
A Grand Synthesis

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How did the peasant society of south Asia respond and adjust to the political, ecological, and commercial changes that the region witnessed since the 18th century? A new book offers a unifying perspective on this enduring question in economic and social history.

REVIEW ARTICLE

Peasant History of Late Pre-Colonial and Colonial India by B B Chaudhuri; *Project of History of Science, Philosophy and Culture in Indian Civilisation, Vol VIII, Part 2, Centre for Studies in Civilisations and Pearson Longman, New Delhi, 2008; pp xxvi + 939, Rs 2,700.*

The somewhat dreary front-cover of the book is cluttered with information about the research project and the publication series to which it belongs. In that noise, the reader may miss the significance of this particular volume. In this book, Chaudhuri, the doyen among historians of Indian agriculture, offers a grand synthesis on peasant history in early modern and colonial India. Economic history of India is a contested field. Peasant history has seen its share of debates and controversies. Chaudhuri returns to these muddy waters with a clarity of vision, analytical depth, and methodical exposition that few practitioners of the field can muster.

1 Introduction

The book sets itself the task of answering the question, “how did the peasant society [in India] respond and adjust to the widening contacts” since the 18th century? The answer, which stretches into nearly a 1,000 pages, integrates three distinct analytical planes. These are ecology (mainly access to and use of water); politics (taxation and war); and economics (incentive systems, markets for goods and factors of production, and peasant collectives). To these three principal axes, a fourth can be added, shocks. In principle, war and famine can be clubbed under politics and ecology, but it is clear that some wars and some famines (such as the 1770 Bengal famine) caused permanent institutional shifts. The resultant narrative accounts for differential experiences across space and over time mainly by analysing the variable pattern of interaction between these planes.

Any informed reader will quickly notice the three principal strengths of the book. Its thematic scope is encyclopaedic. The book encompasses subjects as diverse as the rise and fall of regimes, statecraft and state capacity, caste and community, trends in markets, distinctive regional experiences, stability and transformation of rural institutions, famines and scarcities, the evolving relationship between forests and fields, and ecological imperatives. Second, at every step of the journey the writing ties up the big picture and the detailed picture deftly together. Chaudhuri is balanced and insightful when discussing the general, and masterly in his command over the particular facts which these generalisations make meaningful. Third, all along, one finds incisive and up-to-date revaluation of debates and controversies. These commentaries are completely free of jargon, academic fashion, mannerism and predilection.

Besides an introduction, the book is divided into four parts. Part I (chs 1, 2), ‘Characterising the Polity and Economy of Late Pre-Colonial India’, mainly addresses the debate on the economic implications of the political shifts that occurred in the 18th century – the collapse of the Mughal empire, the breakaway of its main provinces, and the transformation of the English East India Company into the ruler of a large regional state. Part II (chs 3, 4), ‘Peasant Agriculture in Colonial India: Establishment of New Links of the Peasant Village with the Wider Polity and Economy’, conducts a survey of the economic history of the peasant through this transition and the globalisation that occurred later. Part III (chs 5-7), ‘The Social Framework of Peasant Production’, concentrates on aspects of the institutional transition, particularly factor markets, class composition of the rural elite, and labour relations. Part IV (chs 10-12), ‘Beyond the Peasant Village’, deals with the adivasi world and the changing relationship between the forest, the state, and the peasants.

It is impossible to offer a detailed and adequate appraisal of this book in the space of a review. Instead of attempting a comprehensive summary, I will be selective and isolate three meta-themes for

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somewhat detailed discussion. These are, the 18th century transition, the dynamics of agricultural change in the early colonial period, and the dynamics of agricultural change in the late colonial period. The discussion will follow the treatment of the subject offered in the book, but will also occasionally go beyond, and voice my own impression of the relevant scholarships. I try not to mix the two levels.

2 The 18th Century Debate

Chaudhuri in chapter 1 engages in possibly the most substantial critique of the 18th century historiography published so far. To arrive at this critique we need to set out the intellectual context first.

Economic transition in the 18th century has been the subject of a major reinterpretation in the last 30-odd years. The economic consequence of the political transition – from one empire to another through a process of political fragmentation and re-consolidation – is a theme of great interest. There are two reasons why this is so. First, being an era of political fragmentation,

the 18th century does not yield a great deal of archival material. One needs to be innovative on sources, and be bold, at times too much so, in interpreting these sources. The second and more important reason derives from the wider relevance of these interpretations. An assessment of the 18th century matters to how we might assess the economic effects of colonialism itself. For example, 19th century colonial historians saw the rise of British power in the region as an outcome of the decline of Mughal power and the resultant political, military, and economic chaos. Colonial rule, in this view, led India from chaos to stability. The Aligarh school historians interpret the collapse of the Mughal empire in much the same way, as a process that upset and disturbed established modes of production and exchange, but do not draw the same kind of political lesson from it. They seem to see the 18th century as a transition from one exploitative regime to another.

The work of Eric Stokes, followed by the writings of C A Bayly, Frank Perlin, David

Washbrook, Andre Wink, Satish Chandra, Burton Stein, Sanjay Subrahmanyam, Muzaffar Alam, and by Prasanna Parthasarathi and André Gunder Frank more recently, together with scholarship on maritime trade in Bengal, Gujarat and southern India, have questioned the belief in a weak and chaotic pre-British economy. There are important differences in emphasis between these contributors to the debate. But taken collectively, the revisionist writings build upon five propositions.

First, the Mughal state was not a centralised despotic state, but depended too heavily on the cooperation of its nominally allegiant but substantially autonomous constituent parts. And therefore, a transition that saw the constituents declare sovereignty could hardly have been catastrophic. Second, Indo-European trade from the 17th century had created enormous potential for merchant accumulation in export goods, cotton and grain, currency and banking. Third, the fragmentation of the Empire into “successor states” had led the new political elite to forge closer

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relationship with merchants and bankers; their support was essential to the survival of these states. Fourth, the domain of the economy had always been somewhat independent of the state. The former functioned under its own institutional set-up, that presumably changed little. And fifth, while chaotic political conditions did exist, resources were mobile enough to shift from zones of insecurity to zones of greater security. In short, the revision does not see the decline of Mughal power as a shock, but as the beginning of new economic possibilities, enabling the rise of powerful merchant groups, new zones of prosperity, and the emergence of a ruling class that took more interest in trade than before.

Revisionist Interpretation

The revisionist argument rooted in Indian history has cast a powerful shadow on another scholarship rooted in global history, the one exploring the origins of world economic inequality in the modern times. The narrative of dynamism easily leads to the inference that early modern India was substantially similar to early modern Europe, where a similar commercialisation had been in progress. The point about parallel and equivalent development has been made by several authors, including Frank Perlin and Prasanna Parthasarathi. Given this equivalence, the only differentiating factor that might foretell the shape of things to come was colonialism joined with western Europe's fortunate access to the New World land, minerals, gold and silver. Gunder Frank's 1998 book *ReOrient* is perhaps the most influential recent statement, using material on south Asia, of this argument.

None can dispute that the revisionist interpretation has changed the way we look at the 18th century, indeed changed the whole conception of the Mughal state. It represents one of the important advancements in an analytical understanding of modern Indian history. That being said, the point that the 18th century witnessed unusual commercial dynamism with potential for long-term economic growth needs a long hard look. The fit between 18th century dynamism and the origins of world inequality is deeply problematical too.

For one thing, basic data on the scale of economic change remain missing. We

cannot estimate gross domestic product (GDP) or the scale of trade, industry, and agriculture, in relation to GDP; we cannot estimate with any degree of certainty the productivity in these enterprises; and we can barely speculate on urbanisation and population growth. There is serious observation bias. The fragments of the economy we know more about are the Europeans firms, the Indian merchants and bankers with privileged access to the regional courts, and the regions where these businesses were located. Outside these regions, chaos, insecurity and decline were common. The positive role of factor mobility has been overstated (more on the point below). The economic history literature on the rise of western Europe lays great stress on institutions such as property rights and commercial law, and state capacity and mercantilism of states. Serious comparative history on both these themes remains absent. Historians often cover up the missing numbers problem with a liberal use of adjectives. For a recent example, Peter Marshall's masterly review of the historiography remarks that the 18th century in India saw "huge" trade, "much" agricultural production, merchants operating "on a large scale", "much" manufacturing, "masses" of labourers and artisans, "great" cities, and "prosperous" market towns.¹ Another example of hyped up history is the south Asia discussion in *ReOrient*.

Chaudhuri's starting point in the book is the 18th century debate. His critique is rich in detail, nuanced and very wide-ranging. However, it does have two core concerns, state capacity and the peasant.

An important part of his argument concerns state capacity, a theme close to the heart of institutional explanations of economic change and as good as missing in the scholarship of the 18th century. While accepting the presence of a broad-based commercialisation and the rise of "intermediate groups" (to use C A Bayly's phrase) in regions of commercial dynamism, Chaudhuri also considers that the revision overstates its case and underestimates key premises of the Aligarh interpretation of medieval Indian economic history. While the argument (articulated by Frank Perlin mainly) that the economy was autonomous from the state corrects the imbalance implicit in a too state-centric

view of economic change, it goes too far the other way and ignores that the local constituents of the economic system could function autonomously only under an implicit contract with the state. Revisionism, Chaudhuri further argues, underplays at least four damaging consequences of imperial collapse highlighted in the Aligarh scholarship, revenue farming, decline of certain regions, local conflicts, and the atrophy of capital that had been connected with the Mughal empire.

Correcting Misreadings

On the other side the evidence on state capacity "to create proper conditions of economic stability and expansion" even in the larger and more prosperous of the regional states "is of doubtful validity" (p 75). The evidence in support of "resilience" of regional economies of Bengal and Awadh is roundabout and not wholly convincing. The short study in this chapter on Murshid Quli Khan's Bengal reminds us that the apparent fiscal success of this most capable of the post-Mughal regional rulers rested more on arm-twisting the zamindars than any move to raise productive capacity. The author suggests that it would be a misreading of the facts to see in the close collaboration that had developed between Murshid Quli Khan and the Jagat Seths any sign of an unfolding mercantilist state. I think this is an important corrective to popular readings of the economic history of Bengal, and needs to be extended.

The Jagat Seths enjoyed monopolistic access to coinage, money-changing and tax collection for the reason that the state had neither time nor inclination nor indeed the competence to create offices for this purpose. That there was dynamism in banking firms of this sort was a sign of the abject incapacity of the regional states to manage fiscal and monetary systems, a sign of sickness than health. The rot is also evident from the rampant rent-seeking the Jagat Seths practised by using their rights. Likewise, Murshid Quli in the quarter century of his reign relentlessly tried picking the pockets of the Dutch and the English to meet his palace expenses. None would suggest that the companies and their dependents were paragons of honesty. At all levels, this was a venal

business world racked by bribery, intrigue and mistrust, where key institutions such as good laws remained absent. The so-called success of the Bengal state and possibly all 18th century states including the East India Company in the first quarter century, rested primarily on skilful extortion, and only marginally on creating incentives for producers to produce more. The argument leads to a few sobering pointers on the role of the peasant in this narrative.

The peasant, the author of this book alerts us, is a weak actor in the revisionist story. When returned to the centre of the story, we observe that revisionism has little to say about the most important source of potential economic growth in the Indian economy of this time, or before and after, production and productivity of land. Being states that sought to manipulate the taxpayer rather than change the taxable potential of the economy, the regional states "did not have much to do with the complex production system, having no institutional basis at all for its planning" (p 93). This fact did not change despite new commercial opportunities on the margin. If anything, extensive tax-farming strengthened the incentive of the tax-farmer, who had to purchase their rights to tax by paying bribes and whose dealings with the peasants were temporary rather than permanent, to coerce the peasant.

What factors, then, shaped agricultural production and yield?

3 Agricultural Change in the Early Colonial Period

According to the book, agricultural change in the late 18th and early 19th centuries was driven by three forces that were in principle distinct, ecology, economics and politics. The process of change in any region during any time-span would show interaction between these three forces. The nature of the interaction, however, varied over time and space. These variations can be understood by particular chance factors, such as war. But initial conditions mattered too. In arid regions like Punjab or the Deccan plateau, for example, scarcity of water played a relatively larger and more independent role in the narrative of agricultural change than in regions with plentiful supply of water,

such as Bengal. In the latter, on the other hand, economic incentives offered to induce reclamation of forest lands played a relatively large role. Particular cultural-political traditions, such as compulsory military service of peasants in lieu of revenue in Assam, also influenced the interaction.

With a few exceptions, in most regions of south Asia, peasant institutions displayed certain shared characteristics in the 18th century. Chaudhuri calls this complex of characteristics "the internal domain". Within the village, market exchange tended to be limited between its parts. Producers tended to cooperate, especially in such matters as maintaining irrigation works and bargaining with the money-lender, who was usually an outsider to the peasant society. It is perhaps fair to say that the cohesiveness of the community increased with the need for collective labour and collective bargaining, and might have been dependent on ecological conditions such as availability of water and harvest risks.

The presence of collective command over crucial resources such as labour and capital did not necessarily mean equality of rights or equal access to income sources, however. Inequality was present and usually occurred between property rights that the so-called original settlers enjoyed and property rights that the migrant cultivators commanded. The community in some cases cut across these boundaries, but more often, involved families with similar rights and similar status. Usually, these were families bound by kinship

relations, so that collective control of land, labour and capital joined with notions of distinctness often expressed in the idiom of caste.

Recent historical research without exception notes the presence of agricultural growth in some regions in the 18th century. Nearly always, the growth involved settlement of migrant peasants and labourers in new lands and in some cases, resettlement in regions deserted due to famine or war. In northern, southern and eastern India, such shift of resources gave rise to zones of agricultural dynamism. Migration, indeed, was a major driver of such extensive growth in both agriculture and export-oriented industry. In agriculture, migrants often brought not only labour but also superior knowledge, especially knowledge of irrigation. Correspondingly, forced labour and direct state control over resources had little role to play in agricultural growth. Tax incentives, migration and the peasant collective, were adequate substitutes for state directives.

Costs of Redistribution

That being said, it is possible to overstate the positive changes in this period. Chaudhuri's account suggests that tax farming was widespread, reflected the desperation with which successor states went about their fiscal enterprise and that, contrary to some contributors to the revisionist story, tax farming usually had damaging effects upon peasant institutions. Chaudhuri accepts the positive assessment of migration with more than a degree of scepticism. Shift

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(Prof. J. F. Patil)
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of resources from famine-ravaged and war-torn zones to safer areas after all represents a redistribution of resources and not growth.

I would add that the 18th century historiography hugely understates the costs of such redistribution. Revisionist writers cheerfully note that weavers, peasants and labourers could pack their bags overnight and leave for new areas if they felt too oppressed. That did not necessarily make things better for them. Such moves had costs, in the shape of loss of working relationships, knowledge of market, capital assets, and a variety of rights enjoyed by the settlers and denied to migrants. Losses also include lives and days of work sacrificed in the act of moving. We have no way of knowing what these costs were and how many migrants did not make it for those who could peacefully resettle into a livable new home. The social and private cost of redistribution is an under-researched subject.

The latter half of the 18th century and the early decades of the 19th saw too many and too frequent conflicts in southern and western India to sustain political patronage to peasants. Chaudhuri shows convincingly that the war-torn regimes in southern and western India could only sustain war effort by brutally squeezing the peasants. In Orissa and Assam, the state structure collapsed, leading to agricultural decline via a complex and many-sided causality. In Punjab and Awadh, agriculture was less affected by political upheavals. And in Bengal, the 1770 famine put a violent end to land extension. Bengal took decades to recover from the famine and in some accounts the process of recovery saw new class configurations emerge. Chaudhuri presents the best analytical account so far published of the aftermath of the famine. We may never know whether or not on balance in 18th century south Asia there was more agricultural growth than decline and more growth than redistribution. In the account we find here, the foundation for growth was at best weak and short-lived.

The first 40 odd years of the 19th century saw the same politically induced disruption continue in western India. In varying degrees all of south Asia, but most acutely the ryotwari settlement areas, saw a prolonged price depression that significantly

weakened agricultural incentives and growth. Several historians, David Washbrook being the foremost, consider the Great Depression between 1820 and 1840 a turning point and a catastrophic episode. Chaudhuri's account confirms this impression in large measure, while presenting a refreshingly new angle on why it happened.

The default view is that excessively high tax rates imposed in the first round of ryotwari assessment, combined with the requirement to pay taxes in cash, increased the demand for money and squeezed mass consumption. At the same time, fall in aristocratic consumption of luxury manufactures and food, added its weight to produce a massive inward shift in the aggregate demand for food and manufacture. Chaudhuri does not consider the ryotwari tax rates as such to be the main cause of the depression and highlights two other factors. First, currency reforms and unification in the 1830s led to a shortage of money. The second factor points to the transitional nature of the demand shift. The points of aristocratic consumption in the Mughal and post-Mughal regimes had been located close to the points of industrial and agricultural production. When such local demand fell, in principle, production needed to reorient to distant sources of demand. But this process was constrained by huge transport bottlenecks. The transport constraints were especially serious in the Deccan plateau devoid of viable river transportation. The length of the depression was the price paid by private producers for missing public goods.

The depression ended because of a twofold change, reduced tax rates and mass transportation that eased up the demand squeeze.

4 Agricultural Change after 1850

The process of agricultural change from the late 19th century again brings back the interplay of ecology, economics, and politics. But politics plays a smaller and more indirect role in colonial India. Ecological degradation, globalising markets, and infrastructural revolution, played a larger role.

In 'Peasant Response to Markets' (chapter 6), Chaudhuri deals with a theme, commercialisation of agriculture in which his earlier contribution has been seminal. The chapter discusses a trend

towards a larger proportion of output going to meet demand that originated outside the countryside – the definition of commercialisation here; and distinguishes between two phases of the process, the indigo-opium phase until about 1860 and the wheat-cotton-jute-rice-sugar phase post-1860. While international trade was certainly a major factor in the trend, one should not overemphasise the role of exports. Globalisation of Indian agriculture occurred to a rather limited extent compared with many other topical colonies. Major exportable crops such as cotton and wheat quickly found domestic long distance trade a more important source of demand than export. Dedicated exports like indigo declined at the same time. Was this commercialisation forced by colonial policy or was it a voluntary rational response to price signals? Disputing a well known thesis that the commercialisation was forced, the book explains in greater detail a point that the author has made elsewhere, that peasants reacted to price signals in ways that could be called rational and profit-driven. However, Chaudhuri adds, this rationality was bounded by limited knowledge of world market trends. Historians such as Michelle McAlpin might add, it was also bounded by limited access to particular forms of insurance that could protect them from price risks.

The book describes the new business environment primarily in terms of new forms of market exposure, new forms of control over resources and trends in factor markets. The causation of market fluctuations changed from structural ones such as harvest failure to distant and international factors like economic depression in Britain. Harvest risks could be in theory insured against by keeping grain stocks. Price fluctuations called for a different kind of insurance, which, as the recent farmers' suicides remind us, few peasants even today have access to. Further, price fluctuations could also dull the response of peasants to harvest risks. David Washbrook suggests in a paper that peasants in Madras Deccan exposed themselves more to shocks by running down grain stocks in times of high prices. Forms of control changed too. In indigo, the book argues, direct coercive forms of command secured contracts in the early 19th century. Chaudhuri attributes

coercion to coalescence between two forms of authority, planter and zamindar. To me, this thesis does not completely square up with many references in the indigo sources to conflicts between the zamindars and planters. In any case, in the late 19th century, transaction systems had become more market based, with law and credit playing a much larger role in securing contracts than coercion.

Chapter 7 looks more closely at the most important form of new relationship in rural India, credit, noting the three drivers that impinged on the debtor-creditor relationship in opposing ways from the late 19th century – law, market risks and trade. Market risks – could make the debtor dependent too easily, and law strengthened creditor rights formally, though a great deal of settlement could still take place outside the court and with reference to traditional norms. Good trade conditions and high prices, on the other hand, made the peasant less dependent on the creditor. The chapter notes that the interplay of

these three forces did not necessarily produce similar outcomes everywhere and yet, one common factor throughout India was the advantage the rich peasant commanded in both grain and money markets. A peasant with surplus money to lend would be well-off in bad times, by making loans, and well-off in good times, by means of better access to grain trade. This situation points to a persistent bias for moneylending to integrate with grain production and sale.

Peasant Differentiation

Property rights in land, credit relations and the rise of rich peasants, and competition for physical capital differentiated peasants and if my reading is right, weakened or broke up the collective. Land laws, at the same time, also allowed superior right holders to gang up and attempt to protect their rights. For, property laws tended to respect the joint family and kin groups, more or less in all regions. So, the community perhaps did not disappear,

but decentralised among smaller and more homogeneous groups along lines of caste groupings. In any case, unequal access to credit, rights and capital made some members of the community treat the others as clients rather than as brothers, and potentially made the former better off, if not always at the expense of the latter. Peasant differentiation again was not a uniform movement. It was enormously variable depending on regional resource conditions, on whether the region was a new or an old agricultural region, and on new political trends in mobilisation that took shape from the early 20th century.

Economic historians used to discuss how far the “penetration” of capitalist relations had led to the formation of a wage labour class in rural India. The Marxist-Leninist terms of the discussion changed some time in the 1970s when the need to include caste and hereditary contracts into the analytical narrative was underscored. This debate faded out not



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because the economic history question had been answered, but because social historians in the 1980s decided to join Edward Said's convoy and devalued economic history. Postmodern critics did this by proposing that colonial discourses on labour represented Europeanist mental constructs. Chaudhuri revives the economic history debate and suggests that the formation of wage labour owed to distinct processes, dispossession from land was more common in some regions, but in some other regions, integration of pastoral groups into agricultural work was active too. Further, dispossession had limits. Creditors and landlords did not necessarily take closer control of actual cultivation when their clients failed to fulfil contracts. The peasant family farm showed persistence through new market-based relationship. Chaudhuri attributes this persistence to two factors, new capitalists did not often want to take on the complicated management problems involved with actual cultivation, and the low opportunity cost of family labour made family farm a viable form compared with capitalist agriculture. I find the latter argument incomplete because it leaves the low opportunity cost unexplained. Another ingredient in the stability of the family farm was the indirect protection that it received from legislation, a point taken up in the last chapter of the book.

In the two penultimate chapters, the author draws on his own recent works to address a relatively neglected theme in economic history, the experience of the forest dwellers and adivasis in the new agrarian regime. Some elements in the story are now familiar, for example, introduction of forest control and barriers to access of forest dwellers to common resources in the colonial period, or the encroachment of peasants and lenders into the land frontier zones where the adivasis lived. The chapter sheds new insight into these processes with a perceptive review of the recent scholarship on the origins of forest policy, an analysis of revolts and rebellion, an emphasis on the distinctiveness of the experiences of adivasi groups (in this case a comparative study of the more politicised Munda-Oraon and the more decentralised Santals), and a comparison of how state policy affected the economies

of settled peasantry and the forest-dwellers in different ways.

The last chapter (chapter 12, 'Summary and Conclusions') will be of great interest to the professional historian, for here the author takes leave of descriptive history and returns to themes and debates in historiography. It is impossible to offer a summary of the discussion, because the strength of the chapter lies in critical engagement with a wide range of views rather than in the construction of a singular new narrative. Nevertheless, the chapter does have a narrative core featuring the four drivers of agrarian change, ecology, economics, politics, and shocks. It concludes that in colonial India, "the direct role of the state in the peasant agriculture declined, ... on the other hand the direct role of the economic variables tended to become increasingly important" (p 851). Ecological conditions, rather resource endowments such as land and water, changed for the better in some regions that saw canals, but for the worse in the longer run and in the aggregate. Exogenous shocks of a political or ecological kind became less frequent, but possibly more intense in effect because of the increased exposure of a large number of people to market risks.

Commenting on the debate between the "nationalists" who consider the colonial state to be the principal driver of agrarian and economic change and the "revisionists" who consider the state, for its continued dependence on indigenous agents, to be far less powerful and central, the author rightly concludes that a balanced view needs to combine elements of both these positions. A too colonialism-centred narrative takes us away from the other important constraints and opportunities, of an economic or an ecological kind. At the same time, it is necessary to remember that indigenous agency of whatever kind could continue to be locally important owing to the sanction of and implicit contract with the state and could hardly continue unchanged.

5 Conclusions

The book redefines the state of the art as conventionally understood. By "convention" I mean that it is focused on macroeconomic relationships. Themes of current interest concerning the microeconomic profile of

the agricultural household – wages and earnings, consumption, levels of living, demographics, labour supply decisions, allocation of household labour to different tasks, men's and women's work, and diffusion of new knowledge – remain rather marginal, if not neglected, themes. The ecological axis in agrarian history is a breakthrough in my view and enables asking a range of new questions that whole generations of historians, in their preoccupation with the three villains of British India – the officer, the landlord and the moneylender – had failed to ask. That being said, it is necessary perhaps to move from an instrumental or water-centric view of the agrarian environment towards a conception of the environment as biosphere, including soil, moisture supply and distribution, elevation and temperature.

I have a few minor complaints with the design of the book. Given the length and the many themes covered, the text needs to pause frequently and summarise the argument. These summaries are useful for the reader interested in particular themes, but interfere with the flow of the text for one who is wise enough to try reading the book from end to end. The book would have gained from an index of names.

Teachers of economic history of India need to make use of this book in the classroom. Teaching the subject will acquire depth, become easier and be more interesting if they do. The publishers, evidently, did not consider that possibility, for otherwise they would have straightaway brought out a paperback made up of thinner paper, closer print, more attractive get-up, and costing a fourth in price. The present edition is rock-solid no doubt, but with its encyclopaedic bulk and price, likely to scare away the student. That would be a pity. For Chaudhuri's writing joins uncommon erudition with a lively and lucid style and hard-headed common sense, to a far greater degree than the average economic history work emanating from either India or the north Atlantic does these days. These features students should find attractive, and would do well to emulate.

NOTE

- 1 P J Marshall (ed), *The Eighteenth Century in Indian History: Revolution or Evolution?* Oxford University Press, Delhi, 2003, pp 14-15.