the birth of the Lord.
> 3.8. For God appeared, born of flesh among men, who was and ever is of eternal essence.
> 9. And He was made for us finally a man, (and) as he gave existence to men just so in order to grant them a better life and the more so in order through his own incarnation to set them once more upon their true worthiness after the evil fall. And revelation is the name given to his appearance, and Birth(/Nativity/Christmas) to the birth.
> 4.10. This is for us a gathering, and this festival shall we celebrate today. And a little later we shall see also the baptism of Jesus in the river Jordan for our purification and the more for the purification of the waters.

This may be compared with Euthymius' translation

> 3.8. But this finally. And now this gathering is of God's appearance and birth, and both these are articulated by two words which however point to the one thing, for God appeared to men

through birth. And his birth is primary and eternal, higher than causes and the word from the eternal essence.

3.9. And second this birth, for us at last, so that the giver of life might grant to us a noble existence. Rather indeed we shall say that we have been changed from evil-doing through noble existence, so that he might convert us to the good [*k'etilad* vs Ephrem *k'ualad* = again] through his manifestation. And for this incarnation was named the revelation of God and the birth Birth (/Nativity/ Christmas).

4.10 This is our gathering; this shall [Ephrem: do] we celebrate. Today is the coming of God for men so that he might bring men close to God and that we turn towards him. And indeed it is more fitting to say it thus: we have stripped off the old man and donned the new, and just as we perished thanks to Adam, even so shall we find resurrection through Christ having been born together with Christ, born and crucified, buried and resurrected [with him].

To give a flavour of the content of the Commentary let us take a passage arising out the section just translated:

45. And we are buried along with Christ in the sense that, as in a grave, so we descend into the spring of baptism. And in it the old man is covered and consumed entirely. And when we rise thence, along with us comes up the new man, and in this way do we rise with Christ. And as when he rose from the grave, he no longer had corruptible and mortal flesh, in just that way when we come up from the font, no longer do we have the original sins and foul flesh, for as it is easy to poke one's head in the water and lift it out again, just as easy is it before God to bury and consume entirely the old man and to reveal the new (p. 162).

The iambic poem prefacing Ephrem's translation can be closely rendered as:

> From the womb in essence a sun, first among the celestial luminaries,
> whom, without a mother, as his son the Father re-embodied,
> the self-same, without a father, as a body from her own blood
> a woman of Adam's line bears in Bethlehem,
> an infant new-born, God of the centuries.

A question arising from the practice of assigning a French translation to anything in the Georgian that lacks a corresponding term in the Greek is: for whom are these volumes designed? Presumably not solely for specialists of Old Georgian, who would not need the French crib. If, therefore, some potential readers are assumed to be likely to need assistance with the Georgian, why not take the opportunity to guide such readers more comprehensively? For example, on the very first pages of text one finds in the Athos polycephalon the three verb-forms: *ugalobdit*, *miegebvodet*, *aghamaghlebdit*; to these the T'bet polycephalon offers the correspondences: *ugalobdet*, *mivegebvodit*, *amaghldebodet* – why the differences? True, a note appended to the third verb of the Athos version has the French translation "[Habitants du ce monde,] élevez le [Christ]", but what is there to prevent a reader who is struggling with the Georgian wondering whether such differences between the mss are anything other than misprints? Why not point out all three verbs of the Athos ms are Ist series imperatives ("Sing praises to him!", "Go forth to meet him!", "Exalt him!"), whilst in the T'bet ms we have three Present Subjunctive forms (which also did service for the not then developed Future Indicative) to give: "You should/shall sing praises to him!", "We [sic] should/shall/Let's go forth to meet him!", "You should/shall be [sic] exalted!". Such an approach would alter the character of the series somewhat but widen its appeal.

There are a number of inconsistencies between texts that might well have been explained (?corrected) somewhere. Right at the start we have the toponymical appendage to Gregory's name *Nazianzenus* rendered in the polycephala as *andziandzoreveli[sa[y]]*, and yet prefacing the iambic verse

it appears as *nandzinandzoreveli[say]* – why? On p. 3 the T'bet polycephalon has the 2nd person plural Aorist Subjunctive *ikmnet* "that you become", and yet on pp. 52–53 both the Euthymian and Ephremian translations render this as *ikmnnet*, the interesting point being that the extra pluralising *-n-* is selected for the Euthymian version against the form *ikmnet* attested in mss EF, but this latter form re-appears when on p. 125 the original text is quoted in the Commentary – so, if the extra nasal is deemed correct on p. 52, why not insert it in the polycephalon and Commentary? On pp. 4–5 the polycephala offer *eri . . . ixiles* "the people . . . they beheld" (with 3rd person plural verb because of the semantics of the grammatically singular subject, which is standing in the Nominative rather than Ergative case because of case-attraction to an intervening relative-pronoun, itself in the Nominative) – this is a quotation from Isaiah 9.2 (not 9.1 as given in the Notes). Now, the Ephremian text (p. 53) is the same, whilst the Euthymian has the 2nd person plural IInd series imperative *ixilet*, presumably a misprint as Isaiah has the 3rd person Aorist indicative (singular *ixila* in Robert Blake's Patrologia Orientalis edition), and again the form *ixilen* in the Commentary (p. 127) looks like another misprint, for as it stands it would mean "(do thou) see *them*!". Similarly, the 2nd person plural Ist series imperative *ixarebdit* "Rejoice!" of the Athos polycephalon (p. 2) must be a misprint for the 3rd person from *ixarebdin* "Let [the heavens] rejoice!" to make it conform to the T'bet version and to the original from Psalm 96.11.

It is good to see the series progressing at a regular rate, and one hopes that this output can be maintained.

George Hewitt

The Circassians. A handbook. By Amjad Jaimoukha. pp. 384. Richmond, Curzon, 2001.

"I was now about commencing a tour under disadvantages such as I had never before experienced, having always made it a rule to acquaint myself with the history, customs, manners, and, above all, the language, of whatever country I might be about to visit: but here was a country and a people of whom the civilized world know little; and a language, according to the opinion of linguists, without the slightest affinity to any other on the face of the earth" – thus did English traveller Edmund Spencer journey to Circassia in 1836, and, as regards knowledge amongst the international community of the Circassians, who universally designate themselves "Adyghe", little has changed. It is the purpose of Curzon's series of handbooks to make up for this deplorable gap in the wider world's knowledge about the various peoples of the Caucasus, only two of whom in all probability (namely, the Georgians and the Chechens) are likely to possess names recognisable to non-specialists of the region. Of course, it would be wrong to expect a comprehensive ethnographic study to be produced for this series, as one mischievous (and anonymous) reviewer of the only other volume so far to have been published complained, for such a monograph would scarcely fit the handbook template, and Jaimoukha, a native Circassian (Kabardian) from Jordan, has admirably met the challenge he set himself in taking full responsibility for the entire volume. In only one regard would Spencer be left floundering, for, while readers are informed, as linguists have long recognised, that Circassian belongs to the same small North West Caucasian family as Abkhaz(-Abaza) and the now (but not in Spencer's day) extinct Ubykh as well as being directed to works describing Circassian's dialects, no actual description of the language's complex morphological structure is incorporated, which is a real pity, for it would have enhanced the book even more. The rich collection of proverbs (pp. 296–309), written in the author's preferred Roman transcription, in addition to the insight into folk-wisdom always provided by this genre, also gives readers a glimpse (as do some literary citations in Chapter 14) of the wide use of fricatives characterising this most sublimely sonorous of languages, but the structures behind these mysterious sequences remain tantalisingly opaque.

The book is particularly informative on the Circassian diaspora, of which the author is, of course, a member. The world's majority Circassian (and Abkhazian) population has not been found in the Caucasian homeland but across former Ottoman lands (notably Turkey) ever since the mass migrations that occurred following Russia's final conquest of the North Caucasus in 1864, when all the Ubykhs too crossed the Black Sea. It was the noble defence of their ancestral territories that brought the North West Caucasian peoples to the attention of many in Western Europe when visited by such sympathetic visitors as Spencer and his fellow-countrymen, David Urquhart (who designed the Circassian national flag), James Bell and John Longworth in the 1830s – both the latter left fine and moving descriptions of their sojourns. The Kabardians are usually deemed to have offered only half-hearted support in the Great Caucasian War against Russia, but Jaimoukha argues that this most easterly of the Circassian tribes had made their own spirited defence of North Caucasian freedom during earlier years (pp. 60–3). The Circassians' specific contribution to history beyond the Caucasus was their long and distinguished role as the Mamluks in mediaeval Egypt, as described on pp. 55–7.

The book is so wide-ranging and accompanied (as is the author's website) by such a detailed bibliography that lacunae are hard to spot. Min-Kutas Azamatova's illustrative album of embroidery-patterns (*Adyghe Folk-Ornamentation*, Maykop, 1960, in Russian) might have been mentioned on p. 217. Chapter 12, devoted to music and dance, mentions in passing that both Balakirev's fiendishly demanding "Islamey" for solo piano and Prokofiev's 2nd String Quartet are based on Kabardian themes; again reference might have been made to the 3-volume *Folk-Songs and Instrumental Tunes of the Adyghes*" (Moscow, 1980–86, in Russian), collected by E. V. Gippius.

The problem of the needlessly divergent (and clumsy) Cyrillic-based scripts that have been employed since the late 1930s for West Circassian (based on the Temirgoi dialect) in Adyghea (capital Maykop) and East Circassian (based on Kabardian) in Kabardino-Balkaria (capital Nal'chik) is addressed, with allusion to the suggestions that have been made for (re-)romanising. Jaimoukha's own roman system is presented in his Appendix 2 – the first three phonetic representations on p. 322 should read as follows: [ḫ], [ɬ'], [ɬ]; I would then represent the phonemes /q'/ and q'w/ respectively as: [qˣ'], [qˣʷ'], while the two fricatives labelled as "uvular" I would style "alveolo-palatal". As Jaimoukha observes, the Caucasian homeland's decision on the script is crucial, since that is where Circassian still has a role (albeit diminishing under increasing pressure from Russian) for literary and educational purposes; only in the two Circassian villages in Israel is systematic education provided for diaspora-Circassians in Circassian, for which the Cyrillic script is employed. Thus, most of the diaspora-Circassians, amongst whom knowledge of the language is fast disappearing, are not literate in the mother-tongue and would find Cyrillic a disincentive to ameliorate the situation. And so, as Jaimoukha advocates, a combination of Roman script and use of the Internet might provide the language with an eleventh-hour lifeline. Sadly, the powers-that-be in both Maykop and Nal'chik are not sympathetic to abandoning Cyrillic, and the same (in my view, backward) attitude prevails amongst the related Abkhazians in Sukhum. Interestingly, the French visitor Taitbout de Marigny proposed devising a roman script in the 1820s, but as early as in the 1826 Odessa edition of his work a meddling Russian editorial hand had interpolated the question: "Why not adopt the Russian alphabet?" (see p. 72 of the 1837 English edition)!

Some comments are necessary on the historical sections, even though they do not directly affect what is written about Circassia. On p. 63 a decision of 1830 is ascribed to Gen. Ermolov, but, as military command in the Caucasus was removed from him in March 1827, this cannot be so. When the Bagratid king Bagrat III ascended the throne in 975 he inherited the hitherto independent kingdoms of Kartli/Iberia (= Eastern Georgia) and Abkhazia (in Georgian "apxazeti"), which had existed for some two centuries and which incorporated Abkhazia *sensu stricto* (viz. "Apsny" in Abkhaz) as well as the Kartvelian speaking provinces west of the Likhi Mountains – in other words, the whole of what today is thought of as Western Georgia (+ Apsny) was known at the time, and

indeed for a long while to come, as Abkhazia/apxazeti – there was no question of Georgian subjugation of Abkhazia (cf. p. 48), either as this toponym is understood today (viz. Apsny) or in its contemporary wider sense (viz. W. Georgia + Apsny). And prior to the establishment of Abkhazian Kingdom in the *late* (cf. p. 313) eighth century one can hardly speak of any Georgian presence in, or influence on, Abkhazia (though Abkhaz-Laz-Mingrelian relations are another matter). Thus, how is one to interpret this statement on p. 46: "In 455 AD, the Georgian chronicles state that Vakhtang Gourgaslan conquered Abkhazia"? The text in question (see p. 157 of S. Q'aukhchishvili's 1955 edition) reads thus: "For three years Vakht'ang ravaged all the strongholds of Abkhazia as far as Tsikhe-Godzhi." Now, Tsikhe-Godzhi is the ancient Archaeopolis, situated on the left bank of the R. T'ekhuri, and was the sixth–eighth century capital of Egrisi (in modern-day Mingrelia) – in other words, Vakht'ang, moving from east to west, never reached today's Abkhazia (= Apsny). As the late Yuri Voronov noted in his posthumously published *Ancient Apsilia* (Sukhum, 1998, in Russian, pp. 75–6), we have here a case of an XIth century author carelessly assigning a contemporary sense of the term "Abkhazia/apxazeti" to events in the fifth century, when "Abkhazia/apxazeti" could only have signified Apsny (or Apsilia). In view of the exaggerated claims Georgians are given to make for the Georgian role in Abkhazian history, it is important to clear up any misunderstanding that might flow from this reference in Curzon's new publication, which, on the whole, I am more than happy to recommend.

George Hewitt

Main Corrigenda

p. 48 l.15up: as the powerful state was also known; p. 62: in 1801 only today's eastern Georgia came under Russian "protection"; p. 66 l.14up & p. 83 l.2: reached/was building up to a climax; p. 69: *Divide et impera!* was a Roman principle; p. 75 l.3: Rutskoy; p. 125 l.17up: branded with a particular emblem; p. 152 l.12up: woo; p. 250 l.12up: Russian-Kabardian; p. 302 l.5: its; p. 315: 1810 was the date that Russia annexed Abkhazia; in 1864 the Circassians plus the Ubykhs and Abkhazians were finally defeated by Russia; p. 339 under Catford: Ergativity in Caucasian languages.

A monetary history of the Ottoman Empire. By Şevket Pamuk (Cambridge Studies in Islamic Civilization). pp. xxvi, 276. Cambridge, Cambridge University Press, 2000.

Şevket Pamuk's latest book investigates the evolution of the Ottoman monetary system from an empire-wide perspective and as an integral part of the world economy, from its beginnings to the First World War. The study adopts a "big-picture" and long-term approach emphasising the linkages between the history of the Eastern Mediterranean and those of Europe and South Asia over a period of six centuries. At the same time it highlights the complexity and heterogeneity of the monetary arrangements in the different regions of the Ottoman Empire, dependent both on local development and global economic forces. The analysis of the monetary history of the empire, located at the cross roads of intercontinental trade, from a global perspective is also grounded in the author's understanding of the strong two-way interaction between long-distance trade, specie flows and money.

The introductory chapter offers a chronological periodisation of Ottoman monetary history. Ottoman coinage is defined to be the carrier and follower of the great monetary traditions of the Old World and especially of the Mediterranean area, money being considered in its four main functions – as a means of exchange, a means of payment, a means of account and as a store of value. As research framework of the study, the problems of the nature of the Ottoman state and its relations with different social groups, of the economic functions and priorities of the central bureaucracy, as defined by Mehmed Genç, and of state interventionism in the economy are specified.

The currency and monetary practices of the emerging Ottoman state are analysed against the

background of political, economic and monetary conditions in the Balkans and Anatolia in the thirteenth and fourteenth centuries and in the context of trade, payments and specie flows from southern Europe to West Asia and further to the East. Reminding us that the issuing of coinage has been considered an important symbol of sovereignty in the Mediterranean basin ever since Antiquity and that the Islamic states have also regarded *sikke* and *hutbe* (coinage and prayer) as the two symbols of sovereignty, the author raises the question why 1326 (747 AH), the year when the Ottomans began to strike coins in the name of Orhan Bey, has not been accepted in historiography as the date of the foundation of the Ottoman state. The location and operation of Ottoman silver mines and mints and the different systems of their administration in the fifteenth century are then examined.

In chapter 3 a valuable contribution is made with the complex explanation of monetary practices and in particular of the debasements of Mehmed II, considered within the political economy framework of their costs and benefits to the state and to various social groups affected by them. Pamuk argues that the main motives behind regular debasements were fiscal. They were used to build a powerful treasury, to finance military campaigns and to expand the role of the central government. These monetary practices were part of the interventionist measures of the central government in fiscal and economic affairs, specific to this period, which also contributed to the severe shortages of specie known as the ''Silver Famine''.

The emergence of the Ottoman monetary system is subsequently examined, with an emphasis on the different functions of the golden, silver and copper coins and detailed information about their standards and exchange rates. The book provides us with evidence of the increasing use of money and of the development, notwithstanding Islamic prohibitions against interest and usury and the absence of formal banking institutions, of intensive networks of credit in Ottoman society. Pamuk underlines the two-tiered pragmatic approach to money and currencies of the central government during the sixteenth and seventeenth centuries in the Ottoman empire. While unifying the gold coinage, the ultimate symbol of sovereignty, at the existing international standards, the creation of multiple silver currency zones was allowed, in order mainly to avoid economic disruption and possible popular unrest in the new provinces.

Pamuk's study makes an important contribution to the discussion of the impact of the ''Price Revolution'' of the sixteenth century on Ottoman economy and finances. In the large historiographical context of the enduring debate on the causes and consequences of this revolution in Europe, the author draws attention to long-term trends in population growth, urbanisation and commercialisation in Europe, Asia and the Near East and their causal connection to the velocity of circulation of money as an alternative to explanations based on the Quantity-Theory framework and the monetarist approach. On the basis of new research on price changes in Istanbul from the mid-fifteenth to the end of the nineteenth centuries, Pamuk challenges Ömer Lütfi Barkan's interpretation of price increases of the sixteenth century and argues that silver inflation accounted for a smaller part and Ottoman debasements accounted for a larger part in these increases. He also disagrees with their interpretation as a turning point and a leading cause for an irreversible decline in the Ottoman economy and points out more complex reasons for the difficulties experienced, such as fiscal problems, changing techniques of warfare, and changes in industrial organisation in Europe. The disappearance of the *akçe* from circulation in the mid-seventeenth century and the invasion of the Ottoman markets by debased European coinage in the absence of domestic currency are then examined. Pamuk's findings about the monetary difficulties of the seventeenth century and their causes allow him to define these times as a period of monetary instability and even disintegration which had an impact on credit, trade and production.

In the subsequent chapters Pamuk demonstrates how, with the establishment of a new monetary unit of account and means of exchange – the Ottoman *kuruş* – and the centralisation of mint activity in the core regions of the empire, from the eighteenth century until the 1780s, became, in contrast to

the previous, a period of recovery for the Ottoman monetary system. This development was closely interconnected with economic expansion and fiscal stability. On the basis of numismatic and other sources, the author traces the picture of an important trend towards the strengthening of the monetary ties between the centre and the periphery of the empire in the eighteenth century.

The final chapter discusses the attempts at financial centralisation that took place during the reign of Mahmud II and the use of various forms of internal borrowing in order to deal with the large state budget deficits, arising (according to Pamuk) more from wars than from the cost of reforms. The ''Great Debasement'' in which the Ottoman *kuruş* lost, from 1808 to 1844, 83% of its silver content, and the government's attitude towards the opposition of different urban groups, are analysed. The book provides us with a concise overview of the establishment of a more stable monetary system in the second half of the nineteenth century through the abandonment of debasements and the adoption of bimetalism and stable coinage under European pressure, during western-style reforms, integration of the Ottoman empire into the world economy and important territorial contractions of the empire. The use of external borrowing as a permanent method to financing deficits, the issuing of paper money, the development of commercial banking and the limping gold standards are finally considered as other important characteristics of Ottoman monetary history in the period preceding the First World War.

The conclusion summarises some of the most important findings concerning the increasing availability and use of money in the Ottoman empire, the selective, flexible and pragmatic state interventionism in economic and monetary affairs, the motives and consequences of debasements and the long-term correlation between monetary conditions and economic trends and cycles in Ottoman history.

The volume is provided with an index, a rich list of references, a highly representative and illustrative selection of photographs of Ottoman coins and paper money and three (often referred to in the main narrative) appendices with excerpts from Ottoman laws on money, taxation, mints and mines from the fifteenth and sixteenth centuries, price indices and price trends in Istanbul from 1469 to 1914 and some basic economic and monetary magnitudes from the same period. Its numerous graphs and tables are very useful. The book's complex approach to the problems under study together with its comprehensible and remarkably readable style makes it accessible to a wide audience of historians, economists and other social scientists. With its numerous new data and innovative interpretations, as well as the construction of time series for the standards of the different Ottoman currencies, this excellent study is indispensable to all future research in Ottoman economic history.

Svetla Ianeva

Islamic Paper, a Study of the Ancient Craft. By Helen Loveday. pp. v, 90. London, The Don Baker Memorial Fund. Distributed by Archetype Publications, 2001.

This book was written in memory of Don Baker (1932–1994). Based in London, he exercised his skills as a professional conservator of Islamic manuscripts both there and in the Middle East.

It was he who initiated this study of Islamic paper based on its physical characteristics. It was fortunate that after his untimely death one of his students, who was to take her Master's degree at the Camberwell School of Art, Helen Loveday, was there to continue his work; as she so aptly observes ''The greatest tribute to any scholar is that his research should be continued''. Propelled by ''my incorrigible love of Islamic paper'', as she puts it, Helen Loveday has produced a worthy scholarly commemoration of his work. To this task she brought the aid of a computer database and a methodology which she evolved and refined as her research progressed. Her investigation

embraced analysis of the paper of 1237 dated MSS ''selected at random from public and private collections and libraries'' in Britain, but also including some in the Khalidi Library in Jerusalem. A considerable number of the manuscripts she examined in London was in the Library of the Royal Asiatic Society.

As the title would imply, the geographical scope of the study spreads from India to the Iberian Peninsula, but in practice the research focuses mainly on Islamic manuscripts from Persia, and from the region of Syria, Palestine and Egypt. These geographical terms of course should not be construed strictly as they would be today. Persia provided 59%, and Syria, Palestine and Egypt 41% of the examples studied. Her investigations in fact distinguish and compare paper from manuscripts produced in the two traditions of papermaking techniques which are characteristic of these two regions.

The book is divided into two parts. Part I, (which she describes as ''neither extensive nor exhaustive'') traces the history of the introduction and spread of papermaking in the Islamic world. 'A History of Islamic Papermaking' discusses early writing supports (papyrus, parchment) and offers answers to the question ''Why was the expansion of papermaking in the Islamic world so rapid, so total, and so far reaching?'' Subsequently it moves on to discuss the reasons for ''The Decline of Papermaking in the Islamic World''. The answer of course was competition from Europe. ''The export trade to the east grew to such an extent that from the 17th century onwards, the use of European paper for manuscript production was widespread''. But as the expansion of Islamic papermaking was ''total'', so was the extinction of hand papermaking unexpectedly ''total''. When one thinks of the widespread survival of the craft of papermaking by hand in Eastern and Southern Asia, it is puzzling that in the Islamic Middle East and in the Maghrib all trace of handmade paper mills seems to have disappeared. Indeed it is almost unbelievable. One has to deduce the form of the mould from the surviving sheets of paper. ''Although no contemporary examples of the papermaker's mould are known to exist,'' she writes, ''variation and progression in construction can be traced through the impressions left in the sheet by its constituent materials.''

The third chapter, ''The Nature of Paper and its Production'', is a broad-ranging and thorough description informed by Helen Loveday's professional knowledge of papermaking, and her ''active involvement in the profession of paper conservation''. On p. 47 she cites Carter for the fact that ''the first book to be printed in the Islamic world was not until 1825, when the first press was established in Cairo''. This can be refined somewhat. An authoritative discussion of the subject by Dr. Geoffrey Roper was published as ''Arabic Printing, its History and Significance'' in *Ur* 1–1982, pp. 23–30. This refers to non-Muslim works which were printed in the Islamic world, from 1729 onwards; Roper also records that the first book was printed at the Bulaq Press, in Egypt, in 1822.

Part II of the book ''is devoted to the systematic analysis of Islamic paper, and focuses on the examination of the nature and characteristics of Islamic paper as gleaned through the analysis of dated samples according to stated criteria.'' Accordingly, in her fourth chapter she narrows her focus to an analysis of the qualitative characteristics of Islamic Paper. Again it is informed by her paper conservator's training.

She opens her fifth chapter, ''Protocol for Paper Classification'', with these significant words: ''As mentioned above, the aim of this research is to identify the characteristics of a sheet of paper that are significant for its classification according to its origin and date of manufacture, and to plot a chronology of these characteristics in order to evaluate trends, patterns and changes in the craft of papermaking in Syria, Egypt and Persia, from the 12th century AD until the beginning of the 19th century''. Such a systematic study is *exactly* in line with what is needed for the field of Asian manuscripts.

Not only does she describe in detail the methods she uses, at the same time she sets out a rigorous methodology which can be used for future research. A copy of her chronologically tabulated findings (pp 81–7) would be a useful reference tool for any one wishing to identify papers in

manuscripts from the Middle East. Indeed with some rearrangement, namely a kind of flow chart utilising the discernable features as guides to the possible identity of the paper, we should be a step nearer a global guide to the identification of Asian papers. In other words the discernable physical features would be set out in systematic order, and beside each would be listed the papers in which they could be expected to occur. Thus we could envisage practical hints like ''*Narrow chain line intervals*: Persian paper, 15th, 16th centuries; Chinese paper (to modern times)'', to take an over-simplified example.

She does enter a caveat ''the information in the table must be viewed as corroborative evidence, rather than as the primary means of classification''. And she does recognise the danger of ''trying to reduce to order that which cannot be ordered''. Manuscripts are like that, but this timely caution does not rule out the likelihood that more sophisticated means of analysis will eventually enable us to develop refined techniques for identification and information storage which we can use with more confidence.

There is no index. Her bibliography comprehends the main sources available in English, and some French and German sources. A most useful addition, (perhaps it came too late to be included) would be Marie-Thérèse Le Léannec-Bavavéas, *Les Papiers non Filigranés médiévaux de la Perse à l'Espagne*. Paris: CNRS éditions, 1998.

So far as this reviewer is aware, remarkably enough, no such global survey of Islamic paper has been published hitherto in English. But one should mention here another book with the same title, but different subtitle, written about ten years ago by Mrs. Aliza Thomas, a papermaker living in The Hague. This book, *Islamic Paper, a collection of historical facts*, unfortunately has not yet been published, and I have seen it only in draft form. It is different from Helen Loveday's book. It cites copiously from Arabic and Persian sources, and reproduces the original text in Arabic or Persian, which would be of great interest to many scholars in this Society. Aliza Thomas' book does not cover what is perhaps the most valuable aspect of Helen Loveday's study, the detailed analysis of the papers of a corpus of manuscripts. The two books are in fact to a great extent complementary.

The book under review is attractively produced. There are helpful pictures. Where these refer to characteristics which can be seen by transmitted light (e.g. Figs. 11, 12, 12a), perhaps betaradiographic illustrations would have been clearer.

Summing up, this book has two very valuable features. It provides for the first time in English a concise and scholarly study of papermaking in the Islamic world derived largely from published sources in English, and informed by the writer's professional knowledge of papermaking. Secondly it embodies the fruits of meticulous research into the papers of a corpus of Persian and Arabic manuscripts which provide the basis for a methodology for the study, classification and identification of the papers of manuscripts from other Asian cultures.

Russell Jones

(This is a slightly adapted version of a review published in *The Quarterly* of the British Association of Paper Historians, July 2001.)

Arab Women in the Middle Ages. Private Lives and Public Roles. By Shirley Guthrie. pp. 268. London, Saqi Books, 2001.

This study presents a useful survey on Arab (mostly Muslim) women's lives in past centuries, aimed at a general audience. It covers a wide range of topics from marriage and domestic arrangements to health issues, food, dress, and women's appearance in public life. A final chapter is devoted to women considered to be at the margins of ''respectable'' Muslim society, such as those who prepared corpses

for burial, prostitutes, lesbians etc. It is somewhat surprising to find a brief discussion of upper class women from religious minorities in this context (p. 210), thus implying an identity of ''Arab'' and ''Muslim'', but unfortunately this is not the only oddity of the book.

While the book will be informative for those readers who have little prior knowledge of the Middle East, its claim ''to present an alternative vision of Arab women to counter western perceptions distorted by the lens of Orientalism and historical experience'' (p. 7) seems overly ambitious. Guthrie rarely engages actively with western perceptions of Middle Eastern women, a well-trodden subject in its own right. Where she does so, the argument hardly surpasses the crudest stereotypes (i.e. p. 40). In addition, her presentation is rather untainted by the consideration of any of the theoretical approaches which have contributed to the increasing sophistication of gender and women's history over the past decades.

Furthermore, some of the most convincing studies of Middle Eastern women's lives, and those showing the greatest sensitivity to historical change and variation, have been those engaging in detail with specific historical experiences, such as the works by Judith Tucker, Beth Baron and others. Thus, Guthrie's claim of undefined historical experiences tainting perceptions would at least warrant further discussion of, for example, the imperial context. Unfortunately, historical specificity does not seem the author's strongest point in her own discussion, either: we do not learn what constitutes ''Middle Ages'' for Guthrie. If one was to go by the evidence used in her book, this period would range from the pre-Islamic period of ''ignorance'' (*jahiliyya*) – or even the Old Testament – to the nineteenth century (p. 44f.). If anything, this would seem to confirm the image of an unchanging, rather static Orient, and Guthrie's occasional references to variations over time and space do little to systematically alleviate this impression. While it is admittedly difficult to balance the specific while attempting to draw a wider picture, statements such as ''Women took milk to sell in the market-place'' can hardly be generalized. The subsequent speculation that their visits to the cities might have afforded them the chance to acquire ''a few personal knick-knacks from the *suq*'' (both p. 37) quite obviously complies with wider gender stereotypes, without adding any concrete knowledge or insight. Unfortunately, this is a quite common feature of the book, and while one might feel a certain sympathy for the assumption of international sisterly preoccupations such as ''It is arguable that most women would take an interest in fashion'' (p. 213), they remain stereotypes nevertheless.

Guthrie draws on existing scholarship as well as on some (translated) Arabic sources for her presentation. There is nothing wrong with that per se, except that her introductory stress on the importance of the often-neglected Arabic language and sources (p. 8) had raised slightly higher expectations. Thus, many of the interesting legal texts pertaining to women's roles or issuing judgements on women's behaviour have not been translated, and could not be used. It is very regrettable that Guthrie, whose real field of expertise is manuscript illustrations, does not use that source more extensively and was not able to reproduce some of those illustrations to which she refers. There are many other issues which could be raised both with regard to content and methodology, but they do not, in the end, change the general impression that this is a rather hastily compiled sequel to Guthrie's thorough *Arab Social Life in the Middle Ages*.

Ulrike Freitag

The Swahili. The Social Landscape of a Mercantile Society. By Mark Horton and John Middleton. pp. 282. illus. 21, maps 11, index, bibliography, tables 3. Oxford, Blackwells, 2001.

Our Fellow, John Middleton, is an anthropologist of at least half a century of experience in eastern Africa; Mark Horton an archaeologist with some twenty years experience, chiefly in the Lamu archipelago, in Pemba and Zanzibar. His work has been revolutionary, pushing back the threshold of

our detailed knowledge from the tenth century to the eighth and even earlier. It is primary evidence for the spread of Islam, from praying areas demarcated by reeds to mud-and-wattle mosques whose postholes survive, to stone mosques such as al-Masʿudi, would have visited on Pemba Island in 916, and grander stone mosques, one large enough for some 600 worshippers, superimposed within extensions of the same sacred space.

The context here is the coast known as Swahili by the fourteenth century, in Arabic as Zanj, the people as Zunuj. The name Swahili is that of their language, whose grammar and syntax are wholly Bantu. In the colonial period some writers treated it wrongly as a kind of Arabic Créole, in that it absorbed Arabic words, as well as words from Hindi and Portuguese, and later from English, transmuting them with their own agglutinative grammar. The theme of this work revolves round a people who inhabit a long thin strip of the eastern African coast, roughly from Mogadishu in Somalia to Sofala in Mozambique. They were already a mercantile community some 2 000 years ago, before they accepted Islam and literacy. As brokers with neighbouring African peoples they did not travel inland; as importers their commerce was carried by Arab seafarers. They rarely ventured themselves. Peoples such as these live on all the shores of the Indian Ocean, and beyond as far as the Timor Sea. Frontiersmen, they all performed a specialist function, until it was replaced by the steamship and modern cities, such as Mombasa and Dar es Salaam.

The first chapter describes their physical environment. The periodicity of the monsoons provided regular means of communication, as brokers they were dependent not only on African neighbours, but on visiting merchants: their copals and millets reached Central Asia even in the Bronze Age, as described in the second chapter. An enigmatic reference in Agatharcides of Cnidus, *On the Erythraean Sea* (ed. S.M. Burstein, Hakluyt Society, 1989, pp. 171–2), not mentioned by our authors, relating to Pemba, is echoed by a find of a contemporary coin of Ptolemy Soter (110–108 B.C.) near Dar es Salaam in the 1890s. Only little by little does the evidence build up.

As the authors say, the origins of East African Islam is "one of the most difficult and controversial issues." A tale told by Buzurg ibn Shahriyar in the tenth century refers it to an undated "negro" king after the foundation of Baghdad. Al-Jāḥiẓ (nick-named "Goggle-Eyes"), who may have been born in Pemba, refers to Muslim rulers there and in Zanzibar. Horton's mosque excavations bridge the gap until Ibn Battuta found all the coast Islamised by 1331.

Nineteenth-century traditions ascribe this to colonisation by the "Shirazi". Of more than 200 inscriptions so far recorded (*JRAS*, 1973) only one refers to Shiraz, the place of origin from which the founder of Kilwa sailed (sic) in the tenth century; a successor displayed Shirazi as a *nisba* on his signet ring in 1777. Early twentieth-century writers even refer to "Shirazi" architecture, regardless of the fact that the mosques bear no resemblance to any in Iran. This is hardly a foundation on which to build a "colonisation". Horton ascribes many mosques to Ibadi, and this may well be so.

The fourth chapter, on Indian Ocean trade, shows clearly that principal wealth derived initially from ivory exported to India and China, from gold through Sofala, and of rock crystal to Egypt, apart from the other local commodities. The fifth chapter is a detailed description of the trading system of Mozambique, for which so far we have somewhat scarce evidence. A rounder picture would have been obtained from the *Ethiopia Oriental* of João dos Santos O.P., 1609, where much detail is to be found. His survey dates from the 1590s.

Chapters six and seven discuss Swahili society. Key words are *utamaduni* (urbanity) as opposed to *ushenzi* (upcountry barbarity). Another is *ustaarabu* (civilisation or conduct befitting the *gravitas* of an Arab). Connected ideas run through the whole of society, from the mosque to the lay-out of houses. It is continued in chapter seven, Social Networks.

Chapter eight, Governance, describes a pattern which begins with Ibn Battuta, who likens the court etiquette of Mogadishu to that of Yemen. A pattern, with some variations, could have been constructed through the court of Kilwa up to the late-eighteenth century, where the system of the

sixteenth-century Arabic and Portuguese *Histories* emerges again in a description by a French slave trader, Morice (cf. Freeman-Grenville *The French at Kilwa Island*, 1965). The sultan has a court with *amirs* and *wazirs*, a *qadi*, a treasurer, a *muhtasib*, state trumpeters, fife-players, and in the eighteenth century, two executioners. The *siwa*, a side-blown ivory or metal horn, was the perquisite of royalty throughout the coast. The *siwa* of Pate makes a splendid illustration on the jacket, being blown to mark the beginning of the third millenium A.D.

The ninth chapter, Knowledge, Purity and Power, is concerned with religion, and social and moral order, of good and evil spirits, of Sufi fraternities. There is too much detail to summarise it. It is notable, however, that nothing is said of the characteristic "pillar tombs", columns rising from tombs, chiefly on the Tanzanian coast, and of unmistakeably phallic appearance in many cases. At Mtitimira the mihrab of a ruined mosque is surmounted by what is a phallus with an unmistakeable *glans penis*, hardly placed in so prominent a position undeliberately.

The last chapter, Constructing the Mercantile Landscape, draws together the preceding chapters: of a society based upon a minority of merchants, who nevertheless had a *oikoumene* far beyond their shores, but never coalescing under a single government before the establishment of the Zanzibar Sultanate. The patrician merchants never became landed aristocrats, but had a consciousness of belonging to a world-wide religion and a literacy which marked them as different from their immediate neighbours, a difference now steadily disappearing.

In the different context of a debate on Spanish nationality in the Cortes, when the nineteenth-century Spanish Prime Minister Cánovas de Castillo was heckled about the meaning of the word Spaniard, he retorted: "I tell you, a Spaniard is a person who is different from everyone else".

There are a not unremarkable number of what may well be printing errors resulting from linguistic misunderstanding; for it is noteworthy that the book was printed in Pondichéry. So, p. 16, it is Qanbalu, not Qunbalu; p. 32, he is Ptolemy Soter, not Sotar; Map 2.1, the place named Zing is correctly Zingion; p. 65, there is no language *Zahjiyya*, but rather *Zanjiyya*; p. 66, the place is not Churaka, but Chwaka; p. 70, the name is not Harum, but Harun; p. 70. *umma* does not mean sanctity, but, in general, community; p. 98, the river is the Ruvuma, not Ruvuna; p. 107. *waungwana*, not *wangwana*; p. 122, Comorian, not Comoran; p. 121, Comoros, not Comores (French); *fumboni* is a locative, made by adding *-ni*; *fumbo*, in this context, would mean a private enclosure; p. 122, translated *Haram al-Khedima*: *haram*, rather is a sacred enclosure, but *khedima* can hardly mean anything unless a female slave; one may suggest *qadima*, old, ancient; p. 144, *kike* is female, not *kiike*: p. 162, Bernardino, not Bernadino; p. 168, *mtenzi*, not given in the dictionaries of Sacleux or Johnson, but as *mtensi* by Krapf, meaning one who makes himself important by giving receptions; p. 172, *nzumari*, for *zomari* or *zumari*, which Johnson renders as "a kind of pipe, flageolet, clarinet, of wood with a harsh, piercing tone, rather like a bagpipe". Plate 9.1, the reading is erroneous, and copied by Kirkman from an error of Stigand, *The Land of Zinj*, 1913; in fact, Martin and your reviewer, (*JRAS*, 1973) separately read it as reciting the names of the four Orthodox Caliphs; p. 186, confuses *sayyids* with *sharifs*; p. 212, n. 27, country mainsions, not county; p. 218, n. 9, *Zunuz*, not *Zanuz*; p. 230, peninsula, not peninsular, p. 238, n. 36, Bagamoyo, not Bagamogo; n. 49, read Abubakari, not Abubarkari; p. 204, *oikoumene*, not *oikumene*.

There is a splendid and comprehensive bibliography, a tribute to the authors' wide reading. Priced at £40, it is welcome news that a paperback edition, which will be useful to students, is on its way. For, all in all, in spite of its unusual and original approach, it can serve as a useful introduction to what is a very complex subject. The actual text is little more than two hundred pages, with thirty-two pages of footnotes. There are many more ancient sites, large and small, as yet unexplored. There is a Swahili *diaspora*, the most notable being the Sidi of western India. There are pockets of Swahili speakers, on Socotra Island, all along the southern Arabian coast, and up the Gulf, as well as those who have settled in the west. Libraries and archives in Zanzibar are relatively unexplored, and not

least that of Tarim and others in Yemen. As to a list of sites, the last having been made by your reviewer in 1956, was published by H.N. Chittick in his first *Antiquities Report*. There is indeed much left to be done.

G. S. P. Freeman-Greville

Between Bible and Qur'ān. The Children of Israel and the Islamic Self-Image (Studies in Late Antiquity and Early Islam). By Uri Rubin. pp. 318. Princeton, Darwin Press, 1999.

This study focuses on ḥadīth material concerning the evolution of the self-image of Muslims and the perception of their own earliest history, related to Biblical material, not dealing with the usual approach concerning "the influence" of the Bible on the Qur'ān and the Islamic Tradition. This book is coherent with research already presented by the author in *The Eye of the Beholder* (Princeton, 1995) and other volumes of the same collection (SLAEI), in the tradition of M.J. Kister and Hava Lazarus-Yafeh. In the first part (the shortest), notions of Jewish Messianism adopted by Muslim apologetic historiography are analyzed (in particular the conquest of the Holy Land legitimizing the Islamic Presence in al-Shām, but also including Arabia as well). The story of the Israelites, the Children of Israel, and Moses's Torah coexist with the history of the Muslim conquest and Muhammad's religion in such a way that there is indeed a kind of "Judeo-Muslim" approach of the events of the first century of the Islamic area. As the image of the Children of Israel given in this first part is a positive one, the author called them "Virtuous Israelites", in opposition to the much more extensive second part, where they are called "Sinful Israelites". Most of the ḥadīth material of the first part goes back to the Jewish convert Kaʿb al-Aḥbār. In these traditions the Arab conquest is understood as a fulfillment of the true message of the Torah, Bible and Qur'ān sharing the role of predicting a divine eschatological scheme; and in the case of the construction of the Dome of the Rock "Jewish messianic dreams have been employed here to legitimize the erection of an Islamic sanctuary on the site of the Israelite Temple" (p. 20). The very existence of traditions pointing towards a common monotheism and the same messianic goal shared by Jews and Muslims together, constituting a kind of Jewish-Muslim community is in fact not surprising if we consider the earlier Meccan sūras, such as 29, v. 46 («Our God and your God is One . . . »). It seems that there is some ambiguity in the introduction (p. 2), where it is said that "this set of designations (i.e. "people of the Book", "those who were before you" . . .) indicates that the differences between Jews and Christians are insignificant, as both are treated as belonging to the same monotheistic group that preceded the Muslims in world history". This is only true in the earlier Qur'ān, while the terms "Naṣārā" and "Yahūd" are used only in Medina. It is then, in the later Meccan and in the Medinan Sūra's that the Children of Israel are described as violating God's covenant, losing the status of chosen community given to them in the earlier Sūra 44, v. 30–33, becoming in fact the sinners described in the Sūras 7, 2, 5 and in particular Sūra 4, v. 153–161 (the "Qur'ānic Israelites") (p. 59). It is not so surprising then, that some Jewish converts to Islam, like Kaʿb, preferred to see their Islamic history in continuity with the Biblical tradition, proposing traditions presenting a more positive image of the Israelites, in the spirit of the "earlier" Qur'ān. While on the other end, the overwhelming majority of the ḥadīth went on in the spirit of the Qur'ānic verses which were so critical about the "sinful Israelites", rejecting the other traditions as "Isrā'īliyyāt", but nevertheless alluding, just like the Qur'ān, to Biblical traditions. Interesting in Chapter 4 is the very satisfactory suggestion that the term "ḥiṭṭa" (Qur'ān 2, v. 58–9 and 7, v. 161–2), which renders the famous word Shibboleth (Judges 12, 5–6), has to be understood according to the synonymous Hebrew word "ḥiṭṭa", meaning "wheat" (p. 84). It seems that the only explanation for this, is that the Qur'ān knew the Syriac translation of the story, and simply used the Syriac term, from the same root, meaning "wheat"; this hypothesis is likely to

solve Jeffery's problem (''the word is still a puzzle''). The analysis of the theme of the ''sinful Israelites'' and how it was used to show the superiority of the Arabs, or to denounce the conflicts and schisms in the Islamic community, proposing a chronology of the evolution in the Islamic self-image, is a major step forward in the analysis of this complex ḥadīṯ material.

Emilio Platti

In Quest of God and Freedom: The Sufi Response to the Russian Advance in the North Caucasus. By Anna Zelkina. pp. xxiii, 265, London, Hurst and Company, 2000.

Despite its catchy ''journalistic'' title that conjures up a spate of recent publications on the Russo-Chechen wars along the lines of A. Lieven's *Chechnya: Tombstone of Russian Power* (New Haven, 1998), S. Smith's *Allah's Mountains* (London and New York, 1998) and A. Nivat's *Chienne de guerre* (Paris, 2000 and New York, 2001), this book was written by an academic who obtained her Ph.D. degree in Islamic Studies from Oxford University. It appears to be a reworked version of the author's Ph.D thesis defended at Oxford a few years ago. Given the sensational and superficial nature of most recent publications on the current Caucasian conflicts and their historical roots, the appearance of a serious study of the subject based on a large body of historical evidence is to be commended. Whether the study under review meets our expectations and stringent academic standards is a different matter. After providing a concise description of the social, ethnic and economic structures of Northern Caucasus (Part I), the author proceeds to examine the spread of Islam and Muslim institutions in the area in the eighteenth and nineteenth centuries with special reference to Daghestan and Chechnya (Part II). Part III deals with the Russian colonial expansion in the Northern Caucasus in the last decades of the eighteenth century that triggered the anti-Russian uprising lead by Imām Mansour Ushurma (d. 1791). After these preliminaries, the author turns to the principal topic of her study and, in Parts IV–VIII, examines the history of the Muslim resistance to the Russian onslaught in 1830–1859, which was led by three Daghestani *imāms*, Ghāzī Muḥammad, Ḥamza(t) Bek and Shamīl (Shamwīl). Although the book will no doubt be of value to the reader without any prior knowledge of the subject, most experts on the Caucasus will find it to be thoroughly derivative. The author follows closely in the footsteps of Moshe Gammer's fundamental (and much more detailed) study of Shamīl's uprising against the Russian conquest of the Caucasus and its antecedents.[1] At times, the author's narrative appears to be little more than a paraphrase of Gammer's study (e.g., p. 72, cf. Gammer, *op. cit.*, p. 32; pp. 162–4, cf. Gammer, 60–63; p. 183, cf. Gammer, p. 90, p. 131–2, cf. Gammer, pp. 36–7, etc.) to the extent that one begins to wonder whether the author has anything new to say on the subject. Even the arrangement of material is patterned on that of Gammer's seminal book. The author anticipates these concerns by arguing that she wants to correct the ideological biases and racial prejudices inherent in the Russian and Soviet scholarship of the nineteenth century Caucasian war (pp. 1–4). It is true that Gammer's book is based largely, if not exclusively, on Russian sources. Yet, a mere glance at Zelkina's own footnotes also reveals her almost total dependence on the works of Russian and Soviet historians, such as P. Butkov, N. Dubrovin, A. Neverovskiy, L. Lavrov, A. Runovksiy, N. Volkonskiy, N. Smirnov, N. Pokrovskiy, and many others. Even when local sources are available, the author prefers to quote them in Russian translations rather than in the original Arabic. A typical example is Muḥammad Ṭāhir al-Qarākhī's history of Shamīl's imāmate, *Bāriqat al-suyūf al-dāghestāniyya*, that depicts the Caucasian war from the viewpoint of Shamīl's entourage. Although available in the original Arabic, this chronicle of Shamīl's movement is consistently quoted in the translation of its first Russian student, A. Barabanov. The same is true of

[1] Moshe Gammer, *Muslim Resistance to the Tsar* (London, 1994).

Jamāl al-Dīn al-Ghāzī Ghumuqī's Naqshbandī treatise *al-Ādāb al-marḍiyya*, whose Russian translation is quoted on pp. 108–15. Furthermore, the author's persistent mistakes in the transliteration of Arabic words and names (Qa'ba instead of Ka'ba, pp. 83, 91; al-Shawqānī instead of al-Shawkānī, pp. 32, 240, 264; Qubrawī instead of Kubrawī, p. 92; *nazr* instead of *nadhr*, p. 45, etc.; see also Bibliography on pp. 239–40, where almost all Arabic titles are misspelled) seem to indicate that she had probably never seen these names and titles written in Arabic. When she does use a local source (pp. 174–7), she refers to manuscripts in her private possession, which makes it impossible to ascertain the accuracy of her renditions of their contents. Luckily, in one case we have a detailed analysis of one such manuscript by a German scholar that, incidentally, is superior to the author's.[2] Zelkina's main thesis, and the principle *raison d'être* of her book, is to prove that all the resistance movements examined in her book were somehow inspired, motivated and sustained by the "ideology" and "teaching" (pp. 7–8, 50, 122–3, 132, 150, 153, 159, 169, etc.) of the Naqshbandiyya-Mujaddidiyya Sufi brotherhood, as propagated by its Kurdish exponent Shaykh Khālid al-Shahrazūrī al-Baghdādī (d. 1826 in Damascus). Due to its "reformist", "activist" and "militant" character, so goes the argument, the Naqshbandī brotherhood was ideally suited to become the principal vehicle of resistance to western colonial encroachment on the Muslim lands, not just in the Northern Caucasus, but across the Muslim world as a whole. The argument is not new. It can be traced back to the works of A. Bennigsen, Ch. Lemercier-Quelquejay,[3] and more recently, M. Bennigsen-Broxup,[4] who considered "militant" Sufi orders to be natural vehicles of anti-Russian resistance. I have already criticized this explanatory paradigm,[5] which, in turn, rests on the equally problematic notion of the "sober", "*sharī'a*-oriented", "militant" and "reformist" Neo-Sufi movements of the eighteenth and nineteenth centuries,[6] and will not belabour the issue here. Throughout, the author refers to the leaders of the mountaineer resistance primarily as *murshids* of the Naqshbandī order, who used its "reformist" and "activist" ideology to muster popular support and to unite the fiercely independent mountaineer communities around the common cause, that is, anti-Russian resistance. Typical in this respect is the following statement by the author at the beginning of the book, ". . . with the political Naqshbandiyya remaining in the forefront of the events, it was the spiritual-mystical dimension of the brotherhood that provided and maintained the momentum for the far-reaching religious and social reforms in the area" (p. 7). Similar statements run like a red thread across the author's entire narrative (see, e.g. p. 99), yet the process by which the "spiritual-mystical" tenets of the order were translated into its "militant" stance *vis-à-vis* the colonial powers remains a mystery. The author's assumption thus has to be taken for granted by the reader. I, for one, beg to disagree. The fact that, for instance, the fealty sworn to the imāms by their armed followers was called by the Sufi term *tawba* ("repentance") and not by the more common *bay'a* ("oath of allegiance") does this necessarily mean that Sufism was the chief driving force and motivation of the resistance movements in question. If Shamīl's levies were referred to as *murīds*, does this really imply that they were fully-fledged practicing members of the Naqshbandī *ṭarīqa*? The author turns a blind eye to the oft-mentioned division between the practicing dervishes, who abstained from fighting precisely due to their loyalty to

[2] Michael Kemper, "Einige Notizen zur arabischsprachigen Literatur der *ǧihād-Bewegung in Dagestan und Tschetschenien in der erste hälfe des 19. Jahrhundert*" in Anke von Kügelgen, Michael Kemper, Allen Frank (eds.), *Muslim Culture in Russia and Central Asia*. Vol. 2. *Inter-Regional and Inter-Ethnic Relations* (Berlin, 1998), pp. 78–87.

[3] For a concise summary of their views see Alexandre Bennigsen and Chantal Lemercier-Quelquejay, "Brève remarques sur la Naqshbandiyya en Union Soviétique", in Marc Gaborieu, Alexandre Popovic and Thierry Zarcone (eds.), *Naqshbands: Cheminements et situation actuelle d'un ordre mystique musulman*, (Istanbul–Paris, 1990), pp. 441–6.

[4] M. Bennigsen-Broxup (ed.), *The North Caucasus Barrier: The Russian Advance Towards the Muslim World* (London, 1992).

[5] See my *Islamic Mysticism: A Short History* (Leiden, 2000), pp. 289–300.

[6] For a critical analysis of this notion see Rex O'Fahey and Bernd Radtke, "Neo-Sufism Reconsidered", in *Der Islam*, vol. 70/1 (1993), pp. 52–87.

Sufism's injunction to stay away from the affairs of this world, and the so-called *nā'ib*'s *murīdūn*, who were little more than professional soldiers, albeit highly motivated ones. In a like vein, the persistent opposition to the *ghazawāt* on the part of some Naqshbandī leaders, including Shamīl's own preceptor Jamāl al-Dīn al-Ghāzī Ghumuqī[7], the chief Naqshbandī *murshid* of the Northern Caucasus, should have alerted the author to various possible interpretations of the Naqshbandī principles by members of one and the same mystical order.[8] Although the author is aware of Jamāl al-Dīn's opposition to armed struggle (pp. 143–4), she prefers not to give it much weight. Nor does she pay much attention to any evidence that contradicts her initial thesis. Instead, she engages in a persistent search of the Naqshbandī ''ideologies'' and ''networks'' that she views as the principle motivation and cause of the Caucasian *jihād* against the Russian colonial encroachment. Since the author is often hard pressed to find any proof of the Naqshbandī underpinnings of several mountaineer movements against Russian domination, her narrative is strewn with disclaimers bemoaning the absence of relevant data (e.g., p. 6 and 58, regarding Shaykh Mansour; pp. 127–8 regarding Avko; p. 134 regarding Beybulāt; p. 161 regarding Ḥamza(t) Bek, etc.). Finally, when there is absolutely no evidence of any Naqshbandī involvement in these movements, the author declares it to be ''symbolic'' (in the case of the Chechen leader Beybulāt, p. 134). The author's obsession with the Sufi foundations of the Muslim resistance movement in the Caucasus leads to some glaring contradictions. For example, on one and the same page (50) Zelkina recognizes the lack of evidence of any ''elaborate Sufi network'' in Chechnya, yet several paragraphs later ''confidently states'' that ''Sufi ideas were widely disseminated throughout the North-eastern [sic!] Caucasus.'' The fact that the first proponents of Islam in Chechnya were portrayed by the local folklore as ''holy men'' does not mean they were Sufis (pp. 48–9). The alleged ''Sufi connotations'' of these legends, invoked by the author (p. 49), are far from obvious to the impartial reader. Bringing her narrative to the Soviet period Zelkina discovers ''a clandestine network of *murīd* organizations'' that ''remained totally outside Russian reach'' (p. 236). I have criticized such ideas, which were introduced by some French Sovietologists on the flawed analogy with the North African *confréries*, in my *Islamic Mysticism*,[9] and will not repeat myself here. I cannot however ignore the author's closing arguments that, contrary to all available evidence from contemporary Chechnya,[10] ''the Naqshbandī legacy continues to dominate political and cultural discourse in the area''. Given the fact that most of the contemporary Chechens belong to the Qādirī *ṭarīqa*, which has often been at loggerheads with the Naqshbandiyya, this statement is but an eloquent demonstration of the danger of approaching a subject with ready-made stereotypes. While the role of the Naqshbandī order in the mountaineer resistance to the Russian conquest of the Caucasus cannot be denied, to derive it almost exclusively from Naqshbandī influences and institutions is a gross oversimplification. As I argued elsewhere, the notion of the ''militant'' and ''conspiratorial'' nature of the so-called ''Neo-Sufi'' orders was unwittingly appropriated by some western authors from the Russian and, later Soviet, sources that show a characteristic obsession with the clandestine plotting of some elusive ''secret societies'' and ''fanatical religious brotherhoods'' intent on undermining the civilizing and cultural agendas pursued by the empire. That the Russians were not unique in this obsession is attested by the abundant French literature on Maghribi ''confrérisme'' that was motivated, at least in part, by similar imperial fears. The flaws of the author's main argument are not redeemed by the quality of editorship. Even by the low standards of today's academic publishing (of which this reviewer has himself frequently fallen victim) the number of spelling mistakes in both the main text

[7] See, e.g., Kemper, *op. cit.*, pp. 83–4.

[8] A similar uneasiness about *jihād* was shown by the leadership of the Sanūsī brotherhood of Cyrenaica, which has often been described by Western scholars as a typical ''Neo-Sufi'' institution, see Knut Vikør, ''*Jihād, 'ilm* and *taṣawwuf*: Two Justifications of Action from the Idrīsī Tradition'', in *Studia Islamica*, vol. 90 (2000), p. 158.

[9] See note 5.

[10] Moshe Gammer, ''The Qādiriyya in the Northern Caucasus'', in *Journal of the History of Sufism. Special issue: The Qādiriyya Order*, vols. 1–2 (Paris and Istanbul, 2000), pp. 275–94.

and in Bibliography is staggering. There are factual mistakes as well. The mysterious ''Abū Maslama'' repeatedly invoked on pp. 26–7 and in the ''Index'' is, in fact, Maslama b. ʿAbd al-Mālik b. Marwān (d. 738), who indeed conquered and destroyed the city of Bāb al-Abwāb (Darband). On page 229, Shamīl's eldest son Jamāl al-Dīn is confused with the imām's spiritual preceptor Jamāl al-Dīn al-Ghāzī Ghumuqī. This confusion, in turn, leads the author to assert that the latter was mediating in Shamīl's secret negotiations with the Russian high command, of which al-Ghāzī Gumuqī was quite innocent. The author then proceeds to erroneously identify Shamīl's second son Ghāzī Muḥammad as the imām's ''eldest son'' (*ibid.*) The author's assertion that the ''prophet Muḥammad suffered a defeat at the battle of Badr'' sounds too radical a revision of Islamic history even for British Orientalism inveterately prone to skepticism. On the whole, Zelkina's book has provided a helpful historical survey of a critical turning point in the political, religious and social history of the Northern Caucasus. She has demonstrated a good command of Russian and Soviet sources on the subject and made use of heretofore little known works by local writers, including those directly involved in the Muslim resistance movement. These works however provide no irrefutable evidence to support her thesis that Sufism served as the main institutional vehicle and ideological foundation of the movement. It seems more accurate to see it as an important part of the very complex mix of circumstances and factors that provoked and sustained the movements discussed in the study. I would go as far as argue that Naqshbandī Sufism was incidental to the resistant movement in the Caucasus (its role could have been played by any other religious movement, such as the virulently anti-Sufi Wahhābiyya or the ''pacifistic'' Qādiriyya order, which indeed supplanted the Naqshbandiyya of Chechnya as a vehicle of Chechen resistance). Furthermore, this role should be viewed in conjunction with the other important pieces of the puzzle, especially the messianic expectations entertained by the local populations, the rigid and ostentatious enforcement of the *sharīʿa* by the majority of local resistance leaders, and the strong *ghazawāt* ideology that developed in response to the Russian oppression of the mountaineer communities. In particular, *ghazawāt*'s integration into the mountaineer code of honour that continues to serve as a major motive of contemporary Chechen resistance calls for further investigation. In sum, the role of Sufism should be seen in a much broader socio-political context than we find in the book under review. Rather than emphasizing its ''natural'' potential to launch and sustain movements of resistance, one should ask how various Islamic communities developed mechanisms of mass-mobilization in response to European expansion and why their resistance to the European challenge took different or similar forms in different parts of the Muslim world.

Alexander Knysh

Geschieden von allem ausser Gott: Sufik und Welt bei Abū ʿAbd al-Raḥmān as-Sulamī. By Lutz Berger. (Arabische Texte und Studien, Band 12). pp. 219. Hildesheim-Zürich-New York, Georg Olms Verlag, 1998.

This book provides the most comprehensive and detailed examination, to date, of the life and work of the great Sufi writer Abū ʿAbd al-Raḥmān al-Sulamī of Nishapur (d. 412/1021). After furnishing a brief description of al-Sulamī's age and outlining the major stages of his life and work (Chapters I–III), the author proceeds to discuss the Sufi writer's views of man and the world (Chapter IV), of the major ''stages'' and ''states'' of the Sufi path (V), of the Sufi communal life and relations between the Sufis and society at large (Chapters VI and VII). In Chapter VIII Lutz Berger examines al-Sulamī's concepts of mystical gnosis, divine love, ecstasy, Sufi auditions (*samāʿ*), prophethood and sainthood, mystical union and the knowledge of the unseen. In Chapter IX, he discusses al-Sulamī's role as an historian and systematizer of the Sufi tradition as well as his attitude towards several mystical and ascetic movements in Khurasan, in particular the *malāmatiyya* and the *ṣūfiyya*. This discussion is

followed by a brief conclusion (Chapter X), recapitulating the author's observations in the earlier chapters. Based on Berger's Ph.D. thesis, this work exhibits all the characteristic features of this academic genre. The author demonstrates an intimate knowledge of al-Sulamī's original writings, both manuscript and published. Equally impressive is his sure grasp of Arabic grammar and the Sufi terminology of the age. Throughout his study, Berger meticulously supports his observations with extensive quotations from the original Arabic sources at his disposal. In many instances, he also provides a transliterated version of the Arabic text in either a footnote or in the main body of the text. The author's narrative is deliberate, condensed, if, at times, overly dry. Addressed to the specialists on early Sufism who are already familiar with the problematic and the technical terminology of the field, the book is unlikely to have much appeal beyond this rather narrow audience. It will, however, be welcomed by Islamicists who seek a solid and well-documented summary of al-Sulamī's life and work. At the same time, those already familiar with al-Sulamī's work and the *Gedankenwelt* of early Islamic mysticism will not find here much that they have not already known. Berger relies heavily on the groundbreaking works of Johannes Pedersen, Richard Bulliet, Jacqueline Chabbi, Clifford Bosworth, Fritz Meier, and Tilman Nagel. While the author agrees with most of their conclusions, he does try to refine or challenge them by making use of the sources at his disposal. For example, he takes issue with Chabbi's claim that in al-Sulamī's work we are dealing with the "constructed" and thoroughly apologetic historiography aimed at proving the perfectly orthodox nature of ascetic and mystical movements in eastern Iran. Berger also disagrees with Chabbi's insistence that al-Sulamī deliberately obfuscated the movement's internal diversity by presenting it as a monolithic and unproblematic whole (p. 194). In contrast to Chabbi's skepticism, Berger takes a much more positive view of al-Sulamī's work. In his opinion, it does provide a reasonably accurate picture of the spread of Sufi movement in Khurasan and beyond, which was later confirmed and refined by other Sufi historians, especially al-Hujwīrī, Abū Nuʿaym and al-Qushayrī. In line with this idea, Berger refuses to draw a sharp line between the local ascetic-mystical movement known as the *malāmatiyya* and the Sufism "imported" to Khurasan from Iraq. For him, the former was nothing but a regional manifestation of the latter (pp. 175–89) and the difference between the two movements has been blown out of proportion by overly skeptical western scholars. One would agree with this thesis, if it can be proven that there indeed was a distinctive Iraqi Sufism that was shared by all Sufis in Iraq and Syria. This, however, is far from clear, given the internal diversity of the Baghdad school, not to mention the serious disagreements between the Basran and Baghdad schools of Sufism.[1] On the other hand, one cannot ignore the acute disagreements over pedagogical techniques between some Sufi shaykhs of Baghdad and the *malāmatī* masters of Khurasan. These disagreements are captured in the famous encounter between the Baghdad Sufi Abū Bakr al-Wāsiṭī (d. 320/932) and the followers of the *malāmatī* leader Abū ʿUthmān al-Ḥīrī (d. 298/910) of Nishapur.[2] In my view, any attempt to juxtapose or to reconcile an idealized and homogeneous Baghdad Sufism with an equally idealized and homogenous *malāmatī* movement of Khurasan is highly tenuous. Neither of them were as uniform and coherent as they are presented to us by later Sufi historians, including al-Sulamī. It seems more productive to see the relations between various ascetic and mystical trends as in the context of inevitable tensions between individual spiritual masters who, by the mere logic of events, found themselves in competition with their peers (either local or foreign, and who therefore were compelled to promote the advantages of their spiritual method at the expense of that of their rivals). That these disagreements were sometimes presented by Sufi writers as rivalries between regional ascetic and mystical schools should not obscure their underlying social reasons.

Berger's book raises a number of other important issues, including the periodization of Sufism's

[1] See my *Islamic Mysticism: A Short History* (Leiden, 2000), pp. 43–82.
[2] *Ibid.*, pp. 100–101.

history. In particular, Berger takes a critical view of Meier's thesis that al-Sulamī represented the end of ''classical'' Sufism and the beginning of its ''postclassical'' evolution that was characterized by the formation of Sufi institutions (brotherhoods) and the formalization of the relationship between the Sufi master and his disciples (pp. 195–6). In Berger's view, al-Sulamī can be as well seen as the harbinger of a new-age Sufism, whose work creatively combined the elements of both ''classical'' and ''postclassical'' ascetic and mystical traditions, producing a synthesis that has determined Sufism's development for many centuries to come (pp. 198–9). This is a good point. Unfortunately, Berger refrains from offering his own version of the major stages of Sufism's history, thereby weakening the thrust of his argument. I, for one, am not quite comfortable with a crisp distinction between ''classical'' and ''postclassical'' Sufism. This distinction springs in part from the apologetic agendas pursued by al-Sulamī and his fellow Sufi writers of the age, who viewed the Sufi masters of old as the unsurpassed paragons of piety and sanctity. While Berger makes extensive use of western scholarship, he surprisingly overlooks the groundbreaking studies of the major western expert on al-Sulamī's legacy, Gerhard Böwering, including the latter's critical edition of al-Sulamī's *Ziyādāt ḥaqā'iq al-tafsīr*.[3] Overall, al-Sulamī's monumental *Ḥaqā'iq al-tafsīr*, which has been the principal subject of Böwering's investigations for over two decades now, receives only a short shrift in Berger's book. Given the fact that the *Ḥaqā'iq* contains many exegetical passages of metaphysical nature, one should perhaps treat with caution Berger's repeated statements that al-Sulamī took no interest in speculative theology or mystical metaphysics (pp. 120–5 and p. 198). It is hard to imagine that he remained indifferent to the fascinating metaphysical speculations of the earlier Sufi exegetes, which he diligently assembled and preserved for the future generations. Also to be regretted is Berger's neglect of the pertinent, if controversial, articles by Christopher Melchert, who has challenged a number of common stereotypes regarding the relationship between the ''traditionalists'' and the early Sufi authorities and who has tried to pinpoint the momentous transition from the simple asceticism of Islam's early devotees to the fully fledged mysticism of the later Sufi tradition.[4] A discussion of these issues in the context of al-Sulamī's life and work would have been a valuable addition to Berger's study of how the Sufis positioned themselves *vis-à-vis* the other strata of medieval Muslim society in general and the learned classes in particular (pp. 83–101). My critical comments, however, in no way diminish the author's undeniable achievement. He has provided his colleagues with a detailed and thoroughly documented account of the life and work of one of the most influential Sufi writers ever. For this, he deserves our heartfelt gratitude.

Alexander Knysh

For God, Mammon and Country: A Nineteenth Century Persian Merchant. By Shireen Mahdavi. pp. xviii, 286. Boulder, Westview Press, 1999.

This is a very well written and engaging book. As suggested by its title, this is primarily a chronological account of the life of Haj Muhammad Hasan Amin al-Zarb, a pre-eminent merchant in nineteenth-century Iran. However, it becomes clear in the first pages of the book that the author has a broader aim than mere biography. In fact, Shireen Mahdavi describes her project at the outset to be ''a blend of social history and biography''. So not only does she outline the development of the career of Amin al-Zarb from his early days as a struggling merchant trying to pay back the debts of his

[3] *Minor Qur'an Commentary of Abu 'Abd ar-Rahman Muhammad b. Husayn al-Sulami (d. 412/1021)*. Ed. by Gerhard Böwering (Beirut, 1995).

[4] Christopher Melchert, ''The Transition from Asceticism to Mysticism at the Middle of the Ninth Century C.E.'', *Studia Islamica*, 83/1 (1996), pp. 51–70; *ibid.*, ''The Adversaries of Ahmad b. Hanbal'', in: *Arabica*, 44/1997/5 pp. 234–53.

bankrupt father, to a time when he becomes one of the wealthiest entrepreneurs with a business empire that extends far beyond the borders of Iran, but she also brings to life the world in which Amin al-Zarb lived.

Haj Muhammad Hasan Amin al-Zarb (c. 1834–1898) was born into an established family of *sarrafs* (money changers) in Isfahan. Even though, as a child, he did have the advantage of gaining some experience accompanying his father and grandfather to the bazaar and acting as an apprentice to them, he, soon, found himself on his own as the family fell on hard times. Upon discovering that his father had failed in his business venture in Kerman, and had died, leaving behind a number of debts and a temporary wife, with her own claims to a financial settlement, he is suddenly thrown into the position of acting as guardian to his mother and brothers, and so his "story" begins. In fact, the picture that Mahdavi portrays of Amin al-Zarb is of a pragmatist, very much in tune with the times in which he lived. That this period coincided with the encroachment of European influence into Iran, in terms of new markets and trading patterns as well as ideas, actually seemed to benefit Haj Muhammad Hasan. Not only was he receptive to these changes, but he was able to adapt and thrive on the new emerging trends. In fact, he made much of his fortune and reputation as a result of engaging in the business of import-export, ensuring that his *hujra* in the bazaar was considered exclusive in carrying select luxury goods from Europe. With regard to his degree of interest in new ideas, so intent is the author to capture and recreate the changing impressions and ideas of Amin al-Zarb, that she devotes many pages to describing what he would have seen and experienced on his several trips abroad, starting with the pilgrimage to Mecca, and his two trips to Europe. Not only does she describe the different routes that he could have taken in detail, but whenever her subject's personal papers do not provide her with the necessary information, she uses travel accounts or memoirs written by contemporaries to speculate on what would have caught his eye, and how these observations would have inspired him to introduce changes that would in turn benefit himself and his country financially and developmentally. So his *hajj* trip (with various stops in the Ottoman Empire and Egypt), for example, is said to have "laid the seeds for many original ideas and innovations in Haj Muhammad Hasan's future career" (p. 51), and his trips to Europe, to have made him aware of the institutions that were absent in his own country. Amin al-Zarb is, thus portrayed as a "transitional" figure, one who by virtue of the profession that he practices and the letters that he writes is not only representative of the changing times in late nineteenth-century Iran, but also sympathetic to reformist ideas. However, Mahdavi is at pains to point out that he was not "westernized". Thus she establishes his religiosity, and asserts that "his admiration for western technological and political institutions did not affect his private life or beliefs" (p. 166). In this way, she underlines the multifaceted nature of her subject, whom she depicts as being both religious and patriotic but also concerned with material gain, as is expressed in the first half of her title, *For God, Mammon and Country*.

Biography aside, this book has several other strengths. First of all, it is primarily based on the unpublished family archives of Amin al-Zarb, which consist of a large number of account books, monetary documents, as well as an array of letters and correspondence, of a commercial, financial as well as a personal nature. Thus not only do we have examples of exchanges between Haj Muhammad Hasan and his correspondents and representatives of his business empire, stationed in different major ports and cities, but there are also confidential exchanges between himself and his mentor, Amin al-Sultan, ʿAli Asghar Khan, as well as Nasir al-Din Shah. That this material was copied and preserved is in itself highly significant, not only in terms of the insight that it gives us into the character of Amin al-Zarb as one who may have perhaps been concerned about "posterity" (p. 112), but also as an example of the meticulous record-keeping that was practiced among individuals of different ranks in Qajar Iran. Secondly, it is by combining this rich collection together with an unfinished biography of Amin al-Zarb by his son (the full text of which is included as Appendix A), travel accounts and memoirs of other major figures in Qajar Iran that Shireen Mahdavi is able to shed new light into the

complex and paradoxical social and political setting in Naseri Iran. Whereas from one perspective, this could be considered a society with rigid social boundaries where different classes of people were distinguished from each other by way of dress and lifestyle, from another, it was flexible enough to enable an astute merchant like Amin al-Zarb to rise in the ranks, gain a foothold in the court, be appointed the "master of mint", become the protégé of the Prime Minister, and enjoy the favour of the Shah to the point of having the latter refer to him as "our own special merchant" (p. 95). Politically, however, the picture that emerges is of a court where the intensity of rivalry among courtiers means that the Shah is not totally in control and has to be wary of his entourage. This is to the extent that at times when Amin al-Zarb writes to Nasir al-Din Shah to complain about the inequities perpetrated by the local governors, he asks that his letters be kept confidential and returned to him after they have been read. Perhaps one of the more interesting episodes described in the book is the encounter of Haj Amin al-Zarb with Sayyid Jamal al-Din Asadabadi which develops into a lasting friendship. This rather mysterious figure about whom books have been written, is here presented as one whose charisma wins over Amin al-Zarb along with others. Not only is the latter said by his contemporaries to have "truly changed" (p. 101) as a result, but as is shown by Mahdavi, in fact, becomes the main financial benefactor of Sayyid Jamal al-Din for several years after.

In short, Shireen Mahdavi has fulfilled the objectives that she has set out in the preface to the book. Not only has she given us an account of the trials and tribulations of Amin al-Zarb as reflected in his own correspondence and papers, but she has also enabled us to gain a better understanding of the workings of politics and society in late Qajar Iran. Moreover, she has managed to make it read like a "story". The imaginative style of writing as well as the detailed information that it contains concerning the social mores of Qajar Iran also makes this a good textbook for students of Iranian history.

Negin Nabavi

The Architectural Heritage of Himachal Pradesh. By Laxman S. Thakur. pp. xx, 180, illus. 48 figs, 4 maps, 44 plans and drawings, 132 bl. & wh. plates. New Delhi, Munshiram Manoharlal, 1996.

There has been much debate on the relationship between the wooden and stone architecture of India. From the very first studies of the Hindu and Buddhist cave temples in Ajanta, Ellora, and Aurangabad it became apparent that the architectural details often represent timber structural components imitated – rather than interpreted – in stone. This is not a unique feature of Indian architecture. There is evidence that Greek temples and even the pre-Columbian architecture of Central America reflect the structural characteristics of timber buildings in stone, but no early wooden architecture remains in these regions because of the perishable nature of wood, which rarely survives more than a few centuries. In the Indian subcontinent, however, two regions have preserved the tradition of wooden architecture: Kerala in the south and the Himalayan states in the north. Gujarat also had a well-developed tradition in both stone and wood architecture, but timber structures in the region, well preserved until the early nineteenth century, have now virtually disappeared and only certain architectural elements have survived, mostly in museums and private collections. In Kerala, surviving wooden inscriptions indicate that at least parts of some structures are nearly five hundred years old, but Kerala does not possess any substantial stone buildings and for comparison one should look to the traditions of Tamil Nadu and perhaps Orissa.

Himachal Pradesh and the Kathmandu Valley in Nepal, on the other hand, are regions which have preserved both types of structure and their study provides an opportunity for understanding the relationship between the two traditions. While the splendid examples of the religious and domestic architecture of the Kathmandu Valley have long attracted attention, and many buildings have

undergone careful investigation and restoration with the aid of international funds, those of Himachal Pradesh have been left relatively neglected. One of the reasons is perhaps the quality of the wooden architecture, which although incorporating many fine details, is, on the whole, on a less grand scale than that of the Kathmandu Valley, but other determinants, such as the geographical terrain and internal and international politics may, have also been contributing factors.

Nevertheless, the importance of the architecture of Himachal Pradesh has been recognised since the mid-nineteenth century. Alexander Cunningham (*JASB*, 1848, 241–327; *ASIR*, V, 145–84; XIV, 109–25) was the first to carry out a study of the major temples at Mandi, Baijnath, Bharmaur, Chatrahi and Chamba on the route from Simla to Srinagar as well as Kangra on the route from Amritsar to Baijnath. These old routes are the main access to the lower Himalayan ranges and pass through fertile valleys cut by the Ravi and Beas Rivers and their tributaries, where settlements have developed since ancient times. Although the area was relatively accessible in the mid-nineteenth century, Cunningham's studies omitted some of the important monuments, including the great rock-cut temple of Masrur set in the high ridge of a mountain near Kangra. Nevertheless, the architecture of the region soon attracted the attention of other scholars. Fergusson devoted a section to the subject in his *History of Indian and Eastern Architecture* and more detailed studies followed; the first by Harcourt in 1871 covering the monuments of Kulu and other regions; then in the early twentieth century that of Marshall, who published a number of the monuments in the *ASI Annual Reports*; and later Voglel (1946), Chetwode (1968), to whom the present work is dedicated, and many others.

Thakur's recent book is a fresh study of the subject and while the author has benefited from the earlier publications the work is based mostly – but not always – on his own fresh survey of the buildings. The informative notes and extensive bibliography indicate that the author, a history and archaeology lecturer at Simla, is closely familiar with the subject and has made a good use of his knowledge, although some of the expressions used for building types and names are unconventional. The chapters of the book are structured on the basis of these categories.

The book first briefly describes the geography of the region, which lies in the south-western foothills of the Himalayas. The ancient routes through the valleys of the Beas and Ravi rivers, already noted, link up with the Sutlej and Yamuna valleys via certain passes, connecting the region with the Punjab plains on one side, Jammu, Kashmir and Ladakh to the north and east, and Uttar Pradesh to the south-east. This route would have played an important role in bringing the architectural traditions of North India to the region. The more ancient stone architecture of Kashmir, which echoes the traditions of the eastern Greek satrapies in its roof structures and the pediment-like features on the façades, is not immediately apparent in the present stone temples of Himachal Pradesh. A reason may be that the existing buildings are of much later dates and relate almost entirely to late mediaeval North Indian temple architecture of the *nāgara* type, distinguished by *śikhara* towers. The temples of this type cover a main part of the book, as they are prevalent in the region. These buildings are constructed entirely of carved and dressed stone, with a plan consisting of a single shrine (*garbhagṛha*) roofed by a *śikhara* tower and with a small porch in front of the entrance. Thakur describes the features of this building type and others in an introductory chapter, and follows with descriptions of the major monuments, the oldest and perhaps the most imposing of which is the complex of rock-cut temples at Masrur. Other temples of this type described are those in Balag, Baijnath, Bajaura, Bharmaur, Chamba, Dashal, Dhabas, Hat Koti, Jagatsukh, Kangra, Naggar, Nirath, Nirmand, Saho, Sarota, Saura and Triloknath. The work presents a collection of the major temples in one volume, but is neither a gazetteer of all temples, nor are those discussed presented in a particular order such as chronologically, as most of the temples are of course not dated. This is, however, a minor point and with a little searching one can find the required reference.

Most of the *śikhara* temples discussed are of the simple type already described, but there are a few which are more elaborate, amongst them the Śiva temples at Hat Koti and Saura and the Bāśeśara

Mahādeva temple at Bajaura, where in addition to the entrance porch there is a niche similar to the porch on each of the other three façades. The type is a common, but more elaborate, variant of the original type and reveals perhaps the extent of the influence from the southern plains into the mountains. Another variant can be found in the Vaidyanāth temple at Baijnath were a much more elaborate plan with an antechamber (*maṇḍapa*) four times the size of the *garbhagṛha* is designed between the shrine and the porch. This type of plan, known as *mūlaprāsāda*, is popular for the grand temples of Rajasthan, Gujarat and Orissa, but rarely appears in the Himalayan regions. A third variant seen in the Chandraśekhara temple at Saho is the type where the shrine is surrounded by a colonnade. The type is not very common in Himachal Pradesh, but more commonly seen in Nepal, and examples of which are the Lon Degaḥ temple built in 1664 in Kirtipur and the Vatsala temple (1696) in Bhaktapur.

Thakur gives a brief mention of the Chandraśekhara temple, with a plan, but the form of this heavily repaired building is perhaps worthy of more extensive discussion, as the type may present a rare occasion where a direct influence of Himalayan wooden architecture can be observed in the form of a stone temple. A colonnade around the chamber is a structural characteristic of the Himalayan tiered temples, where the timber roof, much larger than the shrine itself has to be supported by external columns. The traditional form of North Indian temples is governed strictly by the rules laid down in the mediaeval Sanskrit *śilpas*, treatises on temple construction. The design of *śhikhara* temples outside these rules is a marked departure from the older traditions, at a time when the wooden architecture of the region had already entered a mature period.

The two other building types according to Takhur are the 'Pent Roofed Temples' and the 'Pagoda Style'.These two types are local and their roots can be traced in the vernacular architecture of the Himalayas.The main characteristic of these types is the timber roof over a simple masonry or brick structure. The names Thakur uses for these types are not entirely appropriate as the term 'pent roof' is given for those which have a single gabled or hipped roof, and 'pagoda style' to the temples with tiered roofs. The term 'pagoda' (as Yule put it in *Hobson Jobson*, 'this obscure and remarkable word', used to refer to 'idol temples' by early European writers) seems particularly unsuitable. The term is neither Indian nor used locally, and does not strictly refer to buildings with tiered roofs, although it has been used earlier in this sense by some writers such as Bernier in his book *The Nepalese Pagoda*.

In both types the masonry or brick structure is rather simple with little or no decoration, but the woodwork may carry fine and elaborate carving. Thakur discusses only a few examples in each category. Amongst the buildings of the former type the temples at Bharmaur, Chatrarhi, Jagatsukh and Udaipur are given. These temples are indeed the most impressive examples of their type, particularly the Markūla Devī temple at Udaipur, which contains some very fine carvings. However, the type, essentially consisting of a shrine in the form of a simple timber roofed hut, is perhaps most commonly applied for the construction of local temples in villages. Tiered temples – landmarks of the Kathmandu valley towns – are hardly seen outside the Valley in the Himalayas, except in Himachal Pradesh, where, according to Thakur at least fifty examples are known, and there may be still more to discover. While a link between the temples of these far-apart regions is somewhat to be expected there is little evidence for establishing such a link. Only three of the tiered temples at Kammand, Mandi and Nirmand, are discussed in the book, but there are others – and at least one with five tiers – which although simple structures with little decoration, could perhaps help expand our understanding of the relationship between the Nepalese and Indian tiered temples. The three which are discussed do have certain architectural features shared with those in Nepal. Although there are occasional examples of temples with four-tiered roofs, the essential rule given in the Nepalese temple treatises indicates that temples should have an odd number of tiers: three, five or seven. The temples at Kammand, Mandi and Nirmand are in three tiers, the common form in the Kathmandu Valley.

The proportion of the tiers and many other structural details, however differs in the temples of the two regions.

There has been much speculation about the origin of the Himalayan tiered temples, some claiming that the form is indigenous to India, and perhaps once prevailing from Kerala to the Himalayas. Apart from some superficial similarities there is in fact little relationship between the structural methods employed for the wooden architecture of Kerala and that of the north. Vogel (*Indian Arts and Letters*, 1946, 33) suggested that the pediments of the ancient temples of Kashmir may be a reflection of the tiered form, and others argue that the form has been imported from China to Nepal or vice versa, again without any hard evidence. Thakur makes his own suggestions and attempts to find the origin of the form in among other things, the umbrella-like finials (*chatra*) of the ancient Indian stupas. The light structural nature of this form supported by a single slim central shaft does not, however, compare with that of a massive tiered roof, which requires the support of solid walls and columns. As far as no historical and cultural evidence is at hand to support possible links between the forms in different parts of the world it may be more relevant to study the structural properties of the form not from the point of view of cultural influences, but through the eyes of the architect and builder. The form could have developed independently in each region from vernacular and domestic architectural techniques for dealing with a humid climate with a long rainy season. Without entering a long discussion, in these areas houses usually consist of a simple masonry structure with a veranda supported on columns at all sides, to keep the rain away from the foundations. The core of the structure itself is taken higher than the level of the veranda, often with high windows for ventilation above it. The roof of the veranda, together with the higher roof of the central core of the house gives the appearance of a tiered structure to houses in these regions. If a ventilation level is applied at the upper part of the hipped roof, a feature characteristic of the domestic architecture of Kerala, it looks like a further tier.

The book continues with two further chapters on architects and craftsmen, and on the socio-economic function of the temples, followed by a concluding chapter and appendices. A map at the end of the book marks the location of the temples in the region, and following the glossary, bibliography and index are 82 clear and well-produced photographs of the temples and their details. While the reader may disagree with the author on minor points, this does not detract from the value of the book as a relatively concise work in one volume for finding quick and reliable references to the main temples of Himachal Pradesh.

Mehrdad Shokoohy

Creating an Islamic State: Khomeini and the Making of a New Iran. By Vanessa Martin. pp. 240. London, I. B. Tauris, 2000.

The major thrust of this valuable book is a two-tiered attempt at exploring and analyzing the experience of tradition and modernity by the Islamic groups in Iran since the revolution of 1979. The first tier involves a discussion of the intricate relationship between the thought of two of the principle architects of the Islamic revolution in Iran, Ayatollah Ruhullah Khomeini and Ayatollah Murteza Mutahhari, and the discourse of modernity. At the second level, the institutional dimension, the author engages in an examination of elements of tradition and modernity that can be found in the structure of the state that was constructed since the founding of the Islamic Republic.

At the discursive level, Vanessa Martin discusses different influences on Khomeini's thought, to counter the western misperception of Ayatollah Khomeini as a "dogmatic cleric with rigid views on Islam and the West derived from a hermetically sealed religious tradition". The first influence has been that of Greek political thought, especially Plato's *Republic* and its interpretation by al-Farabi (d.

950), the *al-Madina al-Fazila* (Virtuous City), on the notion of the leadership of the Islamic community, which in Khomeini's later thought crystallized as the Guardianship of the Jurist (*Vilayat-e Faqih*). The second, and more important, influence, has been that of *'Irfan*, or ''Islamic Mysticism'', on the thought of Khomeini. Khomeini's many years of learning and teaching philosophy and *'Irfan*, two subjects frowned upon if not banned by the mainstream of the Shi'i seminary, allowed him to construct a view that approximates some of the basic elements of the metaphysical foundations upon which the modern world stands. These metaphysical elements that view humans as active subjects endowed with the power of will and agency, can be gleaned from the visions and teachings of some of the major figures of *'Irfan*. Martin shows how Khomeini and his student Mutahhari, actually developed philosophical notions such as ''self-discipline'', that arises from ''self-knowledge'' and which in turn give rise to ''self-empowerment'', on the basis of their reading of *'Irfan* and philosophy. The very informative, but unfortunately brief, second chapter of the book deals with the major figures of *'Irfan* and philosophy in the Islamic world, such as Muhyi Al-Din Ibn 'Arabi (d. 1240), Mulla Sadra (d. 1640) and Mulla Hadi Sabzavari (d. 1878), examining how their ideas, principally the notion of the Perfect Man (*insan-i kamil*), was utilized by Khomeini to create notions of a revolutionary leader, a political vanguard akin to the Marxist-Leninist body, and to a lesser degree, to mobilize the populace. In these *'Irfani* and philosophical traditions the idea of Perfect Man refers to an existential journey that purportedly elevates certain humans to commune with the divine and the consequent acquiring of some of its power and wisdom that is necessary for leadership.

To be sure human empowerment is one of the hallmarks of the modern world. However, it is the attempt at dissemination and sharing of this power that truly marks the positive aspects of the modern era – what makes democracy possible. The empowerment of rulers, whether new or traditional, can hardly be considered a modern phenomenon. Moreover, the principal beneficiary of human empowerment in modernity has been the individual citizen. Of this fact, Martin is, implicitly, cognizant. Ayatollah Khomeini, however, did not pay much attention to the notion of the individual, because the idea of the individual citizen as the locus of empowerment was of little interest to him, at least at the theoretical level. It was Ayatollah Mutahhari who was most interested in the notion of the individual and the socio-political implications thereof. However, Mutahhari's interests in the question of the individual was mostly by default and stemmed from his critique of collectivist and materialist ideologies such as Marxism and the socialist interpretation of Islam that was popularized by Ali Shari'ati and his followers in Iran. As Martin shows, Mutahhari as a student of Khomeini, also was well versed in *'Irfan* and Islamic philosophy. Like Khomeini, Mutahhari utilized certain notions of human empowerment and agency derived from the *'Irfani* and philosophical sources. Unlike Khomeini, however, he addressed the question of the individual to an extent unsurpassed by other Islamic thinkers of his time in Iran. Yet, the notion of the individual that Mutahhari was subscribing to was different from that of modern liberalism: ''To Mutahhari the individual is thus a believer who promotes [the interests of the socio-religious] system – a view which may be contrasted with the cornerstone of liberalism, the individual pure and simple'' (p. 88). On the other hand, as Martin points out, Mutahhari did emphasize the freedom of moral choice by the individual who is charged with responsibilities and duties.

At the second level of her analysis, the institutional dimension of Islam and modernity in Iran, Martin discusses the different features of the institutions that were built upon the thought of Khomeini and his students, chiefly Mutahhari, through the efforts of their supporters. Chapter 3 is devoted to a discussion of the social movement that was thus created by Khomeini and his lieutenants as well as the organizational dimension of the movement whose mode of operation was based on an increasingly tight network of bazaar merchants, shopkeepers, and artisans. Khomeini and his lieutenants by experience had learned the values of the modern model of centralized organization under a single leadership that was provided by Khomeini himself.

Among the institutions that were created in the aftermath of the 1979 revolution, the state is by far the most important. Martin traces the evolution of the concept of Islamic state in the thought of Khomeini back to his early work, *Revelation of Secrets* (Kashf al-asrar) published in mid 1940s, where he first broached the idea of a more active involvement of the clerics in the affairs of society and state, alluding to the desirability of a strong supervisory role, but not direct governing, by clerics. As Martin points out, however, it was only in the early 1970s that the full-fledged idea of the Guardianship of the Jurist emerged. The Islamic state that was established in 1979, Martin contents, is a centralized and strong state that is modelled on the ''modern'' state that the Pahlavi monarchs had already created.

The paradoxical standing of legislation by humans in the state that Khomeini and his followers constructed is a topic that receives due attention in this book. In conformity with his notion of the authority of the Guardian Jurist to rule on behalf of the Hidden Imam, Khomeini opposed the idea of legislation by the people. This is despite the fact that the Islamic Consultative Assembly or the *majlis* has been quite active ever since the revolution. The aporia of legislation is in fact a reflection of the larger question of sovereignty that Martin shows prevails in the Islamic state in Iran: the dual sovereignty of God and the people enshrined in the constitution and institutions of the Islamic Republic.

In another chapter Martin compares the discourses of Khomeini and Mutahhari to those of their counterparts in other parts of the Islamic world, namely, the thoughts of Abu'l Ala' Mawdudi, Sayyid Qutb, Rashid Rida and Hasan al-Banna. In the same chapter the ideas of another political Islamist movement in Iran, that of the Fedayian Islam, and their leader, Navab Safavi (executed in 1956) are also compared to those of Khomeini and Mutahhari. These comparisons are indeed valuable because they shed light on the mutual influence of these thinkers on one another and the similarity of the essential ontological and political ideas that each of these thinkers developed, as well as the issues and challenges that their respective movements faced.

In other chapters of the book, the author discusses certain questions that do not seem to bear a direct relevance to the main thrust of the book, notwithstanding the interesting discussions presented there. These questions include the treatment of the Western media of the Islamic revolution and state in Iran as well as the contemporary Muslim perspectives in other countries of the Islamic revolution in Iran. However, the most unfortunate shortcoming of the book is the lack of development of the conceptual issues that are essential for a book that attempts to analyze such troublesome and complex notions such as modernity and tradition. In the book, Martin's discussion of what may constitute modernity and tradition is limited to a mere sentence on each in the introduction. Furthermore, the inadequate development of theoretical leads that emerge in the book makes for a partial frustration of the otherwise excellent research effort.

Despite these criticisms, the book is an inspiring work of scholarship on the history of contemporary Iran and very stimulating in terms of exploring the possible indigenous sources for the construction of modernity based on philosophy and *'Irfan* in a Muslim setting.

Farzin Vahdat

The Parsis of India: Preservation of Identity in Bombay City. By Jesse S. Palsetia. pp. xv, 368, (Brill's Indological Library, Volume 17). Leiden, Brill, 2001.

Himself a Parsi, Jesse S. Palsetia [JSP] has limited the formation and evolution of the protean character of Parsi identity – *pārsīpanu* – to the confines of Bombay where the majority of Parsis are even today to be found. Of necessity he takes us back to their modest beginnings along the south Gujarat littoral and its hinterland. The well-traversed couplets of the *Qisse-ye Sanjān* are invoked in ratification of early Parsi history: 720 AD is given here as the traditional Sanjān fire-temple establishment date. The Parsis had embarked for Sanjān from Diu after their 19-year sojourn there, alerted to the Arab

invasion of Sind in 714 AD, giving a more realistic reading of 716 and not 936 to an ambiguous *Qisse* cipher. Fire-temples and funerary enclosures usefully occupy our attention, both being the mainstays of Parsi religious identity. A political identity took shape through their contacts with the Great Moghuls when European travellers first encountered the Parsis as traders, entrepreneurs and brokers. Adaptability was their key to interactive communal relations, whilst preserving their religious particularism. Social and cultural norms reflected the changing extent to which they had successfully integrated into Indian society (p. 12). JSP gives wholly credible instances of such mutations and integrations in the course of his later chapters. Properly speaking, the Parsis remained external to the Indian caste system, just as surely as they were to stratify into *athornān* or priestly, and *bēhdīn* or lay classes with an underclass of *nasāsālār*-s or (untouchable) morticians who doubled as public criers in their communal wards.

This excellent survey is shared out among six chapters, each keyed with ''Identity'' in its title, and cushioned between a thoughtfully composed Introduction and what could easily have been a mine-field of an Epilogue. The author skilfully steers a steady course through the turbulent waters of post-Colonial Dispersion history, bestowing favours upon none, and emerges gloriously with the Epilogue's triumphal note on page 337. Indeed, his Parsis have come proudly through thirteen hundred years' history on the subcontinent, visibly dwindled in numbers, but undying in spirit.

''Identity and the Urban Setting'' reflects Parsi endeavour in the wake of the Portuguese ceding of Bombay to the British in 1662, marking the rise of their first great entrepreneurial families. The religious signposts of the devout Parsi – the Fire-temple and the *dakhma* or funerary edifice, were constructed: the first, in 1673, in the Fort district; the latter safely distanced on Malabar Hill (hardly ''on the Pedder Road''! p. 37) across the bay in 1675. Parsi landed gentry of the *shetia* families duly materialized through the wealth-amassing opportunities which enticed the enterprising spirit of these pioneer settlers lately come from humble agricultural and artisan pursuits around the northerly coastal towns and villages. Property, textiles and shipping formed the power bases of the eighteenth and especially the nineteenth century Parsi entrepreneurs. With expansion in shipping, their mercantile acumen was exercised farther afield, to the Gulf Arabs and beyond to the west, and South-east Asia and China whose treaty ports offered secure entrepôts. We are provided accurate résumés of the dealings of these merchant-princes and their successes.

With success came the founding and disbursement of charities on an undreamed of scale. Here JSP displays a curious reticence: ''The advantages for the Parsis in cooperating with the British *were not meagre*''! (p. 51). They were in fact *massive*! The ''holy thirst for gold'' took on an unholy twist when the patriarchs of at least three vastly affluent families participated in the odious but lucrative opium trade with the Far East. The taint attached to such activity was ethically cleansed with the unarguable defence that had they not dealt in this commodity, someone else would most certainly have! To be sure, Jews and Khojas soon muscled in, ultimately relieving the Parsis of their nefarious burden. The fortunes made were generously diverted into unimpeachable charitable activities paradigmatic of the prospered Parsi: from educational establishments through affordable communal housing and industrial complexes, and on to exemplary ameliorative works often extending well beyond community bounds.

JSP's utterly absorbing account of the founding, growth and decline of the Parsi Punchayet of Bombay exemplifies the thorough research through which he has proceeded to delineate the changing Parsi perceptions, and identity, under British administration of Bombay. This system of internal government of communal affairs well suited the Parsis: it is important to realize that the identity redefinitions entailed prolonged conflicts between the urban and provincial *panchāyats*, some unresolved to this day. One such issue concerned inter-class marriages among the priestly and lay sections. Underlying the former's objections to allowing their daughters' out-marriages was the economic disadvantage incurred by dowry expectations. With the relinquishing in 1777 of the

Navsari priestly authority over the Bombay Parsis, this anomaly was gradually removed. In fact the traditional conjugal system, complete with infant marriage, bigamy, dowry arrangements, estrangement and divorce, had led to all manner of contractual abrogations; with its considerably augmented regulatory powers the Bombay *panchāyat* resolutely managed to assert its authority for the closure of the several loop-holes exploited and widened by the venturesome Parsi. The Laskari case is concisely presented and analyzed. So are the difficulties which attended the instances of religious conversions: they were mostly dealt with by the manufacture of traditions intended as safeguards and means of social cohesion and control (pp. 33, 93). With its own internal difficulties and the enforcements of the 1865 Parsi Marriage, Divorce, and Succession Acts, the Parsi Punchayet of Bombay entered upon its decline; the author winds up this comprehensive chapter with a telling "Dénouement".

Out-conversions significantly occupy "The Challenge to Identity". The Scottish divine John Wilson became notorious in the 1830s for his conversion to Christianity of three Parsi boys who had attended his mission schools. The implied threat to Parsi socio-religious identity was firmly countered by a stout riposte supported by rationalist philosophic argument. It led to the educated Parsis re-examining and revaluating their own religion, thus laying the foundations for well-structured studies of Zoroastrianism. The enduring contributions of Dadabhai Naoroji, K. R. Cama, Nowrosjee Fardunjee, Shapurji Bengalee and B. M. Malabari became increasingly appreciated, having countered the religious ignorance of the priesthood (p. 163). Absurdly accused of being reformists and menaces to social cohesion, today they are recognized for initiating the overdue regeneration of the faith among the Parsis.

JSP's detailed discussions on the social and religious aspects of identity are engrossing to the point of near effacement of his personal standpoints which place him at the forefront of impartial scholarship. It now becomes easier to distinguish between a Parsi and a Zoroastrian (pp. 235; 247) despite the intricate arguments, advanced and demolished by learned counsel at earlier times, of various judicial enquiries into cases of acceptance or rejection of alien and adopted converts by opposed factions within the notoriously litigious Bombay Parsi community. In suggesting, however, that "the Parsis may only have ended the practice of selectively admitting non-Parsis to the Parsi community by the seventeenth century" (p. 87), he appears to have overlooked both the *Itthōtēr* Rivāyat (1773) confirmation and Carsten Niebuhr's personal observation (c. 1765) in his *Reisebeschreibung in Arabien* . . . (1774/8). The Bansda and Mazagaon *navjote*-s are recorded (p. 321).

The bulk of JSP's thesis, of which this handsome book is "a substantial revision", concentrates on the British period when the real guides of the community's welfare were not religious leaders but those whose lasting endeavours for the establishment and maintenance of urban identity were recognized by the imperium. Some dozen immensely distinguished Parsis figure among the modest number knighted on the subcontinent: the author has faithfully outlined their considerable achievements in both the legal and political arenas. He could perhaps have extended his generosity to some very great prelates who knowledgeably exercised their scholarly authority in the last and first quarters successively of the nineteenth and twentieth centuries. One thinks of some half-dozen rightfully honoured as *shams-ul-ulema*-s. The visceral outpourings from among those less intellectually endowed among the modern-day representatives as *ex officio* "ultimate arbiter(s) of faith and identity among the Parsis" (p. 326) have sadly resulted in a diminishing regard for the Parsi priestly institution.

A major plank on the Parsis' cultural platform was the wonderfully inventive Parsi Gujarati vernacular with its uninhibited poetic licence which found its fullest expression in press and public debate, popular literature, and the irreverent *nātak*-s of farcical theatre. A misprint has shown mid-1830s instead of 1930s for Parsi participation in the Indian film industry. Throughout, the author has evinced great accuracy and insight when tackling those aspects of Parsi productivity which most bear upon the shaping and assertion of a changing identity. The sections on education, especially female education, and the press are notable instances: they are also quite entertaining.

The Parsis of India must surely take a rightly deserved place at the very peak of studies on this dwindling but undaunted community. It is congruent with Kulke's 1974 classic which it now both supplements and surpasses; the earlier standbys of the obviously dated Karaka (1884) and Murzban/Menant (1917) today have only an archival feel, both moreover being consciously Anglophile; the recently translated Volume III of *The Parsis* from the later chapters of Menant's original 1898 *Les Parsis* is a readily available tribute to *fin-de-siècle* scholarship. If one work only has to sum up modern erudition, it must be Palsetia's.

Accompanied by an adequate Glossary (prefaced by a curiously muddled note!); an extensive and obviously thoroughly utilized Bibliography; a serviceable Index, being a directory of great names with puzzling inclusions (Conan Doyle and George Edalji), two unchecked *Quisseh* and a Bramo (!); and, not least, representative plates of Parsi institutions, formidable phalanxes of laymen and priests, priests officiating at *navjote*-s, Parsi youth over the last two centuries, family portraits, and some well known potentates and public figures.

A rather ponderous and repetitious style detracts somewhat from the solid merits of JSP's approach and sound argumentation: it makes for occasionally heavy-going reference, and often requires re-reading; but the excellence and immense value of his contribution outweigh these really quite minor irritants. Why the *Bundahishn* is "a religious work" (p. 120) mystifies this reviewer. Having throughout exhibited a commendable impartiality as truly befits the tenor and scope of his far-ranging thesis, JSP has carefully avoided the excesses of some *école britannique* proponents' offerings on Parsiism. JSP's well-judged references to the reformist and orthodox/traditionalist factions are cases in point: one wryly notes (p. 124) the (associated?) contrast of "lower orders" and "elite".

Farrokh Vajifdar

Kalapani: Züm Streit uber die Zulässigkeit von Seereisen im kolonialzeitlichen Indien (Alt- und Neu-Indische Studien, Band 52). By Susmita Arp. pp. 258. Stuttgart, Franz Steiner Verlag, 2001.

Originally a German PhD thesis, *Kalapani: Zum Streit über die Zulässigkeit von Seereisen im kolonialzeitlichen Indien* offers a survey of the debate which raged in the nineteenth and early twentieth centuries on the question of whether Hindus should travel overseas or not. Many Hindus travelling abroad during this period, mainly to England but also further afield, faced opposition from their relatives as well as social ostracism or exclusion from their caste groups on their return to India. Re-admission to their caste community could often be achieved only through undergoing expiatory rites, but even those did not always rub out the stain of having crossed the seas and resided in non-Hindu countries. Susmita Arp charts this dispute presenting an overview of the wide variety of opinions put forward, the people and organizations involved and the different modes of argumentation used. Her study draws on newspaper articles, conference reports and proceedings, published books in the vernacular as well as short stories and a theatre play. Most of her material deals with Bengal in the period from c. 1870 to the mid-1890s.

The controversy about the prohibition on sea-travel acquired public prominence only in the 1860s when the number of voyagers began to increase. Previously it had been up to each traveller to make their peace with their caste community, or not, as the individual preferred. A number of developments, technological as well as social, combined to propel the matter into the public arena. Firstly, sea-voyages to England became significantly shorter after the 1850s. Steamships as well as the opening of the Suez Canal in 1869 helped to cut the travelling time from Bombay to Southampton from about six months to three weeks. Secondly, more and more Indian students travelled abroad to acquire a western education and to train as barristers or physicians. Additionally, in 1853 the entrance exam to the Indian

Civil Service was opened to Indians but candidates had to come to England to sit the exam. Thirdly, greater numbers of Indian elite society journeyed to England. The aristocracy did so to pay homage to the British royal family and to take part in society events, and Indian social reformers in order to bring their causes to the attention of British politicians, the press, charities and influential individuals. Of prime importance to the issue of the permissibility of sea-travel was that these Hindu voyagers belonged to the upper castes which had no previous seafaring history and thus faced stringent opposition from members of their own communities in their attempts to break the mould.

Arp shows that the debate about sea-voyages was just one in a series of concerns addressed by the broader movements of social reform, along with other causes like remarriage of widows, abolition of child marriages, education for women and the relaxation of caste barriers. These social reform movements, which arose in the second half of the nineteenth century, were initially dominated by people inspired by western values and ideas who sought to modify Hindu social customs along western norms. Towards the end of the century, however, social reform came to focus on India's Hindu heritage and social reformers looked to their own intellectual traditions to justify a reformation of their customs. The idea took hold that India had once been a great (sea-faring) nation with flourishing arts and sciences, had experienced a drastic decline, partly because of Muslim conquest and occupation, and now needed to rediscover the pristine Hinduism of its golden age to become great again. The Hindu revivalist movements, thus, drew on a selective understanding of their past in their efforts to standardize Hinduism and to turn it into a systematic and universalistic belief system. Spreading nationalist sentiments led to a further reassessment of attitudes towards western or British culture. At the heart of all these developments and repositioning was the basic question of how to deal with the British and the values and norms they propagated. This theme also informed the dispute about overseas voyages, which was largely concerned with the problem of the possible wholesale adoption of western culture by Hindu travellers and the ensuing difficulties they would cause to "traditional" Indian society.

Arp examines the broad spectrum of arguments advanced both for and against the interdiction on ocean-crossing voyages. Assertions in favour of the utility of such trips for educational and commercial purposes, which would benefit the country as a whole, were joined by arguments drawn from different Hindu scriptures that generally all but sea journeys to acquire *dharma* were allowed. These were countered by claims that most journeys were only made for reasons of self-aggrandizement or personal amusement, to the detriment of the Indian population, as well as the fact that the journey itself by boat contradicted rules of purity and that caste, especially food regulations could not be kept outside of India. Through the controversy about sea-travel, Arp shows further the dual nature of Hindu "traditionalism": the growing involvement of pandits, providing manifold interpretations of Hindu scriptures, in public disputes, but also the increasing "lay" readings of these sacred texts made possible through the nineteenth-century revolution in printing and publishing. By analyzing and juxtaposing the opinions of pandits (1870 to 1890s), both for and against sea-travel, proceedings and publications of the "Sea-Voyage Movement" (1892–1894), the theatre play *Kalapani*, "Black Water", by Amritalal Basu (1892) and a short story by Rabindranath Tagore (1894), Arp is able to refute the notion of a clear division in Bengali society between traditionalist opponents of sea-travel on the one hand and liberal promoters of it on the other. The debate as delineated by Arp was very much more complex and centred largely on the traveller's motives for his trip and how he conducted himself both while abroad and on his return.

Arp's book throws light on one aspect of a fascinating topic: that of how Indian society dealt with the challenge of westernization, or acculturation, in the colonial period. How much western culture should be allowed to inform Indian society, how much inspiration should be sought from within Hindu tradition? And which elements of Hinduism were to be emphasised in this revivalist phase and who would choose them? *Kalapani* provides a useful insight into this dilemma. The fact that the

public discussion on the permissibility of Hindu sea-voyages was conducted largely on a regional basis permits Arp to limit her study to the province of Bengal with only occasional references to developments in other parts of India. This gives the book great internal coherence and allows for a tightly arranged structure. It is a pity, though, that the author decided not to include any personal experiences of sea-travellers, two short examples on the first page of the introduction apart; this might have underscored her finding that travellers were treated differently by the leaders of their caste communities based on the motive of their journey. On the technical side of things, a glossary of terms and, even more fundamentally, an index would have made this book much easier to consult. That said it is a competent first work which utilises its sources well.

Claudia Liebeskind

The Bombay Country Ships, 1790–1833. By Anne Bulley. pp. xvi, 288. Richmond, Curzon Press, 2000.

Students of eighteenth and early-nineteenth century British India will no doubt testify that there is a real need for a well-researched and comprehensive discussion of the country trade. It is a topic that is often mentioned, usually in passing, but rarely in depth. It is, moreover, a subject about which the same body of constantly recycled information, conclusions and comments comprises the sum of our knowledge. For many, Ann Bulley's exhaustive study will certainly go some distance towards filling this real need and towards providing a far more detailed and textured picture of the era than has previously been the case.

After reading a book like this, it is easy to see why the subject has been neglected for so long. Research into the myriad of sources and archival collections necessary to do the task justice requires considerable diligence, great patience and dedication. Ms Bulley has done a herculean and commendable job in bringing together a real wealth of new information about the Bombay country trade. She has mined the India Office and Colonial Office records, other collections of private papers in the British Library, and the Jardine Matheson Archives at Cambridge University and the collections of English-language Indian newspapers. It was clearly not an easy book to write, unfortunately it is not an easy book to read.

One key problem is the mass of detail with which she burdens the reader. While the weight of this detail is to some extent alleviated by the author's attempts to organize and frame the material, one feels that more could have been done in this respect. The book is divided into six sections. This structure offers some clue to Bulley's analytical framework, such as it is. These are: shipping; East India Company (EIC) controls & involvement; the Bombay country trade; the country ship owners; the mariners; the decline of European commitment to the trade.

The section on shipping covers such topics as shipbuilding, the different types of Asian shipping on the coast, including something about the names and designs of ships, the trade routes, cargoes and ports. This includes a number of digressions on such topics as the role of the Parsis in the Bombay shipbuilding industry; the interface between urban and port facilities in Bombay; fire controls; and the construction and maintenance of rope-walks and dry-docks. Part two deals with the role of the EIC in licensing, protecting and exploiting the country trade. A key issue here is the importance of the country traders in providing transport and other sorts of support for Britain's and the EIC's naval operations in Asia, particularly during the Napoleonic wars.

This section details the vital role that country ships played in Britain's wars, particularly against the French. Between 1801 and 1821, country shipping participated in five major expeditions. Their role mainly involved moving troops and supplies. These included the Egyptian campaign of 1801, the campaign against Mauritius (or Ile de France), and others against the French and their local allies in

the Persian and Arabian Gulfs. They were also involved in later moves against ''pirates'' in the same areas. The relationship between the EIC and country shippers was close and truly symbiotic. If the country traders helped Britain win her wars, they not only enjoyed the fruits of victory, but also considerable profits from their involvement.

The third section on the Bombay country trade is perhaps the heart of the book. In it, Bulley looks at the trade largely according to its various geographic sectors. Thus she discusses the trade with the Malabar coast, the African and Arabian Gulf trade and finally the China trade. There is also a chronological and topical dimension to the discussion here as well, particularly in her treatment of the China trade. These sections deal first with the cotton trade to China and then, following a discussion of the changing nature of the trade in the early decades of the nineteenth century, she discusses the trade which became all important for Britain, the Malwa opium trade to China.

The fourth section, dealing with shipowners provides a wealth of documentary evidence for the rather heterogeneous ''balance'' of interests that were represented in the average Bombay agency house. The typical company consisted of three partners, one a company servant, one a European free merchant and the third a native, usually a Parsi. It was very often the Parsi who actually supplied the working capital and economic expertise to make the enterprise function. This shows the close connections that had grown up between the EIC and the local economy as well as the hybrid nature of the trade. This picture of the trade is reinforced in the next section which looks at the men who actually carried the country trade, the mariners. Aside from fairly small cadres of European officers, the crews were largely Asian in composition, including South and Southeast Asians.

The final section on the decline of European ''commitment'' deals with the fundamental changes that occur in the global trading patterns of the mid-to-late nineteenth century. These include the increasing importance of metropolitan interests in the imperial economy, the declining role of Asians in upper reaches of that economy and their relegation to more menial functions. It also marks the sharper line of demarcation that appears to separate the smaller scale, local Asian trade from the global, European-dominated trade.

Despite her emphasis on the importance of the native/Parsi role in the trade, Bulley, like many colonial historians, is often blandly uncritical of contemporary European discourse regarding Asian traders and mariners. Thus she repeats without comment such term as ''pirate'' when applied to the various fleets of Arab, Indian, Malay and Chinese maritime forces. Although she is clearly aware that some groups that were so labelled by contemporary British officials and maritime personnel, were actually agents of Arabian and South and Southeast Asian states, she does not always present a critical perspective on colonial perceptions toward such shipping.

Finally, Bulley's language and writing style often leave much to be desired. The style varies from the mundane to the difficult, and even the atrocious. There are frequent lapses into sailor-speak, which, even with the glossary, are sometimes incomprehensible or confusing to the layman. While she seems to have been quite conscientious at regularizing and updating the spelling of South Asian personal and place names, she seems to have been quite negligent in exercising the same care in regard to the spelling and orthographical usages for Southeast Asian and Chinese names. She is often quite content to accept the irregularities of eighteenth-century European renderings rather than update and correct them. Thus in reference to the Malay chiefs of early Singapore, she refers to the ''Sultan Tomongong'' when in reality there were two officials: the Sultan and the Temenggong. She uses ''Macasser'' not Macassar or Makassar; ''Minto'' instead of Muntok; ''Banca'' instead of Banka; ''Palamban'' instead of Palembang; ''Billeston'' instead of Belitung (pp. 147–8); and ''Kumsingoon'' instead of Kumsingmoon. One wonders how a non-specialist would ever find the places she mentions on a modern map. Even fairly prominent European names are not spared this negligence: thus, ''Carl Geutzlaff'' instead of Karl or Charles Gutzlaff (p. 162) and John Crawford for John Crawfurd, (p. 261, fn 210). Even in the case of South Asian names, there could have been greater attention to the standardization

of spelling. It is clear that the book could have done with another close editing before it went to press.

Despite these problems the book still represents a major contribution to the literature on the eighteenth- and nineteenth-century economy of Bombay and the country trade. By bringing this considerable body of archival material into print, Bulley's work will be necessary reading for anyone working on the economic history of the empire, or India in this period. It is, however, a book for experts and specialists on eighteenth-century British India, and even in that respect, it may more often be of use as a work of reference rather than as a comprehensive narrative of the topic.

Carl A. Trocki

Hindu Art and Architecture. By George Michell. pp. 224, 186 illus., 77 in colour. London, Thames & Hudson, 2000.

George Michell's latest book in Thames & Hudson's *World of Art* series is a welcome addition to the growing number of affordable, well-illustrated books that introduce Indian art to a wider audience. Michell is well known for his numerous publications in the field of Indian art and architecture, especially Hindu, over the last twenty-five years. This volume stands alongside two other books in the *World of Art* series, Roy Craven's *Indian Art* (1976, revised 1997) now rather out-of-date, and Robert E. Fisher's useful *Buddhist Art & Architecture* (1993) that includes material from throughout Buddhist Asia and not just India.

Deciding what to include in a book about Hindu art is as problematic as defining "Hinduism", and whilst acknowledging the problem of isolating Hindu art from the broader Indian artistic tradition, Michell adopts a narrow definition of Hindu art as "being confined to shrines consecrated to Hindu divinities, semi-divine personalities and mythological narratives" (p. 8). This would seem to be a wise decision, however awkward it is to present, for example, the later Hindu art of the fifteenth to eighteenth centuries in relative isolation from the wealth of both contemporary secular or Islamic paintings and architecture. This is a book about Hindu art in South Asia, primarily India, so interested readers will have to look elsewhere for details of the Hindu art of Khmer, Cambodia or indeed the West in the past century.

Following a short preface outlining the format of the book, the first chapter introduces the main themes in the study of Hinduism and its visual expression: a broad survey of the development of Hinduism over four thousand years, the use of images, the iconography and myths of the main deities, narrative traditions, the architectural setting for much Hindu art, and the impact of royal patronage. The subsequent five chapters each cover a period of a few centuries with titles suggesting the broader developments of the Hindu visual tradition over two thousand years – Beginnings, Early Maturity and Culmination for the period second century BC to thirteenth century AD, and following the Muslim invasions of north India c. 1200. Revival and Lesser Traditions for the period from the fifteenth to the twentieth centuries. Subheadings divide the wealth of material into more manageable sections, defined by region and/or dynasty.

This book is particularly notable for its ambitious aim to cover the whole two thousand years of the Hindu visual tradition, demonstrating artistic continuities down to the present day. Many previous books have highlighted the earliest material up to the Muslim conquest in the twelfth-thirteenth centuries and either ignored or only cursorily covered the great numbers of Hindu monuments and sculptures produced after that in both the North and the South. Mention is therefore made of the temples built in Rajasthan and the Mathura-Vrindaban region in the sixteenth to eighteenth centuries, the wall-paintings of Kerala and Tamilnadu, and the narrative cloth-paintings from all over India that only survive from recent centuries. The historical breadth of this book admirably complements

Richard Blurton's *Hindu Art* (London, 1992) with its more iconographic approach to the Hindu visual arts, from the monumental to the "popular" or "folk" arts of the present.

Hinduism is a very visual culture and so both the author and publishers are to be congratulated for producing such a richly illustrated book. There are 186 illustrations in the book, 77 in colour, so almost every double-page spread has one or more illustrations, often occupying a full page. This allows several views of the most important monuments: the mid sixth-century cave-temple at Elephanta, for example, is illustrated with an annotated plan, a general view of the interior and two details of the sculpted panels (pp. 66–9). Many of the monuments, sculptures and paintings illustrated are familiar but there are plenty that are of unusual sites, such as the Shantadurga temple near Ponda in Goa built in 1738 (p. 167), or objects from little-studied artistic periods, such as the late sixteenth century copper image of Yashoda seated with Krishna from Tamilnadu in the Metropolitan Museum of Art, New York (p. 186).

Hindu Art & Architecture is recommended as an excellent introduction to Hindu art in India for the student or general reader, with a generous number of high-quality illustrations and at an affordable price of £8.95 in paperback.

CRISPIN BRANFOOT

THREE SANSKRIT TEXTS ON CAITYA WORSHIP IN RELATION TO AHORĀTRAVRATA. By RATNA HANDURUKANDE. (Studia Philologica Buddhica 16). pp. xxv, 132. Tokyo, International Institute for Buddhist Studies, 2000.

This silm volume presents three rare and interesting works relating to the practice of the *ahorātravrata*, a religious observance which – as its name implies – extends over a day and a night. The works presented include the *Ahorātravratacaityasevānuśaṃsāvadāna* (AVC), and two texts bearing the title *Ahorātravratakathā*: one in verse (AVK) and another in prose (AVK(P)). In all, these texts provide valuable insights into a certain mode of Buddhist discipline as related to the cult of the *caitya* and its subtending and legitimating narratives. In general, scant attention has been paid in Indological circles to such "practical" literature, and one values the publication of these works in a readily-available edition. However, one wishes more care had been taken with the production of the book as a whole.

The AVC is a work in 356 verses, which was edited on the basis of five manuscripts from Calcutta, Tokyo, "London", and "Berlin" (the latter two being microfilm copies of these MSS hailing from Bonn and the Nepal-German Manuscript Preservation Project). The AVK edition is based on five manuscripts – four from Tokyo and one from "Berlin". This work is also chiefly in verse (140 total), with a small prose section at the outset. The last work, the AVK(P), was edited on the basis of the sole testimony of a "very faulty" MS found in the library of Tokyo University. It is a very brief prose version of the AVK. The texts are presented in Roman transliteration with word divisions indicated where possible. Proper names mentioned in the works are indicated with italic type, which is helpful.

As mentioned above, although the author specifies that the texts were established on the basis of "manuscript copies", he appears to mean "copies of manuscripts". The MSS described as deriving from "London" and "Berlin" are specified as being microform copies of these manuscripts. As the author does not indicate as much, we must assume that these copies were not checked against the originals – in principle an essential task for anyone attempting to establish a proper critical text, though one often overlooked in practice by Indologists. One assumes the MSS deriving from the Tokyo library were consulted directly, though this is not specified.

Very little is done with regard to exploring the nature and history of these texts. Indeed, despite a

longish citation from an insightful letter written to the author by Michael Hahn, and a list of lexemes which illustrate Hahn's observations on the language of the texts, the author provides almost nothing in the way of insights based on his own research. Hahn's contribution notes that – while the AVC is for the most part quite standard Sanskrit – the other two texts are composed in a register he calls "Newari Hybrid Sanskrit" (NHS), whose characteristics reflect the "Newāri mother tongue of their author(s). Handurukande, for his part, suggests (notably without argument or illustration) that the AVC and the AVK show a "literary style" similar to that of the *Avadānamālā*, which (we are told) Winternitz dated to the sixth century (p. iii).

Appended to the edited works, Handurukande provides "synopses" of their content in English. However, the rationale – indeed, the method – behind Handurukande's synopses is by no means clear. The reader is never told precisely why they are presented with synopses rather than full translations, and an examination of these synopses reveals a rather inscrutable picture. For example, at the beginning of the AVC, the setting of the teaching is given in a style typical of Buddhist literature, enumerating the various types of beings who were present at the teaching, and so forth. The list of audience members is quite elaborate and not terribly remarkable: gods, *gandharva*-s, *kinnara*-s, *vidyādhara*-s, etc. Yet the "synopsis" includes the names of nearly all of them and, in fact, *adds* explanatory material, such as noting that *kinnara* are "mythical beings", *graha* are "spirits", *garuḍa* are "mythical . . . birds", and so on (Sanskrit, pp. 2–3; English, p. 102). Indeed, seemingly the only thing left out in this passage is the phrase – repeated at the beginning and end of the list – about the audience's desire to "drink the ambrosia of the teaching". This does not, then, seem to fit the standard profile of a "synopsis", i.e. "a brief or condensed statement presenting a combined or general view of something".[1] It is not at all clear who the intended beneficiary of these "synopses" is meant to be. Those unable to read Sanskrit may be misled into thinking that the author(s) of these works believed *kinnara* to be "mythical beings"; and scholars of Sanskrit, one assumes, can make up their own minds about the reality of *kinnara*. As Handurukande points out, his synopses "are not an adequate replacement for a translation" (p. vii), but he does not give a concrete reason why he did not undertake such a comprehensive translation. One wishes a clear choice had been made between either a more comprehensive or a more consistently selective treatment.

There are a few further small, but noticeable, flaws in the production of the book – particularly in terms of the typography. The text was evidently set by computer, yet an adequate character set was apparently not found. The a-macron (ā) and u-macron (ū) characters are both truncated in the italic font. The publishers have consistently used a cedilla (¸), rather than the usual dot (˙), below letters s, n, and t for the retroflex sibilant, nasal and unvoiced stop(s), respectively, though the voiced retroflex stops are presented by the standard symbols (ḍ and ḍh). The velar nasal (ṅ) also is presented in non-standard orthography, being represented by an *accent aigu* (´) over the letter n. In general, the diacritics used do not match the base font and this – compounded with the non-standard usage of diacritical marks mentioned above – detracts from the book's appearance.

Overall, however, this volume provides a valuable glimpse of some fascinating works relating to both *caitya* worship and the nature of *vrata* among Indian Buddhists of the first millennium. As Handurukande observes, "serious research must . . . be based on the texts themselves." Whatever the shortcomings of the book, Handurukande has done a great service by drawing attention to these works and in publishing them in a convenient form. One hopes that such "serious research" may now proceed apace.

Christian K. Wedemeyer

[1] *The Compact Oxford English Dictionary*, 2nd Edition, (Oxford, 1991), p. 1994.

RELIGION, HEALTH AND SUFFERING. Edited by JOHN R. HINNELLS and ROY PORTER. pp. xviii, 498. London and New York, Kegan Paul International, 1999.

This collection arose from a conference at the Wellcome Institute in 1996, funded by the Wellcome Trust and SOAS. The contributors come from a number of disciplines including religious studies, history of science, anthropology and medicine. Not all of them say much about religion, but most of them say something about medicine, and the collection is a welcome addition to the literature on cultural and ethical issues in health care. Suffering and medicine are present in the book as a felt reality: Porter's paper is dedicated to Roselyne Rey, a medical historian whose death from cancer prevented her contributing to the conference, and the collection is dedicated to Hinnells' orthopaedic surgeon. A general practitioner, John Cohen, writes with few theoretical aspirations but much human awareness about his experiences in multi-ethnic London, and some of the papers end with recommendations for dealing with patients from particular faith communities. Finally, David Parkin pulls all the disparate material together in an essay which goes beyond the oppositions of material and spiritual, biomedical and holistic which appear in many of the papers, to develop a cross-cultural interpretation of the various ways of understanding human personality and what afflicts it.

Several of the papers are concerned with problems of theodicy, whether in the etymological sense of "God's justice" or the wider sense of finding a way of satisfying the human demand for fairness in a world in which suffering appears to be unfairly distributed. Julia Leslie describes the way in which the idea of *karma* serves this demand in Sanskrit texts and in modern Hindu discourse; she points out that it is more readily used to account for the suffering of others than for one's own, and finds similar ideas in popular British culture. Dominik Wujastyk uses Sanskrit medical texts to exemplify the view that miscarriages and infant deaths are caused by demons who attack those who have done evil in a previous birth, or have failed to take ritual precautions. Abdulaziz Sachedina examines classical Muslim views on suffering and God's will, contrasting the Ash'arite emphasis on God's power with the Mu'tazilite view of human responsibility. He ends with a brief account of Islamic views on modern clinical dilemmas, but with little discussion and little connection with what precedes. Lawrence Conrad also discusses Islamic theodicy, in particular the view that plagues, which were frequent in the first two centuries of Islam, were God's punishment, and the alternative view that those who die from them are martyrs, while those who flee are attempting to thwart God's will. Religious responses to plague are examined also by Andrew Wear, who finds a distinction in sixteenth and seventeenth century England between prudent fear of the plague, which recognises it as a form of God's wrath, and impious fear which leads to social disruption and dereliction of Christian duty, and can even be fatal. Pashaura Singh compares Sikh views on suffering with other theories of theodicy using examples from the *Ādi Granth*, and also describing some contemporary spiritual healing practices.

Other papers are concerned with attitudes to medicine in different religious traditions. John Hinnells links this theme with that of theodicy in a survey of Zoroastrianism, described in the introduction as "perhaps the only religion to formulate a doctrine which depicts suffering as wholly evil and separate from the will of God". Besides discussing this radical solution to the problem of theodicy, he traces the involvement of Zoroastrians in medicine in ancient and modern times, relating it to their beliefs as well as their historical situation. David Melling describes some Christian attitudes to sickness and medicine, from the Patristic period to Pope John Paul II. A common theme is that suffering is a distinctive aspect of humanity, shared by Christ; another is that Christ is the archetype of the healer. Norman Solomon identifies three Jewish approaches to healing: popular, scientific and theological; he exemplifies a wide range of theological views on modern medical questions, and on how far Talmudic decisions, made when medical options and knowledge were far more limited, are applicable today. Tadeusz Skorupski surveys Buddhist doctrine as found in the Tipiṭaka and some

later texts, both Theravāda and Mahāyāna. The focus is mainly on *dukkha*, taking the Four Truths as a starting-point but also including some less well-known passages; another important topic is attitudes to the body, including the problem of the Buddha's body and his capacity to suffer pain.

Some papers are mainly devoted to elucidating particular medical traditions. Francesca Bray gives a detailed account of the Chinese medical tradition and its social contexts, including its relation to alchemy and geomancy (though some of the detail is obscured by the absence of tone marks, let alone characters). She points out that the Chinese traditions locate well-being in the patriline rather than the individual, though this has not prevented the "cheerful syncretism" of practices such as acupuncture with Western medicine. Peregrine Horden examines the attitudes to pain in the Hippocratic corpus, and finds a general lack of interest in it which contrasts with the expressive and nuanced statements about pain and suffering elsewhere in Greek literature, especially in the tragedies. Helen King raises the question of why some ancient Greek patients went to shrines of Asklepios and others to Hippocratic practitioners; her answer involves cross-cultural anthropology, which she persuasively argues is preferable to the uncritical analogies with modern culture which have often been resorted to by classicists. Geoffrey Samuel describes the use of the resources of Tibetan medicine by Tibetans in India, tentatively relating this to Buddhist attitudes to worldly well-being. Alcinda Honwana describes the rituals used to restore relationships with society and the world of spirits after the traumas caused by war in Mozambique, and discusses the beliefs underlying them, contrasting the ways in which traumatic memories are erased with the methods of modern psychotherapy. Roy Porter gives a detail-packed survey of ideas from the Enlightenment onwards about pain, its mechanisms, its functions and its divinely ordained or naturally selected purpose. Contrary to some of the theorists he describes, he believes that pain is something we can well do without, and that alleviating it is one of medicine's more useful aspirations. A far more lightweight paper by Mike Saks questions the division between "scientific" medicine and "unscientific" alternative systems; it is unfortunate that he did not consult Wujastyk before completing his account of Āyurveda.

Another area addressed is the political and philosophical assumptions underlying systems of provision for the sick. Roland Littlewood examines the language and structures of recent British provision for the mentally ill, in a densely written paper which is at once highly theoretical and driven by practical concern. Carole Rawcliffe describes how medieval English hospitals provided for the spiritual welfare of the sick poor, and of their wealthy patrons and founders, on the principle that medicine for the soul should precede medicine for the body.

One useful effect of a multidisciplinary collection such as this is to encourage readers to look outside their speciality. We can expect sinologists to take up this book and be attracted by the Jewish or African material, or theologians to gain new insights into clinical practice; this is facilitated by a thorough index. However, there is no overall bibliography, the authors having been left to give their references in their own different ways. In general, the authors have been conscientious in making their specialities comprehensible to those outside them, but some difficulties remain. One such difficulty arises from the need for a value-free term to refer to the kind of medicine which is taught in faculties of medicine in Britain and many other countries. (It is interesting to learn that Tibetans refer to it as "Indian medicine", which shows how relative a geographical term can be.) Some of the authors use the term "biomedicine", apparently in this sense but without explanation. The index has an entry for this, and for another term "Western biomedicine", but in many of the places referred to, the authors just call it "Western medicine". The index thus attempts to impose a term which is not used by all the authors, and is nowhere explained in the book; those readers who know it at all may know it in a different sense.

Dermot Killingley

A Jaina Perspective on the Philosophy of Religion. By Arvind Sharma (Lal Sundarlal Jain Research Series XVI). pp. xix, 153. Delhi, Motilal Banarsidass, 2001.

The author, Birks Professor of Comparative Religion in McGill University, Canada, adds this to his earlier books on the philosophy of religion in relation to Hinduism, Buddhism and Advaita Vedanta. In the preface the author points out that philosophy of religion, one manifestation of the relationship between religion and reason, has evolved primarily in the context of Western Christian civilisation. To redress this he first approached Hinduism. Jainism, he rightly says, cannot be neglected when considering the Indic tradition. (Not everyone would accept a dichotomy between religion and reason: faith and reason, as the author shows in Chapters V and VI, both subsist within religion.)

This is a fairly short book. The author's sources are secondary books and articles, all in English. They receive clear references in the footnotes on nearly every page, but the use of *op. cit.* involves hunting back through all the book's footnotes to find what *opus* is *citatum* and is irritating. The references are not gathered in a bibliography: there is just a brief list of half a dozen works for ''Recommended Reading'' at the end. There is also no index. Sanskrit and Prakrit terms are used sparingly, romanised with diacritical marks: they are explained in the text but a separate glossary would have been helpful. The proofreading is not perfect and some lapses can cause confusion. For example, p. 5, l. 3, quotes from Radhakrishnan ''eternal time, without form, beginning or end . . .'' but makes nonsense by adding ''with'' before ''beginning'', and a wrong page reference is given. Some words are missing between pp. 118–19, rendering the sense obscure, a footnote is missing on p. 60, the four footnotes ''Ibid.'' on pp. 61–2 refer to P.S. Jaini, not K.C. Sogani, and there are other examples.

Sharma relies heavily on John H. Hick's *Philosophy of Religion* (fourth edition, 1990): there are innumerable footnote references to this small work and hardly any for other writers treating the western view. On the Indian side the author draws frequently on Radhakrishnan's classic *Indian Philosophy*, also on P.S. Jaini's *Jaina Path of Purification* (1979). As well as general books on Indian religion and philosophy, he uses a number of familiar books and articles by Indian writers specifically on Jain philosophy,.

The author takes Hick's definition of ''philosophy of religion'' as ''philosophical thinking about religion'', distinguishing it from the philosophical defence of religious beliefs or ''apologetics''. Defining ''religion'' in a way which will include Jainism can cause problems. Hick suggests that it does not have a single correct meaning but that the various phenomena of religion are related in a way which, after Wittgenstein, may be called ''family resemblance''. One widespread phenomenon is the concern with salvation or liberation, which, according to Hick, ''is probably not a feature of 'primitive' or 'archaic' religion''. Professor Sharma points out that, although widely considered as ''primitive'' or ''archaic'', Jainism does possess an unambiguous soteriological structure.

The first three chapters deal with God: ''The Concept of God'' (Chapter I), ''The Case for God'' (Chapter II) and ''The Case Against God'' (Chapter III). It may seem strange devoting nearly a quarter of the text to God when considering what is often described as an atheistic religion. The Jain religion leaves no room for an omnipotent creator God but nonetheless the idea of deity is important. First, the liberated siddha is referred to, and worshipped as god; secondly, nearly every soul has potential to attain liberation and to that extent may be regarded as god, and thirdly, heavenly beings, goddesses and gods of India, have a part in Jain worship and are real to the people. Discounting the realities of religious life in favour of nothing more than an antiseptic philosophy misrepresents the religion. Sharma brings this out in Chapter 1, ''Though God is not a creator, the Jaina religion lacks neither devotional fervour nor ceremonial rituals'' (p. 4, n. 1), as anyone who has attended Jain religious functions will testify.

A lengthy footnote (pp. 8–9, n. 1) quotes from N. N. Bhattacharya's *Jain Philosophy: Historical*

Outline with the argument of the Svetambara writer Gunaratna, (c. 1400 AD) against a creator God, ending "So there is no God and the world was never created. The Universe has neither beginning nor any end, and hence any conception of God as creator is absolutely unnecessary." (There is no indication who Gunaratna was: the same is true of Mallisena (late thirteenth century AD), cryptically misprinted as "Malliea", whose Syad-Vada Manjari is quoted, at secondhand, on p. 33.)

In these three chapters the author examines the arguments for and against the existence of God, with considerable dependence on Hick. Sharma applies Saint Anselm's ontological argument to Jain philosophy by mutating the definition of God, "a being than which nothing greater can be conceived", substituting "higher" for "greater". In Jain belief the liberated soul rises to the very summit of the universe (and nothing higher can be conceived) (p. 16). The argument is not very convincing. An interesting point is that the challenge of modern science has not been felt acutely by Jainism, the author attributing this to Jain adherence to commonsense and the doctrine of *anekāntavāda* (p. 35). On one level this is true: the author might have cited the work of a distinguished mathematician and committed Jain, Professor K.V. Mardia's *Scientific Foundations of Jainism*. Yet, on another level, outdated scientific views of 2 000 years ago still govern much of ethical practice and cosmological beliefs.

The problem of evil and suffering is answered both in Christianity and Jainism by the concept of freewill, the individual's moral responsibility. In Christianity this leads to the moral precept to love one another, in Jainism to the fundamental requirement of *ahiṃsā*, the avoidance of harm even to the least living being.

At the beginning of Chapter VI Professor Sharma quotes Hick as saying that, in spite of the intellectual efforts to demonstrate the existence of God, it is indemonstrable. The Three Jewels, commonly translated as Right Faith, Right Knowledge and Right Conduct, are fundamental to the Jain spiritual path, as is stated in the opening words of the *Tattvārtha Sūtra*. In this, and the previous, chapter Sharma examines the Jain concepts of right faith and right knowledge in the perspective of western epistemology.

The Jain doctrine *anekāntavāda*, literally "non-one-sidedness", that reality takes many forms like the blind men's perceptions of an elephant, can also be used as a philosophical method to reconcile, integrate and synthesise conflicting (should this be *apparently* conflicting?) philosophical views (pp. 118–19). It deals with the problem of religious pluralism, the subject of Chapter IX. The author quotes Siddhasena: ". . . a man who knows the "non-one-sided nature of reality never says that a particular view is absolutely wrong". (p. 112). This is also discussed in Chapter VII on religious language.

The last two chapters tackle human destiny, from the western and from the Indic perspective. Fundamental is the difference between the Judaic-Christian-Islamic view that we come into this world but once, and the Hindu-Buddhist-Jain belief in the transmigration of souls. Also distinguishing the two traditions, which both accept the existence of the soul, is the answer to the question whether only humans have souls. The body-soul dichotomy formulated in ancient Greece has remained as self-evident truth in the West. Jainism too accepts the soul as eternal and, in its pure state, devoid of matter. The question how *karma*, conceived as material particles by the Jains, interacts with the non-material soul then arises.

The discussion on heaven and hell is limited to the eschatological vision, of Jainism as of Christianity: the Christian heaven is equated with *mokṣa*, hell with the fate of those *abhavya* souls damned never to achieve *mokṣa*. The cosmological diagram so familiar in the textbooks with its graded heavens and hells, still within the cycle of birth and death, is passed over. The book ends, rather abruptly, with a "demythologised" model of *karma* and reincarnation offered by P.S. Jaini. With every soul existing along a continuum of consciousness, from the *nigoda* to the *siddha*, we have a model which is both linear and evolutionary.

In a short review it is not possible to cover the whole of the author's discussion. It ranges widely over western philosophy of religion, tackling then the Jain approach to each topic. The author's sources are limited, both on the western and the Jain sides: at one point it seemed that it might be renamed a Jain perspective on John H. Hick. A reader versed in the philosophy of religion but ignorant of Jainism should find it a useful perspective. The student of Jainism would perhaps learn less but would benefit from seeing how Jain philosophy fits into a western structure. Given the continuing paucity of books in the Jain field in European languages, this new work is welcome.

PAUL MARETT

SUI-TANG CHANG'AN: A STUDY IN THE URBAN HISTORY OF MEDIEVAL CHINA. By VICTOR CUNRUI XIONG. Center for Chinese Studies. Ann Arbor, The University of Michigan, 2000.

Victor Xiong's *Sui-Tang Chang'an* is a serious study of one of China's most important cities during its most dynamic historical period. It is a significant contribution to Chinese urban, imperial, and institutional history.

Much about Sui-Tang Chang'an deserves description in the superlative. Its plan is so close to symmetrical that Xiong describes it as executed to perfection (p. 1). The approximately 320 years from the unification of China by Sui Wendi in the 580s until the collapse of the Tang dynasty in 906 are arguably the most glorious period in China's multimillenial history. Documentation about the city is more extensive than that for almost any other Chinese capital, including Ming or Qing Beijing. From the point of view of archaeology, as well, more is known about what stood on every square foot of Chang'an during these three centuries than about even Beijing during its imperial period. Xiong has trawled primary and secondary sources to write his book. His use of texts and clear footnoting will be invaluable to any future student of the city. Xiong has worked hard, however, to take the reader beyond straightforward, factual textual information and interpret or at least elucidate the city whenever possible.

The book is organized in ten chapters plus the introduction, with four appendices, about a dozen maps, interspersed with photographs and diagrams, and bibliography. The organization is logical, Chapter 1 dealing with the city up to the Sui period, Chapter 2 on the plan itself, Chapters 3–9 devoted to specific sectors in the city, and Chapter 10 the conclusion. The maps are largely computer-enhanced diagrams based on plans published in historical and archaeological sources. Sometimes the schematized renderings do not contribute much to the reader's understanding of the space of the city, but the English labels and relevant Chinese characters are extremely helpful.

Chapter 1 is a clear, textually based summary of the most important rulers and buildings in Chang'an from the third century BC to the rise of Sui Wendi. In the second chapter the reader first learns about Xiong's own ideas on the city, focused on what he calls morphology. His purpose is to take issue with the longstanding views of Naba Toshisada and Chen Yinke, published in 1930 and 1963, respectively, who find influences from imperial urbanism of non-Chinese rulers such as the Xianbei in China during the period of disunity (third through to sixth centuries) and non-Chinese names of officials associated with the plan of Chang'an, such as Yuwen Kai, to be strong enticements to view the sources of the plan as outside of China. Xiong does not completely deny the impact of cities like Ye and Luoyang from the three centuries before Sui on the Sui-Tang capital. Rather, he argues strongly for what he calls a cosmological basis for the city plan, siting according to divination, numerology, and alliance with trigrams of the *Yijing* (Book of Changes). The earlier theories are not ruled out by an acceptance of Xiong's scenario, presented in graphic form in map 2.3. Nor are traditional interpretations of Chinese imperial planning, in general, as grounded in a passage that

describes the ideal city of a ruler in the *Kaogong ji* (Record of Artificers) section of the *Zhouli* (Rituals of Zhou), impossible according to Xiong.

The subjects of the next seven chapters are the palace city, the Daming and Xingqing Palaces, the imperial/administrative city and other related government offices, ritual centres such as the ancestral temple, Mingtang, and suburban altars, market places, the approximately 100 residential wards of the city, and religious institutions. The one subject that might have been added are imperial tombs, at least those in the near suburbs of the capital. Readers will find much of the information in the seven chapters and the accompanying charts and appendices invaluable, particularly information about rituals conducted in and around them. The last chapter tells the story of the departure of the Tang court from Chang'an and ultimately the fall of Tang China.

In general, Chinese cities are still among the less-studied subjects of sinology. As explained in the second paragraph above, Sui-Tang Chang'an is a city that lends itself to study. It is thus not that surprising that a major independent work on the Sui-Tang capital, Thomas Thilo's *Chang'an: Metropole Ostasiens und Weltstadt des Mittelalters 583–904* (Wiesbaden, 1997) was produced at the same time Victor Xiong was writing his book. (A second volume of Thilo's study is underway.) The authors used many of the same texts and plans, and much of the documentary information about the structures in the city and some of the same historical facts and insights relevant to them, are found in both books. If ease of reading English or German is not an issue, readers with serious interests in Chinese urbanism and Tang cities and institutions are encouraged to read both. Xiong's book is more interpretative than Thilo's but as Thilo explains in his forward, interpretation of the city plan, or what Xiong calls morphology, will be found in part two.

As an initial work in English on such a major subject, certain stylistic and editorial decisions made by Xiong are especially apparent, and will be followed or amended by subsequent work on Chinese capitals. One practice that I like is Xiong's retention of Romanized names with characters provided for the almost countless halls and structures of Chang'an. In my opinion, few of the names lend themselves to a single translation. I am happier to see Daming Hall used throughout, with characters following its first usage and easily available in the glossary, than wondering if Great Bright Hall might have been better translated as Great Hall of Illumination and if so, if there might be some connection to the Mingtang (Hall of Brightness, Illumination, or other possible English names). Even for building types, which deserve explanation, translation can be a problem. Xiong provides translations and explanations for his translations on pp. xiii–xiv. Even with the explanations, a few of his attempts to find an equivalent term from the European architectural tradition may not resonate well with readers. Although Xiong explains that *dian* is a large palatial structure/hall, he translates it as basilica, for which the primary meanings are specific to ancient Roman architecture and Medieval church architecture in Europe which derived from it. Unlike the majority of basilicas, *dian* need not be oblong in plan with aisles and galleries, and would never have an apse. Estrade, used by Xiong as a translation for *tai*, has variant meanings in its original Latin form and in its Spanish and in French variants. In English it can sometimes mean a platform, but its primary meaning is the elevated portion of a floor, not a platform on which an entire structure is raised which is the meaning of *tai*. The other quibble readers might have is with Xiong's quotes from Han *fu* ("rhapsodies") at the beginning of Chapters 3 through to 7, and a fifth-century source at the start of Chapter 10, that describe architecture in earlier cities, rather than literary works of the Sui-Tang period.

Small points aside, *Sui-Tang Chang'an* is worthwhile reading for anyone who wants to learn about the urban fabric of imperial China in the seventh through to the ninth centuries. One can envisage a similarly significant companion work on Sui-Tang Luoyang, perhaps by the same author.

Nancy Shatzman Steinhardt

Evil and/or/as the Good: Omnicentrism, Intersubjectivity, and Value Paradox in Tiantai Buddhist Thought. By Brook Ziporyn. pp. viii, 482, bibliography, glossary, index. Cambridge, Mass., and London, Harvard University Press, 2000.

In 1017 (the date of 1016, which Ziporyn prefers, is supported by only a single dubious source) the eminent Chinese monk Siming Zhili (960–1028) together with ten of his companions made a vow. They announced that after three years practising the rituals of the *Lotus* repentance, they would burn their bodies as offerings to the *Lotus sutra*. Later that same year the emperor bestowed on Zhili the purple robe as a particular sign of his esteem for this monk. The Hanlin academician and well-known literatus Yang Yi (974–1020), who had recommended him for that honour, wrote to Zhili in an attempt to dissuade him from carrying out his promise. Zhili would not be talked out of it, but his reply to Yang began a series of letters between the two men, which are still preserved. In the end, Zhili never set fire to himself, and although the reasons why he abandoned his plan remain somewhat obscure, it was apparently not Yang's arguments themselves that convinced him to remain alive. The bestowal of an imperial title as a consequence of a memorial written by Yang to the emperor may well have had something to do with his change of heart. During the course of the lengthy correspondence between the two men, Zhili proposed, in one letter, that there was no distinction between the Buddha and Māra, the demon-king (whom Ziporyn calls "the devil" throughout the book).

This expression of the complete identity between value and anti-value (Buddha and Māra, or good and evil) provides Ziporyn's point of departure for an examination of some aspects of Zhili's thought. But from the very beginning he is too quick to leave behind the actual historical circumstances and to plunge straight into his philosophical quest. He blithely announces on p. 20 that the type of self-immolation proposed by the eminent Tiantai monk "did not involve breaking the monastic rules". Here, I fear Ziporyn is wrong. Had he troubled to consult a recent article on self-immolation which does mention the case of Zhili, he would have learned that in fact self-immolation was the subject of considerable controversy within the Chinese Buddhist tradition, especially as regards the Vinaya (monastic regulations).[1] Zhili's defence of his proposed act rested upon his interpretation of two important apocryphal sutras: the *Fanwang jing*, and the *Shoulengyan jing*, often known as the *Śūraṃgama sūtra* (Ziporyn offers "*Śūraṅgama*", which seems a less plausible reconstruction of the title). It appears to me that much of Zhili's worldview was dependent on influential apocryphal works such as these, and it is a pity that Ziporyn has not taken the opportunity to explore these particular avenues.

There are seven chapters including an introduction and conclusion. The introduction lays out the question of antithetical value as Ziporyn sees it expressed in Zhili's statement, "other than the devil there is no Buddha; other than the Buddha there is no devil". In Chapter Two, Ziporyn turns to the classical Chinese tradition in search of earlier positions on the issues of holism, intersubjectivity, and value paradox. But the chapter contains too many digressions and is not rooted firmly enough in the primary materials to be of much value as a clear exposition of these topics as they appear in the classical texts. Chapter Three, on value and anti-value in Indian Buddhism, is brief and focuses mostly on Nāgārjuna before leaping ahead to make some rather general observations about the influence of the Indian tradition on Zhiyi (538–597), the major thinker of the medieval Tiantai tradition. Chapter Four lays out some of the basic concepts of Tiantai, and is probably the most coherent chapter of the book, although I could not really recommend it as an introduction to Tiantai thought. In Chapter Five, Ziporyn turns to Zhili's perspective on the relationship between unenlightened beings and the buddhas, and in Chapter Six he examines the theories of Zhiyi, Zhanran (711–782) and Zhili on

[1] Benn, James A., "Where Text Meets Flesh: Burning the Body as an 'Apocryphal Practice' in Chinese Buddhism", *History of Religions* 37/4, 1998, pp. 295–322.

value and anti-value, and returns eventually to the content of Zhili's responses to Yang Yi. Chapter Seven provides some conclusions to the book.

The conspicuous absence of anything as empirical (or useful) as a discussion of the social and religious context in which Zhili lived and worked, or Chinese characters for the names of people, places, or texts, makes one suspect that Ziporyn is consciously reacting against some perceived historicist turn in the study of East Asian Buddhism, and is determined to make his work a purely philosophical study. One is obliged to assume that this is the case, since nowhere does he make an open declaration of what he is about. This strikes me as a somewhat dangerous ploy, since he probably only succeeds in irritating and alienating those members of his audience who are interested in the historical development of Tiantai thought, which is to say, those who might be most interested in what he has to say. Indeed, it is very hard to imagine just whom Ziporyn is writing for. Although in the introduction he declares his intention of making a contribution to the fields of Chinese intellectual history, Buddhist studies and philosophy, his approach to any one of these areas is so idiosyncratic and unnecessarily convoluted that it is hard to bring away any of his insights and apply them to other topics. He obviously considers it unnecessary to give a general introduction to Chinese philosophy or to unravel the intricacies of Song dynasty Tiantai thought, which is pretty hard going even for scholars of the earlier Tiantai tradition. We must assume then that Ziporyn's imagined reader is already quite familiar with classical Chinese philosophy, with western philosophy in general, and Hegel in particular, as well as the basic concepts of Buddhist philosophy. It is often difficult to separate which of the ideas within the book belong to medieval Chinese monks, and which to Ziporyn himself. Many of the terms and ideas vital to the understanding of the author's argument are not properly introduced or defined. It is true that there is a glossary of the special terminology that the author deploys throughout the book ("intersubjectivity" and "value paradox," for example), but even in these definitions the concepts defy easy understanding. Other terms coined by the author, such as "Practico-inert Track, or the Truth of Provisional Positing" (p. 184) remain even more obscure.

Ziporyn has a cavalier approach to the conventions of scholarship. His use of diacritics is slapdash. To give but a few examples of many inaccurate renderings in the book: while nirvāṇa is usually given correctly, śūnyatā appears as sunyatā (e.g., p. 18), the name of the Bodhisattva Mañjuśrī is entirely denuded of diacritical marks (p. 19), saṃsāra is rendered saṁsāra (p. 106), and Ratnakūṭa is written incorrectly as Ratnakūta (p. 107). Sources are often not identified clearly. For example, what edition of the *Shijing* (Classic of Poetry) is being cited in n. 25, p. 395? There is no entry for the *Shijing* in the Bibliography, nor, for example, is there an entry for the *Mencius*, thus making the reference to "VI.A.6" on p. 52 unnecessarily cryptic.

The widely accepted convention of providing the titles of primary and secondary Chinese texts with an English translation in brackets has unfortunately not been adopted in this book. Ziporyn's style is prolix, and occasionally just plain odd, as when he says "Marxism is good because I am a proletariat", for example (p. 36). His translations read fairly smoothly, although he appears to have elided difficulties in the original, and he provides no annotation or references for the many allusions found in the texts. Much of the subtlety of Zhili's argument is unfortunately lost or blunted because of this. In short, while this book may be an interesting curiosity for anyone interested in a rather personal late twentieth-century view of some aspects of the thought of an early eleventh-century monk, it is not an indispensable addition to the scholar's shelves.

James A. Benn

OPIUM REGIMES: CHINA, BRITAIN, AND JAPAN, 1839–1952. Edited by TIMOTHY BROOK AND BOB TADASHI WAKABAYASHI. pp. 458. Berkeley, University of California Press, 2000.

Few commodities have been as influential in shaping world history as opium. The history of China in particular was changed by the politics surrounding this highly addictive drug, yet until recently, there have been few studies that concentrate on its history. This outstanding volume of essays by sixteen scholars examines various aspects of the history of opium in China from the decline of the Qing dynasty in the mid-nineteenth century to the rise of the People's Republic over a century later. The volume illustrates certain wide themes. First, it shows the centrality of the opium trade to both the British and Japanese colonial empires, suggesting that without the revenues that the drug brought, financing dreams of conquest would have been much harder, if not impossible. Then, it situates opium directly at the centre of the Chinese nationalist project of the twentieth century, showing how protest against opium could be made part of a wider protest against imperialism and foreign arrogance on Chinese soil. Timothy Brook and Bob Tadashi Wakabayashi use their introduction to define the very useful idea of "opium regimes", whole regional and local systems which either made a profit from the drug or found a common cause in attempting to fight it.

The essays that follow are all based on primary research, often in newly available sources, and thus there are many insights that may not have previously occurred even to well-informed students of imperialism. For instance, the British connection with opium sales is well-known, but David Bello's careful analysis of the parallel trade in Xinjiang in the 1830s provides provocative new evidence for reconsidering opium not just as a Sino-British problem, but as an issue in its relations with the Central Asian states as well. Carl Trocki turns to the topic of opium growing in Southeast Asia, suggesting that in its creation of a demand for a non-essential good, the region's first "modern, industrial-age market" was created. Christopher Munn and Motohiro Kobayashi focus on case-studies which show how imperialism in Hong Kong and Tianjin was underpinned by opium revenues, whereas Gregory Blue and Bob Wakabayashi provide a macro-view of the place of opium in the British and Japanese imperial projects over the length of the late Qing period. Alexander des Forges moves from production to consumption, showing that opium was part of the commodification of leisure that emerged in Chinese cities in the early twentieth century, underpinning contemporary representations of the city.

The following essays all tackle the place of opium in the development of Chinese nationalism. R. Bin Wong takes a long view of the role of the drug in statemaking in Republican China. He argues that opium's role was ultimately pernicious, in that it prevented the state from looking elsewhere for tax revenues, and therefore locked state and society in a destructive embrace that would have been avoided if more foreign loans had been sought instead. Furthermore, China's opium problem gave it the image of a "sick man of Asia" on the world stage, and lessened its ability to negotiate on an equal basis with other powers. Judith Wyman, Joyce Madancy, Edward Slack, and Alan Baumler all focus on individual case studies. They show that different interest groups – local elites in Sichuan and Fujian, urban elites, as well as the Nationalist government itself – could all use opium as a rallying-ground for patriotic fervour, even when allowing the drug to be grown under strict, or not so strict, controls. However, Lucien Bianco ends this section with an analysis of peasant producers of opium which suggests that on the ground, farmers were more likely to protest about attempts to *stop* them growing poppies. This gap between elite anti-opium arguments and mass production and consumption is intriguing, and suggests an area that would repay further research.

Opium's endgame in China is incisively tackled by Timothy Brook and Motohiro Kobayashi, who show that the collaborationist regimes set up by the Japanese in wartime China were significantly, if not totally, dependent on opium revenues to survive. Mark Eykholt then provides a fascinating insight into the way in which students in occupied Nanjing were able to use opposition to opium

sales (implicitly Japanese sales) to protest against the Japanese invasion of their city. Finally, a pathbreaking piece by Zhou Yongming uses newly declassified documents to show that the all-out anti-opium campaigns of the unified People's Republic in 1949–1952 finally managed to end widespread drug consumption in a way that no previous regime had managed.

It is clear that this is a wide-ranging and innovative set of essays. The idea of the opium trade as a trope which fuelled the emergence of nationalist discourse complements other recent studies of ways in which commodified leisure shaped the modern Chinese sense of identity. This volume reads well alongside Gail Hershatter's 1997 book *Dangerous Pleasures: Prostitution and Modernity in Twentieth-Century Shanghai*, which shows how another phenomenon on the edges of legality and respectability, prostitution, was also used to shape modern Chinese ideas about nationalism, policing, disease, and gender. These comparisons also suggest that there are aspects of the opium trade which future researchers could explore further. One of those areas is the *consumption* of opium. The editors of this volume state their own position with regard to the opium trade early on: "Opium was *not* a harmless substance like molasses." Certainly, the real shame and hurt that the trade caused in China is evident not only in these essays, but in the way it is discussed in China today. Yet in our own society, there is room for questioning of the wisdom of "wars on drugs", and there should be room at least to consider the role of opium in China as something other than unmitigated social evil. It is notable that only one essay in this collection really tackles the question of opium from the consumers' point of view. This may be partly a question of sources, but again it is worth remembering that the main reason that the Chinese, like people at other times and in other places, took opium is because it gave them pleasure. That we may disapprove of this behaviour does not mean we should ignore the reasons for it, and perhaps explore those reasons further. Related to this, there could be more explorations of the differing ways in which opium consumption differed among social classes (one fascinating observation is briefly made that it was the upper and lower classes that took the drug, explaining why so much of the outrage against it came from the abstentionist middle classes). There may be a story here to tell about the links between gender roles and opium-taking; with the Muslim-run trade in central Asia, perhaps religious elements could be explored too.

However, these are small quibbles about what is, by any standards, an essential volume for all students of China, imperialism, or political and economic history. Brook and Wakabayashi's book deserves the widest readership possible.

RANA MITTER

DEADLY DREAMS: OPIUM, IMPERIALISM AND THE *ARROW* WAR (1856–1860) IN CHINA. By J.Y. WONG. pp. xx, 542. Cambridge, Cambridge University Press, 1998.

Wong's book is a many-faceted meditation on imperialism. It might have been about any specific incident, and indeed, he has written about others, however, he ultimately makes it clear that this was no minor occurrence. While many may believe that the Lorcha *Arrow* War, or the Second Opium War or the Second Anglo-Chinese War, is an event that might not justify an entire, very large book (542 pages), by the end of it, one may well be convinced that it does. The Second Opium War, as Wong would have us recall it, was a classic example of the many levels of action and meaning that characterized British imperialism in Asia as it was practiced. It is also an event that even today remains somewhat clouded in long-standing, ideologically-rooted contention. If nothing else, Wong has laid many questions to rest with this volume, and has indeed, peeled the onion, layer by layer, to reveal a number of salient, if unpalatable facts about British actions and policies in China in the mid-nineteenth century.

There are eight chapters, each of which may be seen as a layer of the onion. After the introduction

there is one focusing on each of the different aspects of "imperialism" as Wong sees it: its confusion, pretexts, personalities, rhetoric, mechanics, economics and finally its dynamics. Wong is like a lawyer, carefully building his case, one step at a time. The book is a *tour de force* of evidence and argument. Wong has quite thoroughly exploited every available source of information. He has looked carefully at the British, American and French official papers as well as at the private papers of many of the key actors and observers. He has made use of the contemporary newspapers both in Britain and China, to get some idea of the media treatment of the events as well as of public opinion. He has also made extensive use of Chinese sources; again, both official and private. Beyond this, he has covered virtually the entire range of western and Chinese secondary writings on these events.

In addition to examining events that occupy only a four-year period in over 500 pages, Wong presents 59 tables and 19 figures. Although this might seem excessive, his case is a relatively simple one, when all is said and done. Imperialism, in terms of the specific sequence of decisions and the actions of its major players, was usually a muddle, and the reading of this event has been similarly muddled. Thus, it needs some additional effort to untangle the web of deceptions, half-truths and ignorance that characterized the era. On the other hand, behind the confusion of events in China and decisions and policy in London, lay a very determined and orderly array of interests that worked inexorably toward specific ends. In this case, the end was the forcible "opening" of China to British trade and the destruction of China's final effective obstacle to the opium trade. Wong makes it clear that the illegal British trade in opium was ultimately what the war was about. The promotion and protection of the trade was the ultimate reason why Britain fought this war with China. And, since there has been much dispute about the role of opium in this war (the topic rarely surfaces), Wong goes to extensive lengths to demonstrate his case.

The book begins with the various accounts of the actual incident in which the brash young British consul, Harry Parkes, a man with a grudge to settle, presented the apprehension of a ship by Chinese authorities. What actually happened? What did Parkes see and do? What did he say he saw and did to his superior, Sir John Bowring? What did his superiors in London, and ultimately in Parliament say and do and what did they think? Was the vessel really registered legally as a British vessel? Was the flag really flying? Was the British flag insulted? Was the 1841 treaty violated? In the end Wong demonstrates pretty convincingly that the answer to all of these questions was "no". There was, in fact, no real incident. Parkes made it up. Bowring believed him, and lacked the courage to admit it when he realized he had been misled. But in the end, it seems that these facts mattered little, because Lord Palmerston, the British Prime Minister seized on the incident because he wanted the war to protect and promote the opium trade. British public and official opinion was carefully manipulated to close ranks and to support this defence of British international pride and to redress an insult to her flag.

Aside from what did or did not actually happen in the *Arrow* incident, Wong argues that it was necessary to look for more substantial causes. He demonstrates quite convincingly the level of Britain's dependence on the opium trade, and he argues that important groups in London, China and India worked systematically to protect the trade and to lobby for war to advance their interests:

> The present research shows that opium was not just helping to balance the United Kingdom's trade with China. It generated huge profits; it funded imperial expansion and maintenance in India; it provided the much needed silver to develop the trading network among the countries bordering on the Indian Ocean; it assisted the growth of Bombay and other Indian cities; it enabled the United Kingdom to obtain tea and silk from China for very little initial cost: and it was a great help to the United Kingdom's global balance of payments. (pp. 411–12)

Without opium profits, another £10 million would have been added to the UK's annual trade deficit in visible goods. This case was, in fact, argued by lobbyists in 1857 as they pointed out that bills of

exchange on China (payments for opium) were being used to buy US cotton and to pay for British manufactures destined for India.

In the end, much of Wong's argument turns on the "circumstantial" economic evidence. Opium was only rarely mentioned in most of the contemporary discussions in Britain, nor was it raised as an issue with the Chinese. It was the great unspoken issue. Because much has been made of other factors in the British discourse, however, Wong carefully examines the mountains of lies, obfuscation and misdirected rhetoric so that we can understand the nature of the juggernaut that Harry Parkes and Lord Palmerston unleashed on China. The economic evidence is all the more significant, because it was never really mentioned in much of the actual contemporary debate about the war, nor has much been done in recent years to adequately document the proposition. The simple fact that the treaty, which ended this war, also legalised the opium trade with China has thus usually been treated as an afterthought by many historians.

Despite the quality of the research and the writing, there remain a number of niggling infelicities. For all his erudition, Wong seems somewhat ignorant of opium as a drug. He repeats nineteenth century beliefs that smoking opium was far more deleterious and addictive than ingesting it (p. 325). Actually, ingestion is just as addictive and equally dangerous. He repeats the misconception that Commissioner Lin Zexu destroyed the opium he confiscated by burning it (p. 401), when it has been well-established that it was dumped into pits filled with seawater and then covered with quicklime (Fay, 1975). For some reason, perhaps a fault of the editing, the "straits settlements" are often given in lower-case (pp. 401, 402, 405). Finally, I believe he somewhat overstates his case when he claims that the "British absolutely dominated Chinese maritime trade" (p. 383). While this was certainly true in regard to the trade of other European powers, it is hard to imagine that British trade outpaced the commerce carried on by private Chinese traders both in junks and in their own square-rigged vessels.

These are, of course, minor items and in no way detract from the importance of the book's contribution to studies of British imperialism in China. Wong's book has without doubt, now become the standard work on the *Arrow* war and will certainly become mandatory reading for students of Chinese and British imperial history.

Carl Trocki

Chinese Modern: The Heroic and the Quotidian. By Xiaobing Tang. pp. x, 380. Durham, Duke University Press, 2000.

Tang Xiaobing's *Chinese Modern* has received high praise: Ted Huters writes that the volume "represents the most comprehensive account of modern Chinese literature that has ever been published in English." Tang's list of acknowledgements reads as a Who's Who in American sinology, and the volume has rightly been seen as a major contribution to modern Chinese studies. *Chinese Modern* presents a series of highly-focussed stills of literary works or themes, taken at intervals across the twentieth century. Each image is brought into focus through multiple analytical approaches, applied in Part I to the works of four canonical writers, and in Part II of the volume to a broad range of literary and other texts from the PRC era. Tang explains in the introduction that his intention is not to construct a systematic literary history, but to engage with "the uneven, multifocal histories that continually surface" among the texts. The outcome is a volume of collected essays: rich, imaginative and immensely useful for teaching courses, but also one which obliges a reader to negotiate between chapters.

The title *Chinese Modern* is deliberately disturbing: with no article to form a noun phrase it hangs, unfinished, ambiguous. This ambiguity presumably evokes the project of analysing modernity, rather than the volume itself. The paperback edition is presented in ochre tones, with a soviet-style

woodcut from 1940 on the front, depicting burly workers reaching up towards some unseen light source. Their heroic stance in everyday clothing and caps illuminates Tang's central theme: the dialectical movement of the heroic and the quotidian, which ''constitutes an inescapable condition of secular modernity.'' To show this, Tang presents detailed readings of seminal works of literature from roughly each decade until the 1950s, broadening out into studies of film and the city towards the end of the twentieth century. From Chapter 2, published in 1992, through chapters 6, 7, 8, 9, 10 and Excursion II in 1998, earlier essays have been reworked to fit into the theme presented. It is no wonder that Tang concludes his acknowledgements by commenting that the task of revealing the contemporary relevance of the Chinese experience of the twentieth century will demand just as active a part from its readers as the author. The reader is not presented with a traditionally structured monograph, but a series of collected essays, whose themes and interactions demand considerable effort to thread together. The effort is amply rewarded, but it is a huge task to read and engage seriously with the volume and diversity of thought (and particularly of critical -isms) that Tang re/presents

Part I of *Chinese Modern* revisits in chronological order four important literary texts: Wu Jianren's late Qing *Sea of Regret*; Lu Zun's *Diary of a Madman*; Ding Ling's *Shanghai Spring, 1930* and Ba Jin's *Cold Nights*. Chapter Two, on the *Sea of Regret*, examines the conditions for the beginning of a modern literature in China, considering the social and cultural position of the new-style fiction. The theoretical anchoring of each chapter in a relevant body of discourse (economic/political/textual/psychological etc.) shows the immense range of Tang's reading and grasp, but lends the volume as a whole something of the tone of a literary theory reader. In Chapter Three, on Lu Xun, Tang articulates most expressly the condition of Chinese literary modernity. The modern Chinese literary canon has been shaped through hegemonic imposition, with a codified realism rather than modernism as its major trope. As Tang indicates, ''modernism'' has come to mean ''all but realism'' in much contemporary Chinese writing, having been promoted and reviled by the state in turn. Tang states that to reconstruct a Chinese literary history on the same pattern as western modernism would result in a Spivakian subaltern cognitive failure, and that modernism in the Chinese context precisely highlights the absence of a high modernism as experienced in various forms in the West. In this context, the use of the term modern could have done with more explanation: how does it equate to *xiandai*, for example? If China's modern is fundamentally different to western versions, one wonders where the justification lies for extended use of western theoretical models and terms. Will western literary analysis produce anything but a western modernism – even for a western-influenced literature?

The Body and the City form the subject of Chapter 3, where Ding Ling's hero and heroine move through painful transitions in their emotional and political lives, seen through the focal lens of gender. Shanghai itself is the embodiment of western modernity for the youth of the 1930s, where, as Tang writes, the competition between revolution and love reflect two ways of appropriating the city: the collective and the individual. In Chapter Four, the frenzied activity of the 1930s turns to the despair of the 1940s, metaphorically depicted through Ba Jin's tubercular protagonist. The modern subject is skilfully dissected by Tang, who writes at his best in close textual analysis. One might quibble, however, with the footnotes on textual editions (especially proposals for a ''definitive edition'').

Part II of *Chinese Modern* comprises six relatively short chapters and an excursion into interior design. The range of subject matter is much wider than in Part I, moving from The Lyrical Age, through Residual Modernism, History as Spectacle, Cinematic Representations of Beijing and the New Urban Culture, through to Postmodernity in Wang Anyi's *Tale of Sorrow*. The chapters form an exciting, and immensely readable whistle-stop tour of moments in the second half of the twentieth century.

In the first chapter of this section Tang looks at the 1960s play *The Young Generation*, searching out the anxiety and discontent which is present but suppressed by lyrical culture. Lyricism for Tang becomes a strategy to co-ordinate the young, a means of creating a collective identity, but one which, forged on stage, ultimately appears staged. The leap forward to the 1980s in the next chapter is probably the most evident discontinuity in the chronological progression of the volume. Perhaps this is deliberate self-referential textuality: Chapter 6 considers the trope of discontinuity in the rhetoric of postmodernism in the 1980s. In this postrevolutionary, postmodern literature, Tang detects traces of a European modernism, and discusses stories by Yu Hua and Su Tong under the heading Residual Modernism. Chapter 7 takes another of Su Tong's works, and compares it with a novella by the Taiwanese author Xiao Ye. Chapters 8, 9 and Excursion II all consider the cityscape: in Beijing cinema, in urban culture, and in interior decor. In the Afterword to the volume, Tang veers away from the modern into contemplation of the whole: twentieth century literature supersedes ''modern'' as the guiding framework for the volume.

The diffuse nature of Tang's volume underlies its treatment of the modern. Frank Kermode argued as long ago as 1968 that somebody should write a history of the term. ''Modern'' was used interchangeably with ''modernist'' in much early writing, implying a relationship with the past which required critical thinking. While definitions faltered (and shifted between modern, modernist, symbolist, avant-garde etc.) the phenomenon remained closely aligned to British, American and European culture, and related primarily to the first quarter of the twentieth century. Tang misses an opportunity in this otherwise excellent volume to explore the theoretical assumptions underlying the modern project: such theorising is mostly relegated to a section of Chapter Two and musings in the Afterword. Tang had already set out this project in editing an earlier volume *Politics, Ideology and Literary Discourse in Modern China*, where he proposed engaging in ''a comprehensive reexamination of modern Chinese literature . . . against the larger background of the world historical experience of modernity''. While the whole volume of *Chinese Modern* builds up towards an understanding of this ''historical multiplicity or non-synchronous synchronicity that is at the core of Chinese modernity'' (p. 343), it may have helped the reader to have an analysis of modernity in a more easily digestible form. Here there is no explicit re-engagement with modern literature as a western model. Instead the author addresses the issue through a series of readings based on various western -isms. If the reader wants to detect the ways in which Tang is presenting a distinctly *Chinese* modern, s/he has to search and construct it out of the separate chapters. Reference to the West, for example, is omitted from the Afterword where Tang draws together his theorizing on the modern, yet western literary critics and methodologies permeate almost every chapter of his Chinese modern, from psycho-pathological study to Frederic Jameson.

As a volume aimed at widening access to Chinese texts (there are no characters in the text or footnotes) and at producing a strongly theoretical study of the modern and of China, the volume would have gained from more than an eight page Afterword. The structural freedom to make fluid associations between literary writings brings many gains (the text is streaked through with brilliant insights) but its downside is visible in the absence of a conclusion. The coherence of the volume derives from its approaching the literary and cultural trajectories of the twentieth century as an interconnected whole, but the sum of that whole needs comment. In Chapter Nine we learn that the anxiety of everyday life is the bottom line of modernity. The everyday reader senses a similar unease at seeing modernity processed as a postmodern text.

Chloë Starr

THE MAKING OF THE REPUBLICAN CITIZEN. POLITICAL CEREMONIES AND SYMBOLS IN CHINA, 1911–1929. By HENRIETTA HARRISON. pp. viii, 270. Oxford, Oxford University Press, 2000.

Henrietta Harrison's historiography of the Chinese Republic between 1911 and 1929 is an elegant and masterful investigation of the Republic as a symbolic enterprise and ritual parade ground. Her narrative begins in Nanjing with Sun Yatsen's inauguration as China's provisional president on the 1st of January 1912 and ends in Nanjing with his burial ceremony (in fact, the re-interment of his embalmed body) on the 1st of July 1929. Sun Yatsen thus provides the framework for the story, and he is an important figure in the book. His role, however, is not cast in the form of ''the man and his deeds'', but in his use of political symbols and the use others make of him as one. A largely hated and pathetic man who at the lowest point of his political failure decided to make a bold, yet desperate and unrealistic, stab at reconciliation with Duan Qirui's national government, Sun undertook a trip to Beijing in 1925, where he died without achieving anything. His death bed, and later burial, however, were infused with so much political significance that he posthumously grew to immense proportions as the ''Father of the Nation'.' Contemporary references associating him with Lenin, George Washington, and Confucius say more about the complexity of China's politics at the time than about Sun Yatsen as a politician.

Our perception today of Sun as a grand nationalist icon looming over the first half of twentieth-century China is thus largely a product of the way he was used as a symbol after his death. The rituals still performed at certain official occasions in Taiwan, bowing to the white sun flag and Sun's portrait, as well as singing the ''Sanminzhuyi'' anthem, indicate that he is still called upon as a symbol.

Any student of modern China will know that the period from the 1911 Revolution to the founding of the Republic in Nanjing 1928 is difficult. It was a period dominated by military strongmen, warfare, short-lived political and military cliques and alliances, treachery and political assassinations. It was also a period in which the Chinese bourgeoisie arose, and in which modern industries flourished. It was a time of great social upheavals, including huge migrations within and out of China, and an age of intellectual ferment and cultural innovation. The period was influenced by the presence of all the colonial powers in the treaty ports and beyond, with foreign missionaries in all parts of China. The Communist movement took root in this period, and internationally, the First World War (especially the post-war Versailles Treaty) and the Russian Revolution deeply affected China. China's history in the 1910s and 1920s has no established core narrative and tends to crumble into many histories each with its own logic. Each of the many histories constitutes an area of specialised scholarship. Historians of Warlord politics, for example, are bound to sink into the mire of wars, skirmishes, secret alliances, and foibles of individual generals who at most paid lip service to the official government, while scholars of communism are likely to focus more on the party's roots than the soil in which it grew strong.

Henrietta Harrison's book creates its own perspective and establishes a coherence and insight beyond existing historiography on the period. Although Sun Yatsen provides the framework for the narrative, Harrison's analysis goes much deeper, examining how the ''republican citizen'' was made.

By examining the symbolism of nation building, Harrison is able to open up the world of the republic in a credible way. Most history books when discussing this period are likely to mention queue cutting and the abolition of foot binding, but few dwell on their deeper symbolic meanings. The republican flag, the anthem, public national day parades, the introduction of the solar calendar, the official dress code, the shifting fashions, forms of greetings, female hairstyles, musical instruments, military uniforms, town planning, advertising and names and packaging of products like cigarettes, and many other aspects of ritual and everyday culture are brought together to paint a picture of a society in fundamental change. The wide range of contemporary sources from newspapers, diaries

and official archives, and even interviews with people old enough to remember the events, give the book a distinct freshness and immediacy. Personal stories of how individuals accepted or resisted their role as republican citizens, how people participated in or were barred from public ceremonies make history come to life. Everybody used symbols to further their cause, whether it was street vendors setting out small white flags or Christian Chinese asserting themselves as patriots. Officialdom defined itself in relation to sections of society into which the citizenry was divided. The roles of these sections were stereotyped and symbolic, and changed during the two decades; by 1928, they were all subordinated the Nationalist Party of Chiang Kaishek, that thus incorporated the citizenry as if it were one body.

The book deals with variation and change in the way symbols were perceived and used. This perspective on history is very fruitful, for it draws together issues that otherwise seem unrelated or unproblematic and gives them a new significance. In particular for histories of political and military power or economic transformations, the debates on the colours of the national flag, or the text of Sun Yatsen's testament, and even more the events around his death bed, may seem trivial and of marginal relevance. However, in Harrison's account they assume explanatory importance in the history of Machiavellian power. The impromptu popular surge of emotion at Sun's funeral in Beijing (where the masses found an outlet for their protest against the sorry state of the country, forgetting their negative feelings about Sun) forced the hands of politicians, and different political factions scuffled to make use of the situation to their own advantage; Sun emerged as a symbol outside the control of anybody. When his body was re-interred three years later, Sun's symbolic power was harnessed by the Nationalist Party under Chiang Kaishek to the exclusion of any spontaneity. Chiang's claim to power was not merely a question of military prowess, but to a large extent depended on his ability to manipulate the symbolic value of Sun and link himself to Sun as a political heir.

Sun represented both modernity and historical legitimacy. As a leader of a modern revolutionary party, normally impeccably dressed in western suits, introducing the solar calendar and doing away with many practices of the past, Sun's political aspiration had been to place China on an equal footing with other nations of the world. He claimed ethnic Chinese historical origins by paying public respect to the tomb of the first Ming emperor, thereby not only venerating the last Chinese leader to overturn a barbarian regime (the Mongolian Yuan Dynasty), but also pointing at the ethnic continuity with the last Chinese dynasty before the Manchu Qing Dynasty he himself had helped overturn. Chiang Kaishek, in his bid for power had Sun Yatsen's body re-interred next to the first Ming Emperor outside Nanjing in a mausoleum even greater than the emperor's. He had a long road built that led up to the monument cutting through Nanjing, entering the vast walled city area at the railway station in the north and leaving through a city gate in the far south east of the city. This new broad western-style avenue, Zhongshan Road, named after Sun Yatsen, was used for the parade that brought Sun's hearse to the mausoleum, formed the centre piece of Nanjing's transformation into a modern city, and linked monuments and buildings of symbolic significance.

Henrietta Harrison's account gives new meaning to many events in the period covered by disclosing the interplay between political manipulation of symbols, popular tastes and identification. Her exemplary and painstaking research is presented in a delightful and highly readable style. One has the feeling that all details are covered by sources, and that Harrison has found no need to make conjectures for which the evidence was not strong enough. Exactly for this reason the book is not, and does not pretend to be, a full history of China during those two decades, for it only discusses important issues such as warlord strife, foreign presence, the role of the Communists, and intellectual debates in as far as and only to the extent that these are essential for the use of political symbols by the protagonists of early Chinese republican politics and the people during their transformation to citizens.

Flemming Christiansen

READING THE *CHUANG-TZU* IN THE T'ANG DYNASTY: THE COMMENTARY OF CH'ENG HSÜAN-YING (FL. 631–52). By SHIYU YU. pp. xiv, 209, bibliography, index. Bern, Peter Lang, 2000.

In the Taoist tradition, the *Zhuangzi* (*Chuang-tzu*) was often regarded as a classic second only to the Laozi's *Daode jing*. But given the concentration of scholarship on other works which fill the Taoist canon it has been hard to appreciate what this actually meant for Taoist believers. Indeed, when the late Michel Strickmann taught "History of Taoism" at Berkeley in the early 1980s, his syllabus announced that since the course concentrated on the history of Taoism from the Han to the present day, only passing reference would be made to the two pre-Han "Taoist" works of Laozi and Zhuangzi. With the publication of this study of a Tang Taoist commentary on the *Zhuangzi*, it will no longer be quite so easy to bracket the questions of the influence and importance of the text. Despite its problems, some of which I shall bring up below, this is actually rather a good book, on a topic which well deserves the attention of scholars interested in Taoism and the intellectual climate of the Tang.

The work in question is that of Cheng Xuanying, an early Tang Taoist priest. His commentary is extensive, and is much broader and more discursive than earlier philological commentaries, such as that of Lu Deming (556–627). In the early Tang, Taoism had legitimacy as a political ideology of the imperial Li clan, and as a consequence Cheng was writing from a position of some confidence. Although we can see the *Zhuangzi*'s language and ideas adduced in earlier medieval Taoist scriptures, Cheng's commentary was really the first to take on the text from a religious point of view, and especially from the perspective of religious practice. Because of the time when it was written, the Buddhist influence on Cheng's thought is very clearly discernable. In a way, Cheng's commentary is a meeting point for philosophical or classical "Taoist" concepts, the ideas of some Indian Buddhist texts as seen through the lens of influential medieval Chinese Buddhist thinkers, and the ideals and beliefs of one particular medieval Taoist practitioner. The way in which these systems and worldviews are brought together may give us some very useful insight into the eclectic nature of Tang intellectual life in general.

Yu's study of Cheng's commentary is in six fairly brief chapters, plus an Introduction. Chapter One discusses the various recensions of the *Zhuangzi*, and how the text was studied, debated and classified, from the Han to the Tang, when it was accorded the official title *Nanhua zhenjing* (True Scripture of the Southern Florescence). This is a very useful and thorough survey which provides much of the background necessary for the later discussion. It is interesting to note that medieval Taoists were more than a little wary of appropriating a pre-Qin text such as the *Zhuangzi* as an exclusively Taoist work. As Yu points out, Tao Hongjing (456–536), who compiled a bibliography of Taoist scriptures, seems to have been rather vague when it came to the status of the text (p. 21). In another work, he ranked the *Zhuangzi* as one of the three great teachings, alongside the scriptures of the Shanqing (Upper Clarity) revelations and the Buddhist *Lotus Sutra*. While the *Zhuangzi* probably never fell out of fashion entirely, it experienced a marked vogue from the Liang (502–558) dynasty onwards. From this point on we start to see the text take its place as second only to the *Laozi* among the Taoists, and it also attracted the opprobrium of some Buddhists, particularly Jizang (540–623).

Chapter Two teases out the life and times of Cheng Xuanying from the rather meagre information found in the extant historical texts. Here Yu makes impressive use of geographical sources in order to determine what sort of place Cheng came from, and the circumstances of his formative years. The care that he has devoted to his materials in this chapter shows just what a deep understanding Yu has of the actual terrain against which the religious history of medieval China was played out. Rather interestingly, given Cheng's knowledge of Buddhism, he appears to have worked alongside the famous Buddhist pilgrim and translator Xuanzang (602–64) in translating the *Laozi* into Sanskrit

(p. 56). Around 652, Cheng was banished from the capital, although we do not know the reason for this, and he apparently composed the commentary while in exile. Unfortunately, we know almost nothing about this period of his life.

Chapter Three focuses on the crucial concept of "double mystery" (*chongxuan*) as applied, and to some extent reinterpreted, by Cheng in his commentary. In this chapter, Yu shows that he has mastered the work of French, Japanese, and Chinese scholars on the "double mystery" in commentaries on the *Laozi* and he goes on to add his own views of Cheng's imaginative search for this concept in the *Zhuangzi*, a very different text. Chapter Four looks at Cheng's view of the void, and the Taoist ideal of transcendence, for which the *Zhuangzi* was always an important source of inspiration and terminology. Yu understands Cheng's interpretation of the void as expressing the personal experience of the "double mystery" as discussed in the previous chapter.

Chapter Five places Cheng's commentary in the context of contemporary debates between Buddhists and Taoists in the early Tang. How did Cheng use the *Zhuangzi* to support his vision of the medieval Taoist church? As Yu reveals, he clearly knew a lot about Buddhism and he used this knowledge rather skilfully in his interpretation of what he saw as the main ideas of the text. Chapter Six acts as a conclusion and is devoted to an appreciation of Cheng's distinctive writing style, especially the manner in which he works Buddhist terms and concepts back into the structure of a pre-Qin work. As Yu says, Cheng blurs the distance between commentary and text in such a way that he appears to speak from within the *Zhuangzi* itself (p. 169). Yu offers a convincing explanation for why Cheng's commentary is so distinctive by tracing it to the styles of lecturing on Buddhist sutras and Taoist scriptures in the medieval period.

Despite the author's considerable erudition and his sensitive approach to the text there are some problems with this book that cannot be ignored. It seems to me that these are nearly all due to premature publication. This would have been a very good book indeed if it had been copy-edited, checked again by a native speaker of English, and if perhaps it had been shown to some other scholars of medieval China. It is a great shame that the book should have gone to press with so much of it marred by poor English. Often the wrong word is chosen, as for example where "repelled" is written, when obviously "exiled" is what is meant (p. 46). Odd locutions such as "stringent similarity" (p. 8), "hand on" instead of "hand in" (p. 20), or "scholarly prowess flashes up abundantly" (p. 30) appear far too frequently. In many places there is an unintentionally comic effect that rather detracts from the serious point that Yu is trying to make. For example, "the fragrance remaining on the unfinished cake that has endless benefit" (p. 29), while it does render the Chinese, looks like it was produced during a round of the surrealist parlour game "Exquisite Corpse". I would not wish to embarrass the author by pointing out other examples, but there are many, and these could all have been so easily avoided had a kindly editor intervened. Chinese characters appear liberally and usefully throughout the book in the body of the text, although they are occasionally inexplicably absent, as in the name of Zhou Yong (fl. 465–82) on p. 31. It may be redundant to list typographical errors in the face of the greater problems with language, but "Kuo Hsaing" should be "Kuo Hsiang" (p. 25), "Tai" p. 60 n. 3 should probably read "Taipei", and "posts" on p. 70 would make better sense as "posits". Overall, Yu's translations from his primary sources deserve praise. They are well chosen, and convey the meaning pretty well, despite the odd clumsy turn of phrase here and there.

More seriously a number of very important works in English do not appear to have been consulted. The most baffling omission is any reference to T.H. Barrett's *Taoism under the T'ang: Religion and Empire during the Golden Age of Chinese History* (London, 1996), which one would have thought might have been required reading for anyone writing on Tang Taoism. There are places in the book where a familiarity with Barrett's earlier, *Li Ao: Buddhist, Taoist or Neo-Confucian?* (Oxford and New York,

1992), would also have been useful. Despite its shortcomings, this is a book that should be welcomed by all scholars of medieval China, and I look forward to further works by the author

JAMES A. BENN

AUTOCRACY AND CHINA'S REBEL FOUNDING EMPERORS. COMPARING CHAIRMAN MAO AND MING TAIZU. By ANITA ANDREW and JOHN RAPP. pp. xiii, 360. Maryland, Rowman & Littlefield, 2000.

The two authors of this book, a historian who specializes on Ming China and a political scientist who appears to focus on modern Chinese political theory, have joined together to compare the careers and personalities of Mao Zedong and the first Emperor of the Ming dynasty. They are aware of but tend to minimize the essential differences between the two men. Ming Taizu did not seek to change the existing social system while Mao attempted to destroy it. The first Ming Emperor, determined to avert a renewal of foreign rule as in the previously-governed Yuan dynasty, simply imposed a tighter grip on the population but did not alter the imperial structure. On the other hand, Mao offered a new ideology which tried to sweep away the traditional institutions.

The comparisons the authors proffer could apply as well to founders of any new governmental system. They assert that both Mao and Zhu originated as leaders of peasant rebellions and thus distrusted bureaucrats and intellectuals. These characteristics are actually not unique to Mao and Zhu, as many rulers in China have come to power with the assistance of peasant rebellions. According to the two writers, both Mao and Zhu initiated purges of top officials and imposed onerous state control over the rural areas. Such policies characterize other Emperors. In addition, the authors' focus on similarities underestimates the considerable differences between the practices and objectives of the two leaders. Comparative history ought to provide insight and real links between the institutions or people compared, but this work concludes with observations that are not particularly illuminating.

It may be useful to have the translations the authors provide in one volume, but they have, after all, been published elsewhere. It is not clear why they were reprinted here except that the work, which comes to about ninety pages, would not otherwise be a book-size.

MORRIS ROSSABI

THE ORIGINS OF THE CHOSŎN DYNASTY. By JOHN B. DUNCAN. pp. xii, 395, Tables, Geneological Charts. Seattle and London, University of Washington Press, 2000.

This seminal work by John B. Duncan of the University of California at Los Angeles has been given a title which might mislead the reader at first. This book is not just a look at the early years of the last dynasty of Korea, the Chosŏn Dynasty (1392–1910) but is a fresh examination of the development of the ruling class of mediaeval Korea during the thousand-year period of the Koryŏ (918–1392) and Chosŏn kingdoms. Moreover, it is at the same time a critical examination of the many different types of theories which had been developed by twentieth-century Korean historians in response to the experience of the collapse of the Chosŏn state and the annexation of Korea by Japan from 1910 to 1945. The issue of the nature and structure of the Chosŏn state was examined throughout the twentieth century in the light of the question of Japanese domination of the nation and Korea's perceived economic, social and political stagnation. Various schools of Korean historical thought emerged including nationalistic historians, positivist (documentary) historians and socio-economic historians who all attempted to explain the reasons for Korea's economic and political state at the end of the nineteenth and early twentieth centuries. By the 1960s, a school of internal development historians emerged which took a critical look at certain crucial and transitional points in Korean

history. These historians viewed the Koryŏ/Chosŏn transition era as the result of the emergence of a new class which effectively replaced the ruling class of the Koryŏ kingdom. This book takes issue with that point of view and argues instead that the difficulties which were evident in the late Koryŏ political establishment in fact are traceable to problems arising at the time of the founding of the dynasty itself. Although there were obvious tensions between the local and central aristocracies throughout the dynasty which became intensified towards its end, Duncan argues that the drive for change in the Koryŏ era came from within the central or grand aristocracy, referred to by the term *yangban*. The changes which took place at the time of the transition from the Koryŏ state to the Chosŏn kingdom were focussed around the interests of the *yangban* who were at the centre of the state. Rather than being a radical change in classes, Duncan argues that the transition from the Koryŏ to the Chosŏn was a "shift" within the existing socio-political arrangements. The text is richly illustrated with charts and tables and consists of seven chapters discussing in turn the political system of the Koryŏ period, the rise of a centralised bureaucracy, the position of the *yangban* during the period of transition, the institutional crisis of the late Koryŏ period, the pressure for reform leading to dynastic change, the ideological concepts of the reformists who led the movement which eventually culminated in the change in ruling houses, and some general considerations of the issues raised throughout the book. This book is well written and should be read by anyone with an interest in pre-modern East Asian history, and could be read with profit by anyone who is interested in the socio-political processes of non-western societies. It is one of a growing number of western-language publications exploring the character of the under-researched Koryŏ period and thus deserves the attention of the serious reader.

James H. Grayson

Nakae Toju (1608–1648) et Kumazawa Banzan (1619–1691). Deux penseurs de l'epoque d'Edo. By Jean-François Soum. pp. 505. Paris, Collège de France, Institut des Hautes Études Japonaises, 2000.

One has to admire Soum's courage in tackling not one, but two Japanese Confucian thinkers, who left a sizable *oeuvre*, and who are both the objects of a voluminous Japanese literature. Soum is, moreover, not content to treat his two thinkers in the ancient format of parallel lives; he uses several chapters to analyse the influences Tōju exerted on Banzan, and on the influences both exerted on subsequent Japanese thought. By any conventional standards, Soum has done a good job. His book is competent and thorough, it is based on solid research, the author is the master of his sources and of the secondary literature, and his translations are on the whole both readable and reliable. As Soum provides the original text of all Japanese and Chinese quotations in the footnotes, it is, moreover, easy for the reader to check them. The book also contains a *bibliographie raisonnée*, in which Soum treats all previous studies of Tōju and Banzan in Japanese and in western languages. All in all, the book can be safely recommended to anyone who has developed an interest in the life and thought of Tōju and Banzan

The book, however, has its problems. One of these is the lack of references to different (or concurring) views of other researchers. I may have missed one or two, but by my count, the number of notes referring to modern scholars is thirteen on a total of 469. Of these only two take direct issue with alternative opinions regarding the matter under discussion, the rest are references of the "see also" type, and suggestions for further reading. Soum may argue that the reader will find extensive synopses of other researchers' views in the bibliography, together with his comments. Nevertheless, the better practice still remains to cite the opinions of other scholars at the appropriate place and to state as part of one's argument why one agrees or disagrees with them. Another facet of this approach

as we see, for example, in the section "La diffusion populaire des 'études du cœur'" (pp. 133–7). In this section, Soum gives one reference (to the source of a quotation), but for the rest, he leaves it to the reader to figure out that most of what he is saying in these pages will be a summary of the sources and studies he mentions on pp. 409–10.

There are other incidental problems, especially with his interpretative tools and vocabulary, and with faulty translations some of which result in a serious misrepresentation of the author's intent. One instance is his translation of the phrase *zenkōno shinpō o yoku juyō sureba* (p. 112). Soum translates this as "être bien imprégné des règles spirituelles qui gouvernent la vertu filiale dans sa totalité." One might quarrel about the translation of *zenkō* (is it really, "filial piety in all aspects", or should "total piety" be defined as the substance that underlies them, on the *tai-yū* model?), but "règles spirituelles" is in any case wrong. "*Shinpō*", the "Method of Mind", refers to psychological techniques, mental disciplines that one should practice in order to get a grip on one's mind, to concentrate it, and to make it behave as it should. One of the vexing things is that our authors do not bother to explain in any sort of concrete detail what these techniques consists of, but that should not mean that one settles for such translations as "spiritual rules". "Discipline mentale", which Soum uses on p. 149, is much better. But he wavers in his translation of this key concept, for a few pages further down, we find again "règles d'esprit" (p. 156). Finally, "to be impregnated with" is an insufficient translation of the word *juyō*. *Juyō* means "to accept and put to use". Although the word has received little attention, it is a key term in at least seventeenth-century Confucian texts. It implies that tenets should not merely be accepted in the sense of "recognized as true or appropriate", but that they must also be practised. *Juyō* implies hard work on the part of the adept who does the accepting, which nuance is absent from the passive "to be impregnated with".

Similarly too little emphasis is given to the effort a Confucian adept has to make, we find in Soum's explanation of *meitoku* ("luminous virtue") (p. 106). In the quotation, Tōju says that "(To have to take all these various things into account may seem difficult, but) in the end they are all subsumed in one thing: to make luminous one's luminous virtue. If only one's luminous virtue is [made] luminous, the distinctions as to time, place, and position, one's duties as a man, and the rulings of fate are all as [immediately clear as] when a mirror reflects shapes." In this case, I have no objections against Soum's translation; it is fine as it is. I cannot, however, agree with his interpretation. "Luminous virtue" and the mirror are both well-known metaphors of *shin* (*kokoro*, the Mind, the Heart). The gist of the quoted passage is, that if the adept has gone through the required discipline of *shinpō* and has cleared his Heart of all clouds of egoism and desire, his reactions will always and automatically be the correct response to whatever situation he is confronted with, just as a clear mirror will immediately render all shapes exactly as they present themselves to it. The quotation does *not* mean, that "there exists no truth outside the mind, and that the pursuit of this truth constitutes the final goal of one's existence." Neither does Tōju claim, that all human actions "are submitted to the exigencies of moral purity".

Soum has insufficiently grasped the fact that Confucianism's first and foremost concern is with *action – action* in the sense of an unmediated, direct and correct response to external stimuli. How "correct" was to be defined, whether as conformity to preexistent, objective rules ("but these rules are ancient, and made in and for different times") or as validated by the spontaneity of the actions themselves ("but then no holds are barred"), is in this perspective a secondary issue, even though it was the issue on which, for example, Wang Yangming (1472–1529) divided from Zhu Xi (1130–1200). Therefore, to regard Confucianism as a "moral teaching" (*dōtoku shisō*) is a wrong, or at best a very partial, view.

Soum, however, is very much a captive of this view. Witness the argument he deploys in connection with Tōju's commentary on the tenth book of the *Analects*, his *Rongo kyōtō keimō yokuden*. This book, Soum says (p. 67), describes Confucius's "habits and *ritual* behaviour"; that is the reason

why Tōju selected this book on which to comment. Tōju's commentary is remarkable, amongst other things, because it is a "pursuit of a *religious* sense"; Tōju "insists on the *spiritual* significance of *ceremonial*". Tōju also "affirms that *morality* is innate, and that *the knowledge of good and evil* is inscribed in the heart". I submit that, if one wants to make sense of Confucian thought, one cannot move in a few short paragraphs from "ritual" through "ceremonial" to "religion" to "morality". For us, these words and concepts may belong to the same context and naturally suggest each other, but that is our culturally determined bias. In a Confucian context, these words either make no sense ("religion"), or to the extent that they do, the Chinese terms they translate belong to lexical networks that have a completely different structure.

Further down (p. 127), Soum makes a connection between the need Tōju perceived to secure an independent and objective basis for his morality and his belief in Taiitsu. It is one of the most interesting passages of the book, and, even though I think that Soum's solution is incorrect, I am glad he raised the point. His argument is that moral rules need an objective basis, c.q., that it must be possible to validate moral conduct objectively. This is only common sense. One cannot have people running around doing all manner of things, and saying that whatever they do is morally justified because this is what they feel like doing at the moment. Next, Soum states that in orthodox Neo-Confucianism of the Cheng-Zhu persuasion, these objective standards were contained in the Classics; that with Wang Yangming, the criterion was, whether "knowledge was practised"; and that in the case of Tōju, a "supreme deity, elevated to the position of judge of good and evil", acted as a check on man's conduct. In regard to these contentions of Soum, I would like to say two things:

(1) In the case of Wang Yangming, it is incorrect to connect the validation of human conduct with his idea of "knowledge and action are one" (*zhixing he yi*). What in Yangming's eyes guaranteed the correctness of human conduct was "innate knowledge" (*liangzhi*), which was a function of human nature and common to all. Actions that emanated directly from the intuitive insights of this "innate knowledge", would necessarily be correct. "Knowledge and action are one", I think, is a slogan that belongs in a different polemical context.

(2) Did Tōju really create, or believe in a "supreme god" who "judged good and evil"? I doubt it; one can take a different view of the correct interpretation of the second quotation on p. 121, but I think a good case can be made to the effect that he purposely refrained from doing precisely that. In any event, the move would not have solved Tōju's problem. A god who only judges after the (f)act, without having had the kindness of handing out a prescriptive set of rules first, cannot act as a guide to correct moral conduct, but can only be the object of fear or of benign neglect. Tōju validated conduct in terms of the sincerity and unselfishness with which a course of conduct was decided upon (see Soum's discussion of *ken*, especially the second quotation on p. 103), and apparently he assumed that man in general would be able to display this sincerity and unselfishness (see the discussion of *meitoku*: pp. 103–6). Furthermore, he judged actions in terms of their results: have they brought peace of mind? (See the first quotation on p. 125.) Which means, that we still do not know what Tōju needed his Taiitsu for.

The main problem, however, concerns Soum's decision regarding the sheer scope of his book. Anyone who is not conversant with the field might wonder why Soum selected these two particular thinkers for treatment in one single volume. The reason is, of course, that scholarly convention has labelled them the first two representatives of the "*Yōmei-gakuha*", the school of Confucian thought that authenticated itself by invoking the Chinese Confucian thinker Wang Yangming (J. Ō Yōmei). In this sense, Soum's decision is a curtsy to convention, for he himself denies the reality of a *Yōmei-gakuha* in Japan (pp. 129–31). This would have pulled the props from under the whole project, if it would not have been possible to construct other, direct links between the two protagonists. Fortunately, this can be done: Banzan met Tōju in 1641 and studied at his school for eight months; after that, they corresponded. There is, therefore, a *prima facie* case for regarding them as master and

disciple. Banzan himself acknowledges as much, though in a rather involuted way, when he claims that he is heir, not to any specific teachings of his Master, but to his ambitions and motivation (quotation on p. 141). ''To us, nowadays, his work appears as a supplement to that of his master'' (''son œuvre nous apparaît aujourd'hui comme complémentaire de celle de son maître''; p. 142), is how Soum summarizes the situation. It is a good phrase, though it obscures the fact that there is no evidence of a conscious, organised effort on Banzan's part to cover those subjects his master had neglected. In view of Banzan's remark, which he made to defend himself against other disciples of Tōju who attacked him for being untrue to the Master's teachings, Banzan apparently found it important to be regarded as a disciple of Tōju. In view, however, of the many differences between the two which Soum himself points out at length (Ch. XI, especially pp. 154–7), one wonders whether this relation in itself warrants treatment of the two in one book.

The problem is exacerbated by the structure of the book, which is straightforward, but also very traditional. It is divided into three parts. Part One contains three biographical chapters, mainly about Banzan. Part Two begins with Tōju's intellectual biography (two chapters), treats several aspects of his thought in four more chapters, and concludes with a chapter on his posthumous influence. Part Three begins with an introductory exploration of Banzan's thought as compared to that of Tōju, and in the following seven chapters Soum explores various fields of Banzan's intellectual endeavours. The final chapter, but for the Conclusions, discusses the influence Banzan exerted on later thinkers.

The analysis of Banzan's thought is especially hard going. It is in the approved text book style: ''On subject X, Banzan thought this-and-this, as is apparent from the following quotations.'' These quotations, of course, do not prove the correctness of the introductory statement, but only illustrate and develop it, and in the selection of the quotations, no consideration is given to the variety of texts and contexts in which Banzan originally formulated his ideas. Soum sees the problem, when he states that his distinction of Banzan's thought into various categories does injustice to the unity of vision that informed all of Banzan's writings – Banzan, for whom ''ethics, culture, politics, and economics'' were parts of ''a general, unified vision of the world''. Here, instead of giving the lame excuse that ''modern cleavages between disciplines'' have forced him to make the distinctions in the interest of ''clarifying his exposition'' (pp. 187–8), Soum should have made more of an effort to project that vision.

With Tōju, Soum fares much better, but here he has chosen three categories (individual, intuitive judgement (*ken*) made in view of the particular circumstances obtaining in each case *versus* adherence to the literal text of the Confucian Classics; filial piety; religious attitude) that conform chronologically to Tōju's intellectual development.

It is, of course, a very difficult task to conceptualize and formulate one's interpretation of another thinker. The further this thinker is removed from one's own time, and the less conversant one is with his times and *milieu*, the more formidable the effort will be. Modern academic practice, with its stress on quickly finishing PhDs and producing publication lists, is not conducive to the kind of sustained and unrelenting concentration such an effort requires. Soum may, sensibly, have settled for what is the best course under these circumstances: publish what you have, mark out your territory, and fill in the blanks later.

W. J. Boot

From Austere ''wabi'' to Golden ''wabi'': Philosophical and Aesthetic Aspects of ''wabi'' in the Way of Tea. By Minna Torniainen. pp. 330. Helsinki, Finnish Oriental Society, 2000.

The master said: by Nature, men are nearly alike; by practice, they become far apart.

-Analects

Japanese tea culture (*chanoyu*) is not well represented in English-language scholarship. Though tea has been a powerful force in the intersecting worlds of art connoisseurship, manners and etiquette, politics, and attempts to define a national cultural identity, little is understood of its development. Jargon-laden studies useful only to tea initiates have been common, as have ideological tracts written by tea school insiders posing as objective researchers. Several recent publications have avoided these pitfalls, but the necessarily limited scope of such critical works has meant that the ''heart'' of tea culture continues to elude the casual observer.[1]

It was with great anticipation, then, that I picked up Minna Torniainen's work, *From Austere* wabi *to Golden* wabi: *Philosophical and Aesthetic Aspects of* wabi *in the Way of Tea*. ''Wabi'' (rusticity), after all, is said to be the core concept of tea practice. This nebulous term has also come to have great resonance in contemporary notions of ''Japaneseness''. I have heard everything from modern garden design and urban restaurant décor to comic book plots and film techniques explained with reference to *wabi* in Japan. Furthermore, as an amateur potter and confessed craft-fair addict, I have encountered artists in Europe and North America with little or no understanding of the Japanese language using the word to describe their own creative *raison d'être*. The diversity of modern appropriations of the *wabi* aesthetic hints at a rich history that is ripe for critical investigation, particularly an excavation of the discursive layers deposited by generations of artists, tea practitioners, and writers.

Tornianen's study (a University of Helsinki doctoral thesis) is detailed, well-organized, and articulate. She shows great ability in reading, understanding, and translating her premodern texts. As a result, this volume will serve the devoted student of tea history well with its many citations and translations of *wabi*-related passages. Theoretically, however, it is lacking in direction. The book is completely intellectually self-contained, containing no references to, say, European or American studies of aesthetics or philosophy (which I understand are rather fertile fields). Likewise, we find no mention of recent (or even not so recent) works in literary, social, or anthropological theory. The absence of obfuscatory jargon and name-dropping is of course laudable, but I wonder if some familiarity with recent scholarly trends wouldn't have endowed this work with greater focus and appeal.

Contents and Argument

Torniainen has arranged her study into seven chapters. In the first chapter, ''Introduction'', she begins with a history of tea that somewhat oddly ends with Sen no Rikyū. The author then succinctly summarizes the goal, sources, and specialized terminology of her study.

In the second chapter, she considers *wabi* as a philosophical concept, focusing on the influence of Zen Buddhism. She argues that the *wabi* mind/heart has several distinct characteristics. The first is ''absolute freedom of the heart'' (*jiyū*), which she understands to mean freedom from attachments resulting from satisfaction in material insufficiency. This is followed by the notion of the ''straightforward heart'' (*jikishin*), which Torniainen (following Hisamatsu Shin'ichi) reads as earnest devotion with an open mind. Also important is ''candour'' (*shōjiki*), the state of mind required to understand the essence of things. Another key element is ''sincerity'' (*makoto*) in the sense of the essential nature

[1] Particularly notable are Louise Cort, *Shigaraki, Potters' Valley* (Kodansha, 1979); Christine M.E. Guth, *Art, Tea, and Industry: Masuda Takashi and the Mitsui Circle* (Princeton, 1993); and Patricia Graham, *Tea of the Sages: the Art of Sencha* (Hawaii, 1998). Also useful are the unpublished Ph.D. theses, Robert Kramer, ''The Tea Cult in History'', (Ph.D. diss., University of Chicago, 1985); Julia Nakano-Holmes, 'Furuta Oribe: Iconoclastic Guardian of Chanoyu Tradition'', (Ph.D. diss., University of Hawai'i, 1995), James Henry Holland II, ''Allusion, Performance, and Status: The Social and Aesthetic World of Elite Practitioners of the Japanese Tea Ceremony'', (Ph.D. diss., Cornell University, 1997). Only the third of these theses is scheduled to be rewritten and published as a book.

of the human, which arises from the pure world of the Buddha. This in turn is connected to the concept of the "true nature of the Buddha" (*busshō*), which Torniainen understands to be equivalent to the notion of the *wabi* mind/heart. She argues that this state can only be reached through spiritual training (*shugyō*). Achieving Buddha nature and the *wabi* mind/heart leaves one in the state of "naturalness" (*tennen*), a condition of pure spontaneity and sincerity.

The author explores how these concepts surfaced in the writings of actual tea practitioners in her third chapter, focusing on the oft-discussed Takeno Jōō (1502–55 AD) and Sen no Rikyū (1522–91 AD). She examines similarities between *General Rules of Poetic Composition* [*Eiga Taigai*] (1216 AD) by the court poet and critic Fujiwara Teika (1162–1241 AD) and teachings ascribed to Jōō in *Record of Yamanoue Sōji* [*Yamonoue Sōjiki*], written by the tea practitioner Yamanoue Sōji (1544–90 AD), as well as the development of Rikyū's notion of *wabi* tea as a spiritual pursuit.

In her fourth chapter the author turns to *wabi* as an aesthetic concept applied to tea utensils, tea houses, and tea gardens. She argues that the *wabi* tea practitioner was portrayed as poor and owned neither famed utensils (*meibutsu*) nor Chinese utensils (*karamono*). Usually rough, domestically produced utensils (*wamono*) were considered to best convey the sense of *wabi*, though some objects imported from the Korean peninsula and China expressed the aesthetic as well. In terms of tea rooms, the author demonstrates that *wabi* referred to small spaces, usually less than 4.5 mats, with special features such as earthen walls, areas of exposed-roof ceiling, and a wood-floored decorative alcove. In the tea garden, *wabi* emerges as an ideal of "barren soil" and a general lack of bright colours as would be produced by flowers or maple trees. She concludes the chapter by examining tea diaries (*chakaiki*) and orally transmitted anecdotes to better understand Jōō's and Rikyū's "sense of *wabi*."

The fifth chapter is short and out of place. In it, Torniainen examines the use of the term *wabi* in classical literature, particularly poetry collections. This section would have been useful at the beginning of the study. The author then turns to a comparison of two works by early Kamakura period (1185–1333) poets, and how these poems supposedly shed light on Jōō's and Rikyū's understandings of *wabi*. The comparison is extremely tenuous and contributes nothing to the argument of the book.

In the final chapter, the author briefly considers different manifestations of *wabi* beauty: luxury, golden *wabi*, chill, calmness, austerity, and absolute subtleness. This implies, to this reader at least, that the concept is polyvalent in application and connotation. Ending with this section certainly ties the study together nicely, though emphasizing the multiplicity of meaning of *wabi* throughout might have been a successful approach as well.

"By Practice, They Become Far Apart"

A few small but serious factual errors need to be mentioned. The three Sen houses are not all located on Ogawa Street in Kyoto (p. 14); since its founding in the seventeenth century, Mushanokōji Senke has been located on a street of the same name, two blocks south of Imadegawa street. Furthermore, the word "*chadō*" may be more commonly used by members of the Urasenke school of tea (p. 36), but this is not necessarily the case among other students and practitioners. Many Omotesenke followers prefer "*sadō*," and most academics not affiliated with either school use "*chanoyu*," born out by the name of our only independent academic association: the Chanoyu Bunka Gakkai [Tea Culture Society]. Kobori Enshū was not one of Rikyū's disciples (p. 147), but studied under Furuta Oribe, who did count himself as one of Rikyu's students. The statement that "Raku was purely Rikyū's innovation" (p. 233), though heard often enough from tea practitioners, is completely unsupported by the historical evidence. These are small mistakes of the sort often found in doctoral theses, which do not adversely affect the work as a whole.

As mentioned above, the lack of a theoretical framework is the biggest weakness of this volume. A

connected problem is Torniainen's methodology. She bases the study almost entirely on texts printed in the series *Complete Collection of Tea Classics* [*Chadō koten zenshū*]. Though comprehensive at the time of publication in 1956, it has since been shown to be riddled with errors; originals and more recent transcriptions should have been consulted if possible. Moreover, several recently discovered and/or published texts must be considered in any serious discussion of the development of *wabi* tea. Also worrisome is the discrepancy between the author's ostensible concern with the reliability of her primary sources in certain passages (such as the discussion of tea diaries in Chapter four) and her willingness to draw from highly problematic texts such as *Nampōroku* in others.

The hermeneutic isolation of this book implies, *ipso facto*, that it was written for tea initiates. With such readers in mind, it is understandable that the author presupposes the universal interest and relevance of *wabi*; assumes that Rikyū is the last and most important person in tea history; and is unconcerned by the questionable reliability of many of her sources. Read alongside other books written by and for tea practitioners, Torniainen's study is an unqualified success, demonstrating a magisterial command of the *wabi*-related literature and the ability to see history through the lens of tea practice. However, I would hope that in the future she might bring her skills to bear on *wabi* for the benefit of a larger community of academics and students of Japan, even those who do not practice tea. To do so will require more critical attention to sources, a willingness to engage larger theoretical debates, and increased awareness of the ideological nature of the *wabi* concept in Japan's cultural history.

Morgan Pitelka

Burmese Manuscripts. Part 4. Catalogue numbers 736–900. Compiled by Anne Peters, Edited by Heinz Bechert. pp. xxvii, 274. Franz Steiner Verlag, 2000.

This catalogue of 105 Burmese manuscripts with 164 texts in the Bayerische Staatsbibliothek in Munich uses the same approach as the first three volumes in this series except that no corrections are made in the transliterated passages. The cataloguing of this collection will be continued in part 5. The introduction discusses the collection, the dates of the manuscripts, the materials used for the manuscripts, the contents, new reference material, and the approach used in cataloguing. There is an extensive list of abbreviations as well as addenda and corrigenda to parts 1–3 of the series. The description of the manuscripts is followed by a number of indexes: titles of texts (works); authors; scribes, donors, and former owners; geographical names; dates of manuscripts; and a list of the manuscripts.

In earlier volumes of this series, the manuscripts were described by category (parts 1 and 2) or a table of the titles arranged by category was included (part 3). A table of this sort would have been helpful in part 4. The introduction does give us a useful discussion of the dates (most of the manuscripts date from the second half of the nineteenth century, the earliest date for the copying of a text is 1767, and the most recent is 1941), and a list of rare or unique texts makes it easy to check on what is exceptional about this collection (all except one are in Burmese or are *nissayas*, and the one text in Pāli is available in a romanized version on CD).

There is a manuscript on paper with coloured drawings of deities, dignitaries, members of the royal family, and monks. There are a number of Kammavācā texts on paper, cardboard, lacquered textile, lacquered palm leaves, metal, and ivory. There is a collection of texts to serve as a handbook for monks on black paper. Otherwise, the manuscripts are on palm leaves.

In the preface to part 3, it was announced that a new scheme of more concise descriptions would be followed in future volumes in order to expedite the cataloguing. The descriptions here, however, seem to be as complete as in the earlier parts. The physical descriptions of the manuscripts is very

detailed. There are long extracts from the beginning and end of many texts. And the cross references to the earlier parts and other sources are very complete.

It would be very helpful if an updated version of the transliteration table given in part 1 could be included in all future parts of this series. This is especially important since there were modifications in the way the Burmese is transliterated in part 3, not to mention the difficulties for a scholar if part 1 is not handy. Instead of using the clumsy superscript numbers for the tones, so easy to confuse with footnote references, a different method is used (see part 3, p. xiii). For the colon-like character used for the third tone, *ḥ* is used. For the small subscript circle used in Burmese to mark the first tone, a dot is put under the vowel (or last element of the vowel in the case of the vowel transliterated by *ui*). For *o* with the second tone, *ō* is used. Since it is clear that there will never be a universally accepted way of transliterating Burmese, any work including transliterated texts should include a table. The table could also be improved by including in it the two forms used for initial vowels that can be written either with their own graphic character or by adding the vowels used in combination with consonants to the graphic character that stands for *a* when written alone (see part 1, p. xxii).

It is understandable that when this series was started, transliteration was the most efficient way to quote passages from Burmese texts. With the progress made with fonts and computers, this is no longer the case. Catalogues such as this would be much easier to use if the Burmese texts were reproduced using a Burmese font. Even after many years of working with transliterating Burmese and using texts like this one which include transliterated text, if I am seriously interested in a passage, I have to put it back into Burmese script. This makes it very difficult to check through material to find if it is important.

It is interesting that the extracts from the text here have not been "corrected". I use quotation marks because the notion of correct spelling in Burma is a recent one. A corrected version of a text will certainly make it easier for someone looking up words in a recent Burmese dictionary, but it is a little surprising that the texts were not quoted as found in the manuscripts in the first 3 parts of the series. Having the original text of a manuscript could be very useful for a scholar of Burmese.

Part 4 is a welcome addition to this series and will certainly prove to be another very useful tool for all who work with Burmese manuscripts.

William Pruitt