It was Abner Cohen (1971) who first introduced the concept of a “trade diaspora” to describe the interrelated commercial network of a “nation of socially dependent, but spatially dispersed communities”. If defined thus, it seems that the concept is of comparatively recent origin, even though trading communities fitting the diaspora description are known to have been in vogue in most parts of the world from a very early period. However, it is essential to the diaspora concept, as has been pointed out by Andre Wink (1987), that “the merchant communities in their spatial dispersion remain an alien element in the wider society in which they become settled”. This is more or less reiterated by Scott C. Levi in the present work. In fact, Philip D. Curtin (1984) tried to demonstrate by using Cohen's model of diaspora community that it was possible to throw light on a very large number of situations, though largely separated in time and in space, where trading groups formed networks that were internally coherent, and linked only in very limited ways to their “host societies”, in which they were notionally anchored. But Curtin's work demonstrates the relative poverty of his Asian examples, which tend to fall into the very conventional categories of “Banyans”, overseas Chinese and Armenians for the most part. Levi's work undoubtedly adds to the empirical richness of a field so far unexplored except to a certain extent by Surendra Gopal (1988, 1992), Stephen Dale (1994), and Claude Markovits (2000).

Generally conventional wisdom has long been, following the “Eurocentric paradigm”, that after the arrival of the Portuguese Estado da India and the Dutch and the English East India Companies, and consequent to the decline of the three great Muslim Empires – the Persian, Ottoman, and Mughal – Central Asia became increasingly isolated and plunged into a lengthy period of political instability and socio-economic decline. This was generally attributed, especially by Niels Steensgaard (1974) among others, to the monopoly of the Europeans in the transcontinental movements of commodities between Asia and Europe through the maritime routes following the decline in the traditional overland trade because of the political instability and the resultant chaos in the region. It is heartening that one of the main arguments of Levi in this work, which in a way confirms the findings of the present reviewer (1985, 1999), though in a different context, is that Central Asia in the early modern period was not economically isolated and that, on the contrary, the commercial relationship between Central Asia and India continued with a greater momentum. It has been aptly pointed out by Levi that it was in this very period of so-called economic decline and isolation that one finds thousands of Indian merchants, estimated at around 35,000, overcoming seemingly insurmountable geographical, political, cultural, and religious barriers to establish a diaspora network comprising dozens of highly active commercial communities dispersed across urban and rural central Asia.

The other central argument of Levi is that there was a qualitative difference between the commerce of the Indian diaspora merchants and the earlier caravan traders. While the latter were mostly engaged in export-import trade and returned home with the next caravan after completing their business transactions, the diaspora merchants remained in distant locations for extended periods of time, usually several years, and they profitably employed their capital in a variety of interest-oriented money-lending ventures. These merchants were well known throughout the diaspora as sources for various types of high-interest loans and for financing elaborate systems of urban and rural credit. This earned
them the outright protection, with a few exceptions, of the host countries despite the fact that the vast majority of these diaspora merchants were Hindus living in Muslim states.

The author argued and elaborated his theses admirably in five well-organized chapters. In the first, discarding the received wisdom that with the increased domination of the European powers in the Indian Ocean trade in the sixteenth to the eighteenth centuries, Central Asia plunged into economic isolation, he focuses on the ways in which the commercial relations between India and Central Asia were transformed and escalated during the period in response to changing early modern social, political, and economic circumstances. In this context, he discusses the growth of trade under the patronage of the great Islamic dynasties of early modern India, Central Asia, and Iran. The second chapter explores the emergence and ethnic composition of the Indian diaspora merchants in the early modern period. These individuals were agents of homogeneous caste-based family firms which sent them to distant markets in Central Asia to transact business on their behalf. As to the ethnic composition of these merchants, it has been suggested that the vast majority of them were originally identified by the more general designation “Multanis”. The next chapter analyses the social organization of the Indian merchant diaspora communities and their diasporic lifestyle, pointing out in the process, that their wide dispersal across much of Eurasia resulted in considerable variation in their social organization and operation. It has been demonstrated here that the diaspora merchants were regarded by their host societies as a cultural “other”, and that this was to a considerable extent a product of the Indians' own deliberate efforts to maintain their distinct identity and avoid social integration with the host society (similar in particular to the Armenian diaspora, Ina Baghdiantz McCabbe, 1999, S. Chaudhury, 2002).

Chapter Four makes an in-depth study of the Indian family firms and their economic functions in Central Asia and other host societies. It has been demonstrated here that the merchant-moneylenders of the Indian diaspora in the Central Asian regions were trained agents of numerous well-organized, heavily capitalized financial organizations, or family firms. It is to be noted that these firms first emerged as homogeneous, caste-based institutions long before active European participation in Asian markets. Their goal was to invest their resources in lucrative ventures for the accumulation of capital, and to use it in other emerging opportunities, with a cautious approach so that the risks were minimal. They could achieve this by diversifying their investments in various commercial ventures across a number of regions and commodities. They invested their firms' capital and extended their sphere of activity to urban and rural markets where their financial services were in demand, thus providing their host societies with much-needed commodities as well as investment capital and credit, both urban and rural, while earning considerable wealth for themselves and their firms.

The next chapter examines the transformation of India's commercial relations with Central Asia during the eighteenth and nineteenth centuries, a period of great political instability in the region. It tries to demonstrate that, throughout the period, Central Asia continued to benefit from its intermediate position in overland Eurasian trade as caravan traders responded well to Russia's increasing demand for Indian textiles and raw cotton. At the same time, it provides an insight into the economic factors behind Russian motivation to establish a military and political presence in Central Asia. It concludes with an analysis of the late nineteenth-century policies of the Russian colonial administration towards the Indian diaspora communities in their newly acquired territories in Turkestan, and it illustrates how these policies brought about the end of the Indian merchant diaspora in Central Asia. Finally, the concluding chapter gives a lucid summary of the important findings and main arguments of the author.

There is little doubt that the present work is an important and welcome contribution to the existing literature on the trade and economic history of Central Asia in the early modern period. It contains also a refreshingly and comparatively new assertion which refutes the generally accepted view portraying Asian merchants as ill-informed, poorly capitalized itinerant peddlers who hammered out a meagre existence by buying cheap, transporting far, and selling dear (J.C. van Leur, 1955; Niels Steensgaard, 1974). It only reiterates the recent propositions of S. Chaudhury and M. Morineau (S. Chaudhury and M. Morineau, ed., 1999).
References

The Golden Sheldrake: Mon Nationalism and Civil War in Burma.
Reviewed by Kazuhito Ikeda, University of Tokyo
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During the mid-1990s, Ashley South had contacts with the Mon “insurgents” as a member of the Burmese Border Consortium (BBC), an organization working for refugees fleeing from Burma (Myanmar). His recent publication may be regarded as an ethnographic history of the Mon nationalist movement on the Thai-Burmese border. It consists of six parts: an introduction and history of Mon nationalism before Independence (Parts One and Two); a history of Mon nationalism after Independence until 1990 (Part Three), until the middle of the 1990s (Part Four), and after 1995 (Part Five); and finally a political analysis of organizations opposed to the Burmese Government, focusing on the Mons (Part Six). The book can be characterized as an ethnography of insurgency and has a predecessor in Martin Smith’s Burma: Insurgency and the Politics of Ethnicity. South’s comprehensive description of the politics of the “insurgencies”, focusing on the NMSP (New Mon State Party) on the Thai-Burma border in the 1990s, provides valuable information which is not available other than through this book.

Unlike Smith, who confines himself to an ethnographical description, South attempts to situate the Mon nationalist movement in theories of nationalism and ethnicity. However, there is a lack of a proper structural analysis and intermediate arguments between the theories and his ethnography. Furthermore, South mostly refers to Mikael Gravers’ publication, which itself sometimes neglects historical details and gives priority to those facts supporting Gravers’ theories on Burmese nationalism. It is not that South should have dwelt only on the detail, but after the overwhelming quality and quantity of Smith’s ethnography, a cautious and systematic analysis is needed, focusing on various aspects of Mon nationalist movements, based on the accumulation of detail. Secondly, although South attempts to draw a picture of Mon history in general, he mainly focuses on politics during the 1990s.
His account of the history before 1990 remains sparse and does not sufficiently prepare the ground for parts four to six, thus polarizing the content between a Mon history on the one hand, and accounts of Mon insurgencies and politics on the other. Intermediate arguments between the theories and his ethnography, as well as detailed connections between Mon history and politics, would have given the work a clearer picture, and questions about nationalism and ethnicity of the Mon at present, as well as South’s fundamental questions about the Mon people, would have been better answered.

Let me raise two points regarding Mon ethnicity and the history of Mon nationalism to supplement the intermediate arguments that South neglects. The first point concerns the history of Mon nationalism. Although South only discusses ethnic politics during the Independence Period superficially, they did have an importance for the political development of the Mon thereafter. As Josef Silverstein has pointed out, the immediate origin of the ethnic problems in Burma up to now lies within the time between 1945 and 1947. The crucial issue, according to Silverstein, that to-be-independent Burma faced was “national unity”. When independent Burma was constitutionally shaped in 1947, the issue of “national unity” took the form of the problem of how to integrate the Frontier Areas into Ministerial Burma, that is, how to incorporate the ethnic groups of the Frontier Area into the Union of Burma. The Aung San – Attlee Agreement was concluded as a priority matter, and it would not be going too far to regard the major political events thereafter – the Panlong Conference, the Frontier Areas Committee of Enquiry (FACE), the general election, and the Constituent Assembly – as actually scheduled to negotiate this issue.

In the midst of these political developments, the treatment of the various Karen groups, who had lived across the borders of two different administrative regions under British rule, came to play a key role in solving the problem. These consisted of the Karenni and the Salween Karen, who, among the major ethnic groups in the Frontier Areas, did not give their consent to the Panlong Agreement, and the plain Karen, living among the Burmans in Ministerial Burma. The Mon had a close relationship with the latter, in that they were both also inseparably mixed with the Burmans in terms of living space, and political cooperation between Mon and Karen was extremely close most of the time. A sharp distinction can be made between the circumstances of the Shan, Kachin, and Chin on the one hand, who had their own territories and lived relatively separate from the Burmans, and the Mon and Karen on the other hand, who shared living space with the Burmans. The latter could be seen as a test of actual national integration in Burma. Historically, this enterprise of national unity led by the AFPFL turned out to be a failure. Karen, Mon, and Pao nationalists from the communities of the peoples living mixed with the Burmans were some of the first, among others like the Communists or the Karenni, to have gone underground by 1950. The history of “insurgency”, therefore, needs to be re-examined in this light, rather than postulating that they have always been separatists seeking independent sovereign states.

On the side of the Burman majority, one should not idealize the role of Aung San, either. In contrast to the admiring statements by politicians and researchers, including South and Gravers, Aung San did not necessarily understand the complexity of ethnic relations in Burma. His address at the AFPFL Preliminary Convention for drafting a constitution, held from 19–23 May 1947, shows clearly that his basic idea for solving the ethnic problem was not derived from the ethnic reality of Burma, but from highly conceptual Leninist and Stalinist theories on racial relations. This caused disappointment and antagonism particularly among the Karen nationalists. The address also connotes the possibility that Aung San was of the same opinion as other Burman AFPFL politicians, who were afraid of the emergence of strong political ties between the Karens in the Frontier Area and those in Ministerial Burma. Wishes like “if Aung San had survived . . .” can prevent one from pondering on the structural character of Burma’s ethnic problem.

Focusing on the same topic, Dr. Maung Maung’s book and the two-volume publication by the Universities Historical Research Centre (Yangon University), both dealing mainly with the politics of
the Independence Period, are far less satisfactory, with researchers tending to find the “true” origin of the ethnic problems in general simply in the British “divide and rule” policy. More profound studies of ethnic politics during the Independence Period should be made, for instance by widening the alternative “insurgency” perspective instead of that of the Burmese nationalists, which still enjoys a dominant status in studies of modern Burmese history.

The second point that I would like to raise is related to Mon ethnicity. Being part of the entity called “Mon”, what kind of political, social, and economic position do the particular Mon on the Thai-Burma border have? In his conclusion, South refers again to the principal question of his book: “Representing a constituency of perhaps one-and-a-half million people, most of whom live close to the political and economic centre, do Mon leaders have any choice but to envisage a future within the (federal?) union?” This question remains, however, unanswered, as “any conclusions are likely to depend upon where one stands on the big issues of Burmese politics”. Although South is clearly aware of the fact that “the Mon nationalist community is not homogenous” (nor is the Mon community), he hesitates to deal with the rest of the “1.47 million” Mon living separate from the 30,000 Mon of the NMSP-controlled area. Although they have not been involved in much political activity under the Burman military regime, their claims on Mon ethnicity have been flourishing within the fields of culture and religion. The Buddhist Pwo Karen, for example, historically neglected by Western historians due to the political conspicuousness of the Christian Sgaw wing, have written many versions of a “Kayin Yazawin” (Karen History) since the middle of the nineteenth century, and claim Karen-ness within a Buddhist context. Similar claims are also observed widely among the Buddhist Mon, as Mon ethnicity finds open space to express its identity through religious activities. This may be the case of the “1.47 million” Mon particularly since gaining independence. It is understandable that this aspect of Mon nationalism lies beyond the scope of South’s perspective. However, the allocation of at least one chapter, focusing on NMSP leaders and their thoughts on the representation of “a constituency of perhaps one-and-a-half million people, most of whom live close to the political and economic centre”, would have given a clearer picture of the plurality within the Mon ethnicity.

South has overlooked questions on the whole and the part, questions on aspects of plurality, which are required for studies on modern Burmese history. Numerous histories of Burma have been produced from a Burman nationalist point of view. Other works, trying to relativize these standpoints, still depend on material in the Burmese language. However, books, journals, manuscripts, pezas, and parabaiks (palm-leaf documents) in minority languages, among them also the Mon language, are abundant in Burma, and should be utilized for further relativizing and plurality.

**Hanzoong kwankae sa** 韓中關係史(History of Sino-Korean relations).


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Since Asian history began to be studied seriously in Korea in the 1960s, research into Sino-Korean relations has become a major theme. In the 1980s and 1990s this study was, with the exception of a few Chinese-history specialists, the preserve of scholars of Korean history. The author of the work under review, Kim Han-Gyu of Sogang University in Seoul, was a student of Professor Cheon Hae-Jong, the best-known authority on the history of Sino-Korean relations.

Kim has studied the East Asian order focusing on ancient China in a number of books including *A Study on the Chinese World Order of the Han Dynasty* (1982, in Korean), *A Study of the Mu-fu System of Ancient East Asia* (1998, in Korean), and *Tibet and China – A Review of the Historical Relationship* (2000, in
In the preface to the present work, he refers to the lack of a comprehensive and penetrating study of the subject among existing studies in Sino-Korean relations, and says that the reason he studied the topic was to understand the place and significance in the East Asian world of the totality of those elements which constitute East Asian history.

The author argues that culture does not flow from a superior position to an inferior one, but that cultures interact with each other to create in the end a higher level of culture. In the process of accepting Chinese culture, for example, Korea and other countries developed their own cultural strengths, giving birth to a third culture. He also points out that existing studies do not clarify the concept of “Korea” and “China” and he argues that Korea and China should be viewed as “historical communities” rather than “nations” or “states”. “Historical community” is defined as a country created from a shared space, blood-lineage, culture, and historical experience. “Sino-Korean history” is understood, therefore, to be the history of the interaction between the historical communities of Korea and China. There also existed a third community, Liaotung (Manchuria, called Manshū in Japan and Tungbei by China), and one of the most distinctive characteristics of the book is that the author understands Liaotung to have been an historical community different to the states established by the Koreans and Chinese. Based on the notion that Liaotung is a separate and unique historical community, and distinguishing it from Korea and China, he argues that Sino-Korean history was one of interaction between Korea and China mediated by Liaotung. Kim does not exclude Ancient Chosun, Koguryo, and Palhae from Korean history nor Liao, Jin, and Yuan from Chinese history, though all emerged from Liaotung, but insists that those countries should be included in both Korean and Chinese histories.

In the first chapter of volume I, Kim discusses the period when Liaotung was incorporated into Korea. Specifically, the chapter covers Sino-Korean relations chronologically in the following order: (1) during the establishment of Ancient Chosun in Liaotung; (2) during the Han Commandery in Liaotung; (3) during Koguryo’s occupation of Liaotung; and (4) during Palhae’s occupation of Liaotung.

In the second chapter entitled “The period of the separation of Liaotung from Korea and its incorporation into China”, he covers Sino-Korean relations (1) during the Liao and Jin annexation of Liaotung and China; and (2) during the Mongol annexation of Liaotung and China. This chapter is continued in volume II with Sino-Korean relations (3) during the divided occupation of Liaotung during the Ming and Jin; (4) during Manchuria’s unification of Liaotung and China; and (5) during the time Liaotung consisted of three provinces of the Qing dynasty.

In the third chapter, entitled “Liaotung’s incorporation with China”, Kim covers (1) Sino-Korean relations during the Japanese occupation of Liaotung; and (2) Sino-Korean relations with the unification of Liaotung and China under the People’s Republic. He thus considers political, diplomatic, economic, and cultural relations between Korea and China over a time-span dating from ancient times to the 1990s. It is also noteworthy that he makes a further contribution to the study of Sino-Korean relations with fifty pages of detailed references.

In addition, Kim criticizes historiography based on narrow-minded nationalism. He also criticizes the attitude of scholars who have supported government policies or who emphasize only the virtues of the national history. According to him, the narrow-minded nationalism which has prevailed throughout society and academia has distorted learning. To overcome this, he asserts that he will be concerned only with scientific truth and logic. Unsurprisingly, this perception courts objections by specialists in Korean history both inside and outside of academia, who stress the nationalistic spirit based on the history of Ancient Chosun and Kokuryo. It is also different from the official Chinese standpoint that defines the history and territory of China as based on present territory in order to solve the minority race problem, and that considers Liaotung as always to have been part of north-eastern China.

The history of Sino-Korean relations should be studied without considering the different interests of related countries in order to establish the nature of Korean and Chinese history as well as that of East Asia. In this respect, the author’s efforts to view Sino-Korean relations as objectively as possible by laying stress on Liaotung are noteworthy as a fresh idea. However, he does not explain specifically
what the historical community of Liaotung (which he defines as stretching east from Shankaiguan and west from the northwestern part of the Korean peninsula. Moreover, if an historical community can be defined as different from a state as the author argues, can Liaotung be understood as an historical community separate from those of Korea or China?

A further defect is that the discussion of the post-Song period, which is not the author's major field, is too general and reflects only existing research, and there is too much overlapping in the summarizing of existing research findings. Nevertheless, there can be no doubt that this book will become an essential guide to those setting out to study Sino-Korean relations.

Jiangnan de zaoqi gongyehua (1550–1850) (Early Industrialization in Jiangnan).  
By Li Bozhong, Shehui kexue wenxian chubanshe. 2000.  
Reviewed by R. Bin Wong, University of California, Irvine  
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With this book Li Bozhong continues his research on Jiangnan's economy in late imperial times. He moves from his important studies of agriculture, including his 1998 Agricultural Development in Jiangnan, 1620–1850, to the study of what he calls Jiangnan's "early period industrialization" in the three centuries after 1550. His work goes beyond most previous studies in two significant ways. First, his coverage extends from the frequently studied textile industries to a wide range of other industries. Second, he combines qualitative analysis of institutions with quantitative estimates of outputs in order to give us a more systematic picture of Jiangnan's industries than any other author has attempted. His efforts lead him to pose in the concluding chapters of the book a comparison of Jiangnan and English industries through the period of the latter's Industrial Revolution. Jiangnan's pattern of industrial development, which differed from that of England, continued to matter in the late twentieth century. The author thus places his subject in both historical and comparative perspectives in order to demonstrate to readers the relevance of Jiangnan's late imperial economic history in more global and contemporary contexts.

Chapter 2 examines the scale of cotton and silk textile production, suggesting that cotton cloth production doubled from 50 to 1,000 million rolls between 1620 and 1850, while silk grew in percentage terms even more – some 25 times in amount and 38 times in value between the late sixteenth and early nineteenth centuries (40). Output increases came both from increased employment, more division of labor, and improvements in technology. Li finds evidence of specialized ginning households and a division between weaving and spinning in the mid-Ming dynasty (61–63). He suggests that the division of labor between spinning and weaving allowed younger and older people with lower skills to do spinning, while weaving was performed by more skilled labor. By the eighteenth century all phases of textile production had become women's work; the division between men working in the fields and women engaging in textiles in Jiangnan was therefore a relatively recent phenomenon, not an old one as some scholars have assumed based on phrases from ancient texts (73).

Chapter 3 considers food processing – rice milling, flour, liquor, tea, and preserved fruits, meats and fish. Li estimates some 96,000 people worked in rice milling during the Qing dynasty, as milling spread into larger numbers of urban areas with the increase of rice imports (92). For both liquor and tea, the author suggests growing markets with more specialized production over time; for tea specifically there were also improved techniques with a shift from steaming to pan drying and better storage techniques (114–15). Regarding seed oils, Li explains the growing scale of operations between the Ming and Qing dynasties, marked by a more precise division of labor than that found in either textiles or grain processing (134–38). Chapter 4 turns to clothing, tobacco paper, printing, and articles of daily use including straw goods, candles, and writing brushes. These are commodities for which less research exists.
Combined with information on marketing that he also provides, Li helps us understand the economic base for the growing culture of consumption that Timothy Brook has made so vivid in his *The Confusions of Pleasure*.

Chapters 5 and 6 move from light industries to heavy industries. Chapter 5 examines tools for various kinds of production, including plowing, weaving, and dyeing, as well as construction materials such as bricks, tiles, and stone for buildings and sea walls. Iron tools were made in major cities like Suzhou and Nanjing. Li assembles examples of firms making iron implements growing over time with increased specialization and some technological advances (191–96). Similar arguments are made for textile machinery and building construction during the Qing dynasty, while improvements in firing techniques raised the quality of pottery (203–18). The chapter concludes by noting the increasing prices of building materials that the author takes as a sign of growing scarcities, a theme that will become central later in the book. Chapter 6 examines shipbuilding and repair, a Jiangnan industry that grew during the Qing period, both in terms of the value of boat construction and the numbers of people employed. Despite the limited sources on private boats, Li argues that their construction was widespread, more than compensating for the shrinking number of government tribute grain boats, so that the industry in general was expanding (237–44). He further argues that previous scholarship has slighted boat repairs, which he thinks were just as important as new construction (259–63). While earlier scholarship has been aware of shipbuilding as an important economic activity, Li argues that the growth of heavy industry more generally has not been appreciated. The chapter concludes by noting the increasing costs of boats, which like the building costs ending chapter 5, points us toward more general problems of resources scarcities.

Chapter 7 directly addresses the problems of resources and materials. Water, wind, animal, and human sources of power were all used in Jiangnan, but human labor was very important because of its relative abundance. The nature of fuel sources available within the region limited the kinds of industries that could easily develop. Some coal was brought in from Hunan and Shandong and charcoal from Jiangxi, but high transport costs discouraged larger imports (294–95). Iron was brought in from Fujian until the mid-Ming, then from Guangdong until the 1820s and finally from Hunan (305–12). Wood imports drew upon ever-larger areas as resource problems continued to grow (342). These resource constraints did not prevent Jiangnan’s early industrialization but contributed to shaping its particular features, which Li analyzes further in Chapter 8 on the rising imports of raw materials and food supplies into Jiangnan. This chapter complements and extends arguments and evidence presented in the author’s *Agricultural Development in Jiangnan, 1620–1850*, in particular on pp. 99–115. By the eighteenth century, the Jiangnan region imported considerable rice supplies as well as soybeans for fertilizer. Indigo, sugar, paper, and precious metals were also imported, while cotton cloth, silk, and books produced in Jiangnan were exported to other parts of the empire. Because of the varying resource endowments of Jiangnan and those areas with which it shared decent transportation, Jiangnan was better suited for light industrial development than the development of heavy industry (389).

Chapter 9 looks at the conditions of labor in Jiangnan’s early industrialization. This topic leads him to criticize Philip Huang’s controversial thesis of “involution” presented in *The Peasant Family and Rural Development in the Yangzi Delta, 1350–1988*. Huang has argued that returns to labor, in particular female textile labor, fell as populations rose with people working harder just to maintain subsistence. Li adds in the book under review to the presentation he made in *Agricultural Development in Jiangnan, 1620–1850* concerning rising labor productivity (133–55). He first argues that the Jiangnan population was less devastated by the Ming-Qing transition than were those of many other areas and that the subsequent rate of population increase here was also lower, which means that the population pressure Huang speaks of was not in fact as serious as Huang believes. Moreover, technology and management improvements led to increasing labor productivity; Li speaks in particular about the economic contribution of education on labor productivity, both specialized training for weaving and...
general education at the elite and mass levels (439–53). Li’s work complements other recent critiques of Huang’s interpretation of economic and demographic changes that appear in *Journal of Asian Studies*, volumes 61:2 and 62:1.

Where Huang continues to argue for an absence of economic development in late imperial Jiangnan based on the economy’s differences from English agriculture and industry, Li Bozhong shows in Chapter 10 how both English and Jiangnan industries were expanding before the Industrial Revolution. Rather than focus on relations of production as has been typical in Marxist analyses, Li Bozhong focuses on industrial structures and the material factors he believes most important to explaining the contrasting patterns of industrial development in Jiangnan and England. For light industries, the author suggests that Jiangnan industrial growth was comparable to English growth and that for textiles specifically, silk production grew faster in Jiangnan than cotton textiles did in England (461). In contrast, English heavy industry grew far faster than Jiangnan’s, with the largest differences to be found in mining, in particular of coal (468). These contrasts lead Li to characterize the structure of Jiangnan industrialization in terms of people’s efforts to save on energy resources – England used far more water and animal power beginning in the late seventeenth century, well before the shift to coal in the early nineteenth century. Jiangnan heavy industry possibilities were constrained not only by the absence of easily available coal, but by resource scarcities in wood and other sources of energy. The character of Jiangnan’s energy sources influenced the degree of mechanization and made small-scale independent management forms most appropriate for Jiangnan’s industry.

As Li shows in his concluding chapter, economizing on resources became a defining feature of Jiangnan industrialization. England’s Industrial Revolution is therefore not a relevant model for understanding the dynamics of Jiangnan’s industrial development. With limited inanimate power sources as well as limited building materials continuing through much of the second half of the twentieth century, Li concludes that it remained sensible to base production on small-scale operations until the economic reforms beginning in 1979. To understand twentieth-century economic developments in Jiangnan, a perspective from the late imperial period proves helpful and confirms development dynamics in an important part of China that clearly differed from English patterns familiar to economic historians more generally.


**Reviewed by Crispin Bates, University of Edinburgh**

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Claude Markovits’ book is beautifully written and provides comprehensive coverage of a fascinating and hitherto neglected subject: the world of the migrant Indian merchant. His carefully researched book lays many myths to rest and refreshingly demonstrates the difficulties and dangers of generalizing about such large scale “diasporas”. Indeed, Markovits casts doubt on the use of the term “diaspora” itself, pointing out that temporary migration accounted for 90 per cent of departures from India between 1830 and 1950, which suggests simple transiency or sojourning rather than the complete separation from the homeland which the word diaspora connotes. Movements of people between South Asia and the rest of the world, he asserts, belong to the sphere of “circulation” more than to the sphere of “migration” – a crucial point.

The introduction offers a convincing critique of the literature on the Indian diaspora – Markovits argues that region and locality were much more important in structuring migrants’ identities than religion, despite its prominence in existing studies. Thus the concepts of a Hindu and a Muslim diaspora are dismissed, as Markovits points out “Migrants from Gujarat, whether they were Hindus,
Muslims or Jains, had more in common with each other in their experience of migration than Gujarati Hindus had with Bhojpuri Hindus or Gujarati Muslims with Bhojpuri Muslims”. He also emphasizes that most South Asian migrants left because they wanted to improve the situation of their families at home, not because they were hoping to make a better life elsewhere: “their aspirations centred around plots of land and real estate in their home region, better houses, better marriage prospects for their sisters. This was true of rich merchants as much as of poor agricultural workers” (p. 6). Markovits is thus able to humanize the migrant experience, and to chip away at some of the enduring stereotypes which see labourers as essentially victims, and commercial migrants as essentially exploiters. In the same vein he is able to demonstrate that solidarities between migrants were chiefly rooted in common locality and hence ethnicity and religion were not crucial structuring factors. Proponents of conventional migration research will find much food for thought in this analysis.

In his first chapter, which deals with South Asian business networks, Markovits also produces a compelling argument about current historiography and the reason for the neglect of Indian commercial networks. He notes that the period between 1880 and 1930 witnessed a massive exodus of traders from India towards the rest of the world, but since they were not classed as emigrants, no detailed statistical record of them was ever kept. Furthermore, many of the networks pre-dated colonial rule, and the colonial state was therefore indifferent to them. As Markovits explains:

The power ascribed to colonial discourse in many recent writings to invent categories which did not exist in the social reality leads many to ignore the patent fact that the knowledge colonial administrators had of economic and social realities could be very partial. The result is that what does not figure in colonial discourse is deemed unimportant or even non-existent. As the British did not produce a coherent discourse on Indian merchant diasporas, about which they knew little and cared even less, except in very specific contexts, these diasporas have been almost obliterated from the historical record. (p. 29).

Markovits concentrates on two groups of Sindhi merchants (from Shikarpur and Hyderabad) but his book is of far more wide-ranging significance. For example, he tackles the question of how South Asian networks managed to adapt successfully to a trading world dominated by European capital. In so doing he draws a number of interesting and important conclusions. He notes how a kind of “division of labour” was worked out between Chinese and Indian traders, who carved up areas of expansion between them, and demonstrates that Asian networks were not a declining and dependent sub-formation within a European-dominated international economy, but demonstrated a dynamic redeployment of resources and strategies in the colonial era.

While some groups, such as the Gujarati Muslim Khoja and Bohra communities, took advantage of European economic penetration of new areas in Asia and Africa, others, such as the Parsis, who played an important role in the opium trade with China, made use of opportunities offered by the colonial regime’s outside ventures. Still others took advantage of “changes in flows and tastes” which resulted from the increasing “Westernization” of the world. Thus Markovits shows that Westernization, far from being a negative phenomenon for Asian merchants, created new opportunities, such as the marketing of oriental goods including carpets and other artefacts. These categories often overlapped, but together delineate “a space within which South Asian merchant networks could operate with a certain degree of independence vis-à-vis European capital, although not in opposition to it”.

Markovits dispels many of the hoary myths about Asian merchant networks — such as the idea of apolitical, shadowy figures, arranging informal, verbally agreed deals within a tight circle of kin-members. He notes that less than 40 per cent of commercial employees were recruited through kin networks and suggests that preference for employing kin was chiefly a matter of opportunity, and points out that “political skills were crucial assets in the long term in the development of international
financial and trading networks”. His dismissal of the fallacy that family and kinship are “privileged breeding grounds for trust” is masterful:

a Sindwork merchant did not trust another Sindwork merchant simply because he was from Hyderabad; he trusted him if he had a clean record which could easily be checked . . . Trust within the community of Shikarpuri or Sindwork merchants was not an automatic outcome of kinship ties or solidarity between townsmen, but the result of a process in which information about past behaviour played a crucial role.” (p. 261)

Markovits’ painstaking study offers further insights which may be useful for scholars of related diasporas; thus his discussion of early emigration as “circulatory” rather than “settlement oriented” can be read within the context of new research on the prevalence of seasonal rural–urban and rural–rural migration within India, and his comment that commercial employees were employed on indenture-like contracts, should be noted by historians re-evaluating studies of Indian labour migration, whilst his critique of idealized accounts of the role of “ethnicity” and “trust” in merchant networks is of significance for scholars working on Chinese and other internationalized Asian business communities. Markovits’ comment that an important distinction should be made between those who were in commercial occupations before migration and those who shifted to trade after they reached their destination, is also pertinent. This book concentrates on the first category of migrant, but the second, particularly applicable to East and South Africa, would be a fruitful field for further enquiry.

Very early in the book, Markovits states that his aim is to provide “a global understanding of such dispersed merchant communities, of their culture, their religion, as well as of the way in which their family lives were affected by their long-range travels” (p. 2). Similarly, because he argues that the merchant networks were not migration networks, with men circulating between the network centre and dispersed places of business, their study cannot be divorced from the women who stayed at home and who influenced the shape of the networks in a variety of ways. In practice, however, the sections of the book which deal with what he describes as the “sexual economy of merchant networks” are rather short and perhaps the least convincing of the book. Markovits elaborates three models of sexual economy – ascetic, permissive, and intermediate, which varied according to local circumstances. Thus in Russian Central Asia, merchants avoided sexual involvement with local Muslim women to ensure the transmission of inheritances to Shikarpur, while more of the merchants took concubines in Chinese-controlled Sinkiang because the local women were not in a position to claim a share of the inheritance. Markovits notes that while some merchants provided for their mistresses in their wills, “most of them seem to have treated the local women as chattels devoid of any right”. Nevertheless, he also asserts that in Japan, China, and Egypt, a number of marriages were celebrated between local women and Sindhi merchants. In discussing the behaviour of women at home, Markovits asserts that Shikarpuri women acquired a reputation for sexual freedom, and that children borne to women while their husbands were overseas, were never repudiated, whilst Hyderabadi women were more closely controlled. Markovits does, however, provide some intriguing clues to vexed questions such as the “kalapani” taboo on overseas migration of Hindus. He suggests that “the generalized taboo on the voyage of women represented a kind of substitution. The fact that the women of the household did not travel beyond the seas . . . seems to have been sufficient to ensure the continuing purity of the household.” (p. 27)

The less savoury aspects of Sindhi business are somewhat glossed over in the book. In the briefest of discussions on the moneylending activities of Sind merchants, Markovits contents himself with reproducing the words of Ashin Das Gupta describing the eighteenth-century Gujarati banias in Yemen: “by their tenacious hard work, their thrift and their expertise, they made whatever profit there was to be made (at Mocha). They were, of course, not ideal men” (p.191). No mention is made of the role of Indian
merchants in the Indian Ocean slave trade, which is another subject that remains to be explored.

In a brief discussion of the Sindhi diaspora after 1947, Markovits notes that the new pattern of migration is completely different from that which prevailed earlier: women have now joined the men in diaspora, and Sindhi business families have become “international families” with various members residing in different countries, and holding different passports. This radical break with earlier circulatory migration traditions is insufficiently explained in the book. Related to this, is his conclusion that Sindhi merchants neglected their home bases, with the result that the region failed to benefit significantly from the activities of their entrepreneurial communities. However, one could argue that the political reality of being a Hindu minority in a Muslim majority area led Sindhis deliberately, and increasingly, to divert resources away from the home territory.

Nevertheless, in his compelling tale of the Sindhis, Markovits describes a little-known community whose ubiquity is breathtaking—itinerant peddlers pop up from Madagascar to Tenerife, and his imaginative use of sources reveals merchants involved in such diverse projects as earthquake reconstruction in Yokohama, Japan, and shopkeeping in Naples, Italy. His dispassionate narration scarcely leaves us time to wonder at tales of Sindhis interned on enemy territory in wartime, or protesting to the authorities about immigration restrictions, and embroiled in political intrigues. His work is a testament to the value of detailed research on a community far removed from the stock-in-trade of most historians.

_Hindu Kingship and Polity in Precolonial India_.


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In *Hindu Kingship and Polity in Precolonial India*, the author attempts, through an examination of the Rajasthani kingdom of Kota, to present a picture of a precolonial Indian kingship and polity ridden with internal tension and disjunctions in which sovereignty was not integrated in the person of the king but shared by subordinates and rivals of king. In doing so, he criticizes, on the one hand, Nicholas Dirks’ study of the south Indian “little kingdom” of Pudukkottai and, on the other, the Saidian critique of Orientalism, as sharing the supposition that the power and authority of the ruler unilaterally operated from the top down to the bottom of society. Thus the author sets forth as his methodological standpoint the multi-dimensional and multi-directional relationships at work around kingship in the precolonial Indian polity. As a symbolic expression of this, he refers in the introductory chapter 1 (“The logic of the fish”) to the classical but reinterpreted concept of _matsya nyaya_ (the logic of the fish) in which not only do large fish eat small ones but the reverse also applies.

In Chapter 2, “The king is dead, long live the king!: or karmic kin(g)ship in Kota”, Peabody puts forward the unique concept of karmic kin(g)ship through his own reading of a painting of the Battle of Pandhar (1720) drawn by the so-called Kota Master. In this painting, the Kota king, Maharao Bhim Singh, is depicted as beheading Qilich Khan, though in fact, Bhim Singh was slain in this battle between the Mughal royal army, with which he sided, and Qilich Khan, later the first Nizam of Hydarabad. Further, the Kota Master included Ratan Singh of Ratlam, who had in fact died much earlier in the battle between Shah Jahan and Aurangzeb (1658), in the configuration of prominent figures at the battle of Pandhar. Thus, this painting has two “historical inaccuracies”, the meaning of which the author tries to read, seeing them as the Kota Master’s “heteroglossic” reading of the event, in which Qilich Khan became Bhim Singh and Bhim Singh Qilich Khan by virtue of Qilich Khan’s becoming the receptacle of Bhim Singh’s _karma_. In the same way, Ratan Singh was also linked to Bhim Singh through the shared _karma_ of exemplary kings. Thus, this painting by the Kota Master, the author
concludes, “was meant to reposition, or decentre” the historical facts by “creating a multi-centred,
heteroglossic domain”. This unique concept of karmic kin(g)ship might have been, I feel, more under-
standable had the author tried to position this ideology against the background of the real political
processes that must have been regulated by “the logic of the fish”.

Chapter 3, “In whose turban does the lord reside? Kings, saints, and merchants in western India”,
takes up the movement of the navnidhi(the nine treasures, that is, the nine idols in the form of the infant
Krishna) of the Vallabha Sampraday, in which Bhim Singh took initiation, as a means of reading
multilateral transactions in Rajasthani polities including Kota. After having peaceably resided in Braj
until the 1650s, the navnidhi began to wander about Uttar Pradesh, Rajasthan, Gujarat, and Malwa,
persecuted by Aurangzeb. For example, one of the navnidhi, Shri Mathureshji, moved to Kota in 1744
after staying for a while in Bundi. These seemingly queer itineraries of the navnidhi were, however,
conducted through complicated relationships between, on the one hand, kings who wanted to utilize
the idols for political purposes and the saints of the Sampraday (gosains) who counted on the king’s
patronage and, on the other, the saints and merchant-followers of the Sampraday who looked
to commercial opportunities in pilgrimage and the festivals of the deities. Thus the author finds
multi-directional actions-reactions in the movements of deities, though concrete documentation of
the commercial activities of merchant-followers as well as of their relationship with kingship seems
to be lacking.

Taking into consideration the methodological standpoint of the author, it is easily predictable that
in Chapter 4, “Military fiscalism and the cultural economy of devotion in eighteenth-century Rajasthan”,
the author should criticize Nicholas Dirks’ study of the south Indian “little kingdom” of
Pudukottai. Dirks’ picture of the polity in Pudukottai, the author alleges, is an incorporative and
redistributive one in which the authority of the king is assumed to permeate down to the bottom of
the society through the granting of such royal gifts as inam lands, honour, and emblems, causing no
disjunctive or adverse reactions to arise from the society. The formation of this sort of polity might
have been possible in the case of a very small kingdom where the king himself would have maintained
dyadic and personal relationships with all members of society under him, though surely this was
not the case with “the larger and more complex state-formations” in eighteenth-century Rajasthan, let
alone such great kingdoms as the Mughal Empire and the Maratha Kingdom (Peshva Government).

The relationship between Maharao Umed Singh of Kota (r. 1771–1819) and his prime minister
Zalim Singh is analysed in Chapter 5, “From ‘royal service’ to ‘maternal devotion’ during the Jhala
Regency: Local politics at the end of the old regime”. Zalim Singh, though having exerted full sway
over Kota for so many years, never attempted to usurp the kingship of Umed Singh. The author tries
to interpret this fact by characterizing Zalim Singh’s attitude toward Umed Singh not as ‘chakari’
(royal service) but ‘seva’ (maternal devotion) corresponding to the Vallabha Sampraday’s maternal
devotion for the navnidhi (infant Krishna). This picture of a unilateral, devotional relationship between
the king and his prime minister, however, seems to betray somewhat the author’s cherished idea of a
disjunctive polity in precolonial India.

In Chapter 6, the author reaffirms his methodological standpoint by criticizing the Saidian critique
of Orientalism in which, he argues, the colonial discourse is assumed to have unilaterally realized itself
as the colonial project without being involved in confusing reactions to or appropriation of colonial
discourses on the side of the colonized, resulting in the total neglect of the agency of the colonized.

Thus, the author’s reasoning concerning a disjunctive polity of precolonial Indian kingdoms
in which multi-directional politico-cultural cum economic relationships were multilaterally func-
tioning is plausible. It may, however, be not sufficiently persuasive for readers insofar as it often lacks
historical documentation as is the case, for example, of the relationship between Zalim Singh and
the idea of ‘seva’ of the Vallabha Sampraday. This leads us to an embarrassing question – at what
point does ethnohistory (ethnohistorical study) differ from history (historical study). If we should not
expect ethnohistory to provide the rigid literary documentation that history demands, then what is
the alternative means of proof for ethnohistory?
Prosperity, Region, and Institutions in Maritime China: The South Fukien Pattern, 946–1368.


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During the first millennium of the history of Imperial China, China’s communication with the rest of the Eurasian world was mainly by means of overland routes, and hence, in all probability, the empire remained a self-sufficient, land-based entity. This picture began to change after mid-Tang when China’s major coastal ports began to be frequented by merchant ships from Persian/Arab, Indian, Southeast Asian, Japanese, and Korean ports. The Chinese response to the emerging oceanic trade was quite rapid, and between the twelfth and fifteenth centuries, China developed as an important maritime power with a standing navy, supported by which Chinese junks dominated sea-routes stretching from the Coromandel coast to the South Seas and the East China Sea.

Chinese seaward expansion evolved apace with the wholesale transformation of the domestic socio-economy, aptly referred to as the “medieval economic revolution”. Among its salient strands are growth in agricultural and industrial productivity, the advent of a new stage of urbanism, commercialization of society, expansion of both domestic and foreign trade, the application of a spate of new technologies to major sectors of the economy, and the enhancement and dissemination of the idea and practice of social mobility. Concurrently, the spatial pattern of the distribution of wealth and population within the empire also changed. A shift in the economic and socio-political center of gravity from the northwest to the southeast was also a feature of this time.

It is difficult to decide whether we should understand China’s growing seaward-orientation as part of the medieval economic revolution. First, although China’s maritime activities after the fifteenth century were maintained and expanded by merchant seamen based on the southeast littoral (evidenced by the extensive import of Japanese/Mexican silver by Chinese traders in the Ming period, and by the progressive proliferation of Chinatowns in Southeast Asia), the policy of the central government in the Ming and Qing grew increasingly xenophobic. The puzzle of the detachment of political stance from economic reality has not been adequately explained. Historians who tend to make much of the institutional framework insist that there was a revival of a land-based China in the Ming and the Qing.

Second, Chinese historiography has tended to be based on sources recorded under the influence of the Han-learning (Han-xue), rather than on a cumulative study of local history. This is a function of the monopolization of literacy by elites, and the outcome of an ideological (Confucian) rhetoric which puts emphasis on the central in cultural, political, intellectual, and institutional terms. While a few pioneering works utilizing local sources exist, with an insight into the close link between the two synchronized phases of endogenous growth and the coming of exogenous stimuli, it is only recently that scholars have begun to build on their foundation. They have been encouraged both by the PRC’s doctrinal shift from an isolationist to an open-door stance after 1979 with a revitalized concern about the link between China’s economic upsurge and the commercialization of society as a result of the growth of domestic and foreign trade, and by the growing awareness among Sinologists since the 1960s of the need to explore social history. Since the 1980s more than a dozen books demonstrating the utility and feasibility of the social history approach in Chinese studies have appeared, amongst which is the work under review.

The book is divided into three parts: “The Process: An Economic Cycle of South Fukien”, “The Space: South Fukien as a Regional System”, and “The Structure: A Transaction-Cost Analysis of the South Fukien Economy”. Part One studies the region first as a frontier, and then looks at the historical process of economic development. Part Two analyses South Fukien as an internally integrated region, and Ch’uan-chou (Quanzhou) as a regional centre and then gives a case study of regional economic integration. Part Three discusses patterns of trade in terms of merchants, organization, and knowledge,
and then both formal and informal institutional constraints. Three appendices are included: “Fukien from Han to Sui”, “P’u Shou-keng, A Reassessment”, and “The Process of Administrative Division in South Fukien in the Tenth Century”.

So’s work joins Li Tung-hua’s Quanzhou yu woguo zhonggu de haishang jiaotong (泉州與我國中古的海上交通, Taibei: Xuesheng shuji, 1986) and Hugh Clark’s Community, Trade, and Networks: Southern Fujian Province from the Third to the Thirteenth Century (Cambridge University Press, 1991) as a study of the economic development of South Fujian (Fukien) in Song/Yuan times. While Li’s book came out too early to benefit from the greatly improved factual bases uncovered by studies made after 1980, both Clark and So were able to incorporate these new findings into their books, enabling them to develop their own arguments in a more analytical way than Li. A comparison between Clark and So reveals a number of similarities and differences. They both deal with South Fujian, the southeast littoral of Fujian Province, with the process of the area’s commercialization in response to the growth of the port of Quanzhou as an emporium for an extensive oceanic trade in Asian waters, and with much the same time span, from late Tang to the Southern Song (though So extends his observation down into the end of the Yuan). They diverge in how they understand and define the nature of the rising commerce in South Fujian.

Clark defines it as “transshipment” trade. He regards the foreign goods which entered the port of Quanzhou as luxury items, with few consumers in the region, which were mostly transshipped to their final markets in the lower Yangtze or North China, or in Korea, Japan, Liao, and Jurchen. He explains Fujian’s commercialization as the result of the limited amount of its arable land, which fostered the formation of a geographic division of labor for products: surplus grain from Guangdong and the lower Yangtze flowed into South Fujian, inducing local people to develop and raise commercial crops or to engage in commercial industry. He understands this to have begun to take shape during the time of the Min Kingdom (896–945), reaching its zenith during the Northern and Southern Song, until experiencing a downward trend at the beginning of the thirteenth century. The driving forces of this growth were, he contends, political unification by the Song, the further expansion of Fujian’s involvement in maritime trade encouraged either by the dynasty’s financial needs or by an enlarged consumption of imported goods in the domestic market (in the northern and central parts of China), and the overall progress in areal specialization and occupational division of labor in the region.

By contrast, So marks Fujian’s development by institutional changes. He identifies an initial upsurge (“take-off”) in the region’s socio-economy with the interim take-over of the Quanzhou/Changzhou area (946–78) by the local warlords Liu Congxiao and Chen Hunjin. With the unification of the empire by the Song, the area was integrated into the administrative territory of Fujian Circuit, whose capital was Fuzhou. This marked the functional division of coastal Fujian into two areas: North Fujian, the cultural/administrative core of the Circuit, and South Fujian, the economic core. With this as a frame of reference, he understands that the years 978–1087 formed the basis of South Fujian’s areal cohesion. Using the concept of “sectoral integration”, he suggests that the sectors of farming, commerce and industry were interwoven to generate the momentum to boost another jump in the areal economy which occurred at the next stage, beginning in 1087, the year the Superintendency for Overseas Trade opened at Quanzhou, helping to establish its supremacy in foreign trade over other competitors like Guangzhou and Mingzhou (Ningpo) in particular. He identifies the heyday of the prosperity of South Fujian centring on the port of Quanzhou as the period 1087–1200, a notion against which no one could raise any objection. He strengthens this view by his attempt to reconstruct in detail the city of Quanzhou itself at that period, giving us in the process our first comprehensive study of the city and its environs.

So is the first to incorporate results of recent archaeological findings about ceramics and their kilns, densely distributed in South Fujian, and demonstrates the existence of a close link between the growth of trade and a concomitant mushrooming of export industries in the area. What this implies is that the notion of the dominance of transshipment trade that Clark argued for can no longer be
sustainable. In addition, using wooden tablets excavated from a wreck in Quanzhou Bay, So demonstrates that at least some members of the imperial families residing at Quanzhou were among those who invested in the trade.

In giving a causal explanation for the sudden decline of South Fujian in the thirteenth century, So is in general accord with Clark – a degenerating political order, corruption of officials, frequent pirate raids, and decreased numbers of foreign ships – but he also identifies the deterioration of monetary policy as a factor in economic decline, and clarifies the role played by Pu Shougeng, a naturalized Persian/Arab trader who was Superintendent of Overseas Trade at Quanzhou in the latter days of the Southern Song. Pu turned against the dynasty, retaining his position under the early Yuan regime. Using sources such as biographical accounts from Yuan and Ming times, So postulates that at the time of the siege of Quanzhou, Pu's naval force was but a fraction of a much wider group of local elites who turned against the Song court. He surmises that without the participation of the regular army under the control of the Quanzhou prefect, the plot would not have been successful.

So's work is the richest source of information we have on the history of the port of Quanzhou during the Song and Yuan, and he has made a positive contribution to the advancement of our knowledge in at least in two areas. One is his use and analysis of archeological findings about export ceramics. This supplies persuasive proof as to the process of the intensifying division of labor in South Fujian when the area was maturing economically in response to the input of exogenous stimuli. The other is his demonstration of the usefulness of a good understanding of institutions relevant to social change. Nevertheless, several problems remain unanswered. First, the prosperous Sino-South Seas trade centred on Quanzhou must have been based upon an enhanced degree of interdependency between China and her trading partners. So concentrates on elucidating how Quanzhou and South Fujian changed as a result of their involvement in that trade, but presents little information about their partners. Publications such as Angela Schottenhammer, ed., *The Emporium of the World: Maritime Quanzhou 1000–1400* (Brill, 2001) and David G. Marr and A.C. Milner, eds., *Southeast Asia in the 9th to 14th Centuries* (Singapore, Research School of Pacific Studies and Institute of Southeast Asian Studies, 1986) attempt to fill this gap. So does not list the latter in his Bibliography. Second, So refers to the entrepreneurship of Quanzhou merchants to some extent, but does not explore the problem in any depth. Had forms of the same-native-place associations organized by Fujianese emerged at the time? We know that the Fujianese and Szechwanese were renowned during the Song period for maintaining a strong provincialism when they lived elsewhere. Was their preference for sojourning abroad a function of their need to maximize human resources at home? We would like also to know more about the shipping industry in Fujian in Song/Yuan times. Because of Fujian’s dominance in oceanic trade from the Song through the Yuan and Ming, Fujian and Fujianese long monopolized advanced skills in the building of deep-sea junks and in seamanship. I hope So will extend his treatment to such topics.

Finally, So uses the concept of “transaction cost” overly and frequently in his arguments elsewhere in the book in order to strengthen his institutional interpretations. If this means that the sophistication of the institutional framework was a vital force for the area’s economic growth, then how should we interpret Maritime Fujian during the Ming and Qing, when the government retreated from the oceanic trade but private activities of Fujian seamen continued. If we go a step further, and see this Ming/Qing phase in terms of the “retrieval of China’s land-based orientation”, it goes against the facts explored by scholars like Quan Hansheng, Tien Jukang, and others.

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**Book reviews**

See Gravers, p. 3. He states in criticism that “the dominant elements of Burmese development tend to be buried by detail” in “the typically voluminous works on Burma”. However, his arguments on nationalism in Burma seem to be largely based on the burial of details, particularly in chapter six which contains numerous misperceptions of historical fact.


5 Although it is proper to assess “national unity” among the various ethnic groups as being the most crucial issue of Burmese independence, Silverstein was trapped in the ethnocentrism of Burman nationalist writings, exclusively referring to Burman material in English and Burmese.

6 Both the communists, who were also Burman, and the Karenni, who were politically allied to the Karen on the plain, obtained a position similar to the one of the Mon, Karen, and Pao nationalists. Therefore, the early stages of the insurgency should be re-examined with regard to their conspicuous closeness with the Burman majority, giving them a distinct background compared to the later Shan, Kachin, and Chin insurgencies.

7 South, pp. 100 and 103.

8 Gravers, above, note 2, pp. 42 and 49.


11 South, p. 340.