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What is This?
The Mughal state—Structure or process?
Reflections on recent western historiography

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"The empire of the Great Mogol comprehends several nations, over which he is not absolute master. Most of them still retain their own peculiar chiefs or sovereigns, who obey the Mogol or pay him tribute only by compulsion. In many instances this tribute is of trifling amount; in others none is paid; and I shall adduce instances of nations which, instead of paying, receive tribute".

"What, then, were the new elements of political chemistry out of which Akbar compounded such a large, stable, long-lasting political structure? At the risk of oversimplification, I would say that these were an extreme systematization of administration, a new theoretical basis for sovereignty, and a balanced and stable composition of the ruling class".

Introduction

Much has been written in the past three decades about the Mughal state, which dominates the study of 'medieval' Indian history, even though its career extends in one fashion or the other as late as the middle of the nineteenth century. The first volume of The Cambridge Economic History of India (1982) is largely devoted to the Mughals; the New Cambridge History of India, a set of volumes still in progress, has an entire section (Section I) devoted to 'The Mughals and their Contemporaries', including—ironically enough—a volume on the Vijayanagara state, which was founded some two hundred years before Babur set foot in Hindustan.¹

¹ Cf. Tapan Raychaudhuri and Irfan Habib, eds., The Cambridge Economic History of India, Vol. 1, Cambridge, 1982; the volume on the general history of the Mughals in the New Cambridge History of India is to be by J.F. Richards (provisional title: The Mughal Empire).

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The greater part of these writings have, almost inevitably, been produced in south Asia, and at least some of them have the sort of coherence that one would attribute to a ‘school’. In contrast, with the exception of John F. Richards’s *Mughal Administration in Golconda, 1687-1724* (Oxford 1975), the 1960s and 1970s saw no major work on the economic and political history of Mughal India produced in Europe or North America. Indeed, at the level of monographic literature, scholars based in these continents have remained remarkably wary of venturing any generalisations on the ‘high’ Mughal period—preferring instead to view the Mughals from the safe vantage-point of the late eighteenth or nineteenth centuries, and very largely on the basis of the documentation provided by the English East India Company. Writings such as those of André Wink, sometimes misrepresented as making a basic contribution to Mughal studies through the use of ‘master concepts’ (in the instance, *fitna*), in fact are essentially treatments of regional states in the eighteenth century, based almost exclusively on materials pertaining to this period.

This is in marked contrast to the situation in respect of cultural history, where much interesting work continues to emerge, on subjects like Mughal architecture and painting, from the western universities. However, these writings remain imperfectly integrated into the larger political, social and...
economic history of the period, and a work of synthesis which takes account of these varied developments is as yet a distant prospect. At present, most writers on art-history and architectural history accept in a relatively unquestioning manner the basic postulates on the nature and history of the Mughal state set out for them by political and social historians, on the basis of chronicles and documents; political and social historians, for their part, seem to have disdain for art-history and allied disciplines.

Some recent monographs (in particular two by Douglas Streusand and Stephen Blake) seem to mark a departure from the earlier trend, in which south Asia-based scholars dominated the study of the political, economic and social history of the Mughal empire.4 The present article is partly concerned with locating these new writings in the ongoing debate on the Mughal state and the society it ruled over, while at the same time showing where such work stands in a larger comparative perspective, taking west Asia in particular into account. This is all the more necessary since the authors of these writings seem to claim for themselves a status that goes beyond the narrow confines of the Mughal historiography, and make frequent reference to the Ottoman and Safavid states.

The received wisdom

Let us begin though by sketching very broadly the received wisdom and the current state of the historiography on the Mughals. It is often stated that modern studies of the Mughals are dominated by the 'Aligarh school', a statement that might itself be open to controversy. Is there an 'Aligarh school' in medieval Indian history? If so, what are the main propositions it has put forward? A consideration of Medieval India—A Miscellany, an occasional publication from the Centre for Advanced Study, Department of History, Aligarh Muslim University, may leave the reader in doubt.5 The Miscellany is precisely that, an eclectic collection of points of view; if


one thing dominates these essays, it is a basic reliance on Persian source material, although even here there are some exceptions. And reliance on sources in a particular language is scarcely enough to define a 'school'.

The 'Aligarh school' may then partially be a misnomer (just like other similar labels, such as the so-called 'Cambridge school' of Indian history). But what is normally meant when the term is used is something quite precise, denoting an adherence to a particular set of propositions in relation to the Mughal state and its interaction with the society of the time. These propositions cannot be associated with all those who have contributed to the Miscellany or who have been associated with Aligarh; rather, the key writings are those of Irfan Habib, Athar Ali, Noman Ahmad Siddiqi, Iqtidar Alam Khan, Shireen Moosvi, and—despite his lack of formal attachment to Aligarh—Tapan Raychaudhuri. The writings of K.A. Nizami or S.A.A. Rizvi cannot be seen as belonging to the same approach as the above writers, nor can those of S. Nurul Hasan. In the case of Satish Chandra, we must distinguish between his earlier writings (which are of a piece with the views of Habib, Athar Ali et al.), and more recent musings by him on the eighteenth century.

Having made this clear, let us examine the core propositions of the so-called 'Aligarh school'. They, in my understanding, are as follows.

1) On chronology: The main focus is on the period from Akbar to Aurangzeb, which is to say 1556 to 1707. This is the period dealt with for example in the major text produced by the 'school'; Irfan Habib's *The Agrarian System of Mughal India* [1556–1707] (Bombay, 1963). Even within this period, the main focus is on the reigns of Akbar and Aurangzeb themselves. This also means giving overwhelming importance to certain texts, of which the 'most favoured status' is extended to the *A'in-i Akbari*, of Abu'l Fazl, produced in the reign of Akbar. It is argued moreover that the key Mughal institutions were put in place by Akbar, and remained there under Jahangir and Shahjahan, only to come under challenge during the reign of Aurangzeb. We note that

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*Madhyaśāhāt, ed., Irfan Habib, New Delhi, Rajkamal Publications, 1981–1990. These volumes contain selected translations from English of articles and book-reviews, both from the Miscellany and other sources; they exclude the writings of K.A. Nizami, but include those of Ashin Das Gupta (whose views on trade and its role in an understanding of the Mughal state, coincide closely with those of the 'Aligarh school').

both the early period of Mughal rule (including both Babur's and Humayun's reigns), and the post-Aurangzeb era, are given short shrift.  

2) The nature of power: The empire in the years under examination is portrayed as a highly centralised and bureaucratised 'absolutism'. Such however was apparently not the case under Babur and Humayun, nor under Aurangzeb's successors. Manifestations of this precocious centralisation are in the Mughal revenue-system, mansabdarî, the coinage system, and the high degree of control exercised over society in general, on which more below.  

3) Extractive character: The Mughal state is thought to have had a massive impact on producers, extracting their surplus almost wholly. In Raychaudhuri's portrayal in *The Cambridge Economic History of India*, the Mughal state was 'an insatiable Leviathan (with) ... unlimited appetite for resources', which had the peasantry 'reduced to bare subsistence'.  

4) Spendthrift elite: This extractive character implied in turn massive concentration of resources in the hands of the elite. However, the surplus extracted, it is argued, was used unproductively for conspicuous consumption, including of imports. One of the reasons why technology remained static was this elite attitude, which was lacking in scientific curiosity and technological application.  

5) Irrelevance of ideology: 'Ideology', usually read as 'religion', may be seen as largely irrelevant for purposes of historical analysis. The main contradictions and tensions are to be viewed as structural, and flow from the clash of interests rather than ideological perspectives. Even the reasons for the curious elite ideology mentioned above (proposition 4) are not investigated, but treated as given. Part of the reason for this appears to be the need to use certain selected texts quite literally, rather than consider the possibility that they might be ideologically motivated. The notion of the 'normative' text thus does not feature in these writings for the most part.  

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7 For the characteristic neglect of the periods of Babur and Humayun, see Tapan Raychaudhuri and Irfan Habib, eds., *The Cambridge Economic History of India*, Vol. 1, passim; M. Athar Ali, 'Towards an interpretation of the Mughal empire', *Journal of the Royal Asiatic Society of Great Britain and Ireland*, 1978, No. 1, pp. 38-49. In contrast see Mohibbul Hasan, *Babur: Founder of the Mughal Empire in India*, New Delhi, 1985, pp. 32-78; and some examples among the earlier writings of Iqtidar Alam Khan, such as his *Mirza Kamran, a biographical study*, Bombay, 1964.  


10 Cf. Shireen Moosvi, *The economy of the Mughal empire c. 1595: A statistical study*, Delhi, 1987. It is evidently no coincidence that this monograph, which rests heavily on the Ā'in-i Akbarī, never discusses who Abu'l Fazl was, or for what ends the text was written. Significantly, most reviewers of the book have also passed over the issue in silence.
6) Sterility of trade: This proposition appears to flow largely from (4). Imports are seen as largely required to service elite consumption. Since this position bears a close resemblance to the one espoused by eighteenth-century physiocratic literature, it is natural that the 'Aligarh school' opposes its writings to those of 'bullionist' historians, who it portrays as praising trade because it brought precious metals into the economy. However, even for the 'Aligarh school' trade may not be wholly irrelevant in one specific sense. This is in terms of the potentially destabilising effects of the bullion inflow through inflation in the seventeenth century, the so-called 'Price Revolution'.

7) Eighteenth-century decline: This proposition has, more than any other, attracted attention, although not even all of the 'Aligarh school' (as we have defined it) have the same opinion on the question. Tapan Raychaudhuri, for example, apparently does not subscribe to the view of a decline in the economy in the eighteenth century, in his contributions to the two volumes of the *Cambridge Economic History of India*. Most fervently attached to the proposition are Athar Ali and Irfan Habib, with the latter having first articulated his position in the closing chapter of his *Agrarian System*. He argued there that the *jāгirdārī* system, whose very nature promoted short-term exploitation of the peasantry, combined with other factors such as inflation to provoke a 'crisis', manifested in widespread peasant rebellions against the Mughal state. This crisis came to a head already in the last years of Aurangzeb's reign, and continued through much of the eighteenth century, leading to the generalised 'subversion of peasant agriculture'. The eighteenth century was in his view a period when 'the gates were opened to reckless rapine, anarchy and foreign conquest'.

The 'formation' of the Mughal state

Having set out, at some length, the received wisdom on a variety of issues concerning the study of the Mughal state, let us consider how recent western writings part company with it. A major issue is clearly chronology, or proposition no. 1, as we have set it out above. It is certainly worth considering that the pre-Akbar period may have had a greater significance than usually given to it. It could be argued, for

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example, following the lead of Iqtidar Husain Siddiqui and more recently Mohibbul Hasan, that the period of Lodi rule, running into the reign of Babur, already showed signs of a new dynamic where the state-building process was concerned. In the case of Douglas Streusand, the title of his book itself gives a clue to his views on the matter. For him the ‘formation of the Mughal empire’ took place under Akbar, and the earlier period can be dealt with quite summarily (Babur and Humayun merit only a few paragraphs, pp. 36–37). Again, for Stephen Blake, the pre-Shahjahanabad history of Delhi (including Humayun’s use of the centre for his court) can equally be given short shrift, and the significance of the fact that the site was repeatedly used, with intervals, is thus lost sight of. In so doing, both writers implicitly suggest that the real history of the Mughal state began with Akbar.

Why were the reigns of Babur and Humayun, as indeed of the Afghan Sher Shah (who though not Mughal, dynastically speaking, does form a part of the epoch considered to be ‘Mughal’ in north Indian history) so unimportant? As articulated by Iqtidar Alam Khan in a brief, but rather well-known, article, the principal reason for this lay in the realm of state structure and the internal balance of power. He argued, against an earlier interpretation by R.P. Tripathi, that just as the Afghan states in northern India had contained an inbuilt tendency to fragmentation, so too the Timurid polity discouraged centralisation on account of its ‘Mongol characteristics’. These characteristics manifested themselves above all in terms of the relations between the Timurid royalty and the nobility, which was governed by customary laws derived from Chinggis Khan (yāsā-yī Chingezi or tūra-yī Chingezi). According to these traditions, it is argued, sovereignty was a shared attribute of the lineage, rather than exclusively held by a single ruler. Succession therefore led inevitably to appanage formation, which thus prevented the emergence of a strong ruler. Equally, the absence of a substantive bureaucratic tradition among the Timurids is also seen as setting sharp limits to the possibilities of centralisation. This, then, is the background as to why Humayun faced challenges from his nobility and siblings (especially Mirza Kamran) in 1538–41 and 1545–53. In turn, only Akbar, rejecting Mongol traditions and embracing more ‘the traditions and practice evolved under Turkish rulers of the thirteenth and fourteenth century’, managed ‘the transformation of the Mughal Empire into a highly sophisticated despotsm’.

Mohibbul Hasan, Babur, pp. 158–60. A particularly significant, and neglected, period, is of the rule of Sikandar Shah Lodi (1489–1517), which seems to have had significant continuities with the reforms carried out under Sher Shah Sur (1538–45).

Such an argument, which is accepted in substance (with some minor modifications) by Streusand, could in fact be reconsidered from various perspectives. First, even though Timur's empire fragmented after his death, there now appears to be a consensus that he did manage a significant transition from the relatively loose structure of the Chaghatay Khanate to a far more centralised and autocratic structure. This he did because in part he claimed to combine Chinggis Khanid tradition with divine sanction; thus, he declared as early as 1361 that what underpinned his rule was ‘the Celestial Decree and Chinggis Khanid law’ (yarlıgh-i āsamatı va tūra-yi Chingêkhâni), and the former could presumably be used to overrule the latter at times.16 Second, as Streusand too points out, the bureaucratic tradition was by no means absent among Timur and the Timurids, who made extensive use of bureaucrats steeped in Persian culture (including, not least of all, their chroniclers).17 But most serious of all is the neglect of a major struggle that was fought out between the 1560s and the mid-1580s, which could be used to test this theory of a significant transition between Humayun and Akbar. Here, I refer to Akbar’s relations with his half-brother Mirza Muhammad Hakim (1554–85), which receive but cursory attention from Streusand, as they have from earlier writers.

Mirza Hakim was born relatively late in Humayun’s life, his mother being Mah Chuchak Begam. Through most of his life, he remained associated with a particular region of what had been Humayun’s domain, namely the area around Kabul. This fact itself is not devoid of significance; Mirza Kamran had operated in much the same area, and as such it remained poorly incorporated into Mughal territories. Now, unfortunately, we have few sources that portray his struggle with Akbar from his perspective. From the viewpoint of Akbar’s court, he was an embarrassment that had to be explained away or glossed over. On two occasions, once in the late 1560s and again in the early 1580s, the latter strategy was not possible: these were moments when he came to be allied with rebels within Akbar’s domains, who had the khutba read in Mirza Hakim’s name in the course of

17 Streusand, Formation, pp. 6–8, citing Khan, Mun‘im Khan, pp. xi-xvi. Compare Streusand, pp. 29–37, where he points out (implicitly against Iqtidar Alam Khan) that Timur and the Timurids did indeed make use of a bureaucracy; however, he then goes on to argue that a major discontinuity nevertheless existed between Akbar and his predecessors, because he was the first to deny the Timurid doctrine of ‘collective sovereignty’, which usually inevitably led to ‘the fragmentation of ... empires into small, struggling principalities’ (p. 30).
Rebellions. On both occasions, Akbar had to move against him, but although defeated, he was never set aside. Implicitly, then, Mirza Hakim's hereditary right to rule over Kabul was not challenged, and he appears to have had a relatively free hand in organising revenue-assignments in the region, as well as in conducting negotiations with the Abulkhairi Uzbek state of Mavarannahr, with the Safavids (who treated him as a sovereign ruler) and with another Timurid poten tate, Mirza Sulaiman. The latter, also a neglected figure of the same epoch, was the ruler of Badakhshan, as well as Mirza Hakim's father-in-law; he eventually lost his territories to the Uzbeks and become a mansabdār with the rank of 5,000 under Akbar, dying in Lahore in 1589.18

Now, even though Akbar's chroniclers (and especially Abu'l Fazl) go to some lengths to portray Mirza Hakim as an unruly subordinate of Akbar, it is evident that his position was more complex. First, we may note that he is never treated, even retrospectively, as a Mughal amir; his biography is thus absent from Shahnawaz Khan's Ma'qṣir ul-Umarā, unlike that of Mirza Sulaiman.19 There is also no clear evidence that he ever held a mansab; on the contrary, several prominent mansabdārs are described as men who had come over to Akbar's service after his half-brother's death. In more senses than one, therefore, Mirza Hakim represented an alternative power-centre, and an alternative focus of authority and patronage to Akbar; and even if the challenge from him did not wholly mature, we cannot dismiss it out of hand. The very fact that Abu'l Fazl himself reports a discussion in Akbar's court in the early 1580s of a proposal to assassinate Mirza Hakim, and thus end the threat from him once and for all, is highly suggestive.20 It is therefore rather surprising that while devoting some attention to the abortive challenge posed to Akbar by the other Mirzas (Timurid descendants, and thus distant cousins of Akbar, usually of Badakhshani origin, discussed on pp. 102-6), Streusand wholly ignores the significance of Mirza Hakim's challenge. In particular, given his claims to posing Mughal history in a wider context, it would have been of interest to examine more closely the perception of Akbar in western and central Asia vis-à-vis his brother, through an examination of such texts as the celebrated Uzbek chronicle of Tanish al-Bukhari, 'Abdullāh Nāma (or Sharaf Nāma-yi Shāhī), as well as the diplomatic correspondence with the Safavids.21

21 For instance, in at least one letter of the period 1576–77, the Safavid ruler Shah Isma'īl II addresses Mirza Hakim as pādshāh- and masnad nishīn; for a summary of this text (from manuscript versions inter alia in British Museum, Addn. 7654, fols. 186a–187b, and Addn. 7688, fol. 128a), see Riazul Islam, ed., A calendar of documents on Indo-Persian relations (1500–1700), 2 Vols., Karachi/Teheran, 1978–82, Vol. I, letter A. 20, p. 100. Also
We might also fruitfully examine the extent to which the succession struggles that occurred during every major transition between Akbar and Aurangzeb differed in their essence from the challenges posed to Humayun by his brothers, or to Akbar by his half-brother. Contrary to what has sometimes been supposed, both the late 1650s and the period after Aurangzeb’s death witnessed proposals to divide up the Mughal empire into a set of appanages among the princes. Aurangzeb himself signed an agreement (‘ahd nāma) with his brother Murad Bakhsh, agreeing to give the latter as his rightful share of the inherited territories (mamālik-i maḥrūsa-yi mauroi), the sūbas of Lahore, Kabul, Kashmir, Multan, Bhakkar, Thatta and the territories bordering on the Sea of Oman, as part of an united front against Dara Shukoh. Again, after Aurangzeb’s death, Bahadur Shah proposed an explicit division of territories to his brother Azam Shah, ostensibly because Aurangzeb himself had so desired before his death. Now, even if these proposals eventually did not take effect, such attempts indicate that the idea of appanaging had not died by the late sixteenth century. This would of course force us to reconsider the extent to which the model of linear succession had successfully been implanted by Akbar, and his ideologue Abu’l Fazl.

The issue of appanaging is, of course, only one dimension of the problem of ‘centralisation’. What were the major institutional novelties, which permit us to assert that the Mughal state of Akbar, unlike that under his predecessors, showed an ‘extreme systematisation of administration’ (as argued by Athar Ali, for example)? This would require us to consider in some detail the extent to which the jāgīr as instituted by Akbar diverged in reality from the wajh assignment under the Lodi Sultans, or the tuyūl as used by Babur and Humayun. We would also have to re-examine the significance of the idea of the mansāb, which older writers like Moreland have seen as rooted in the earlier Mongol practice of numerical ranking (an idea that is currently out of favour). In other words, rather than accept as

significant is another letter, written a decade after Mirza Hakim’s death (June 1596), by Akbar to ‘Abdullah Khan Uzbek, in Islam, ed., Calendar, Vol. II, letter Tx. 334, p. 221. Here Akbar reassures the Uzbek ruler regarding the fate of Mirza Hakim’s children, whom he had taken into custody.

22 For the proposal to divide the empire in the 1650s, see Aurangzeb’s letters of the period, in Shaikh Abul Fath Qabil Khan, Ādāb-i ‘Alamgīrī, edited by Abdul Ghafur Chaudhari, 2 Volumes, Lahore, 1971. Vol. I, pp. 374–76; Vol. II, pp. 791–92. On the projects of the early 1700s, cf. for example, the discussion in William Irvine, Later Mughals, Calcutta, 1922, pp. 21–22, section entitled ‘Bahadur Shah’s letter to Azam Shah and the latter’s reply’; this is based in part on Kamwar Khan’s chronicle, cited in note 47 below. I am grateful to Muzaffar Alam for these references.

23 W. H. Moreland, ‘Rank (Mansab) in the Mughal State Service’, Journal of the Royal Asiatic Society of Great Britain and Ireland, 1936, pp. 641–65; this view also finds mention in Athar Ali, Mughal Nobility, pp. 38–40. However, subsequent works from Aligarh take a different line, probably on the grounds that if the Mongols and Timurids in general lacked
a postulate that Akbar's institutions were created *sui generis*, we might speak of an evolving tool-box of contemporary statecraft, from which a set of institutions were improvised and partly innovated. This would enable us, to start with, to place less of the burden of historical explanation on the ruler's 'genius'.

This, however, is not where Streusand's interests lie. Rather, having accepted as a postulate the notion of a sharp discontinuity in the nature of the state between Akbar and his predecessors, his main thrust is two-fold. First, he wishes to examine (in Chapter 3 of his book) whether Akbar's conquests and successful attempts at centralisation were the result of the introduction of firearms—that is, the so-called 'Gunpowder Empires' hypothesis of Marshall Hodgson. Second, having provided us in the following chapter with a fairly conventional political history, dealing with the years from 1556 to about 1570, Streusand devotes space to 'the definitive reforms' of Akbar, which dated to the years 1572–1580, when 'Akbar's empire became recognizably Mughal' (p. 108). This requires a description of *mansābdarī* and a discussion of the *mahzār* of 1579, leading to the development of the idea of an 'Akbari constitution', to be inferred largely from court-ritual, and Abu'l Fazl's writings on sovereignty. On the basis of an examination of court-ritual, Streusand attempts to demonstrate a rather obvious point about the 'syncretic' nature of the ideology under Akbar, and his use of 'Hindu' elements derived from earlier polities. All the while, he stresses that his intention is not to bring to light new documentation, but rather to read standard primary materials (such as Abu'l Fazl's *Akbar Nāma*, 'Abd al-Qadir al-Badaoni's *Muntakhāb ut-Tawārikh*, or Nizam al-Din Ahmad's *Ṭabaqāt-i Akbarī*), as well as the secondary literature afresh.

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Concerning the ‘Gunpowder Empires’ question, Streusand’s conclusions do not wholly support Hodgson; he argues from brief descriptions of Akbar’s sieges of Chitor, Ranthambor and Kalinjar that artillery played no great role in his success in siege warfare. However, his later assertion (p. 67) that ‘firearms contributed to centralization, the distinguishing characteristic of the gunpowder empires, in a more complex way’, winds up confusing the issue. By his own admission, the Mughals at the second battle of Panipat in 1556 ‘apparently had no guns’ (p. 53); and guns are seen as irrelevant in one of the only two other battles examined, Tukaroi (1575), and Haldighati (1576). To argue, as Streusand does at one point, that the ‘narrow margin of victory’ in some of these battles ‘shows that the Mughals needed the combination of artillery and mounted archers to win easy victories’ (p. 56) is a specious form of reasoning; what he in fact needed to demonstrate were instances where firearms did indeed make a great deal of difference. This he does not do, even in the case of Haldighati, which in his own words ‘meant nothing’ as an engagement anyway. At the end of a thirty-page discussion, we are hence none the wiser on the question.

On the issue of the reforms of the 1570s and the ‘Akbari constitution’, Streusand concludes that the official ideology under Akbar did include significant ‘Hindu’ elements in it, and that this was because the Mughal state was hybrid—Islamic at the centre, but Hindu at the periphery: thus, ‘an Ottoman Sultan would have found the central bureaucracy familiar; a Chola Rajah would have understood the limited imperial role in the provinces’. The conclusion therefore is that ‘the Mughal government [w]as an imperial centre supported by a shifting structure of segments’ (p. 181).

It is only natural, in view of this, that towards his concluding paragraphs, as well as earlier in his book, Streusand pays obeisance to Burton Stein’s ‘segmentary state’ formulation, arguing that it may not be wholly inappropriate in the Mughal context (albeit with some modifications). In effect, the substance of his conclusion appears to be that despite having undergone a process of centralisation, the Mughal state as a structure remained, at the time of Akbar’s death, less centralised than say the Ottoman state. This was, he argues, largely the result of the fact that in the years following the ‘great revolt’ of 1580–82 in the eastern part of the realm, ‘Akbar compromised the principles of centralized government which he and his closest advisers shared’ (p. 178). The result, in his view, was the resort to the jāgīr system, and Streusand maintains that the failure of centralising forces is clearly manifested in the execution in 1581 of Khwaja Shah Mansur Shirazi, who had been appointed wazīr in 1578, on false accusations of rebellion (and loyalty to Mirza Hakim!) (pp. 166–70).

If we compare his monograph to the Aligarh paradigm outlined above,

25 Burton Stein, Peasant State and Society in Medieval South India, Delhi, 1980.
then, Streusand appears to depart from it in certain respects. The extent of Mughal power and the extractive nature of the post-Akbar state do not come through as clearly in his work as in those other writings. Further, an attempt is made to bring in ideological elements, as well as court-ritual, in what is clearly the result of the influence of Chicago-based 'anthropological history' (we may note here that the book, like that of Blake is based on a Ph.D. dissertation from the University of Chicago). Again, the zabt system, which Athar Ali has described as 'the characteristic institution of Mughal revenue administration', gets little space in his analysis, as indeed do matters economic in general. The economic significance of the incorporation of Gujarat, Bengal and Sind into the Mughal domains between 1570 and 1595, for example, is scarcely touched upon, and the focus remains very much on imperial court and centre. This disregard for the relationship between central state and region, and indeed for spatial analysis in general, also characterises Blake's formulation, discussed at greater length below.

Ideology, Islam and the Millenium

It would appear on reflection, however, that even in his analysis of ideology under Akbar, Streusand has chosen not to depart very far from the well-trodden path. We have already noted this in our discussion of the transition between Humayun and Akbar. In this context, it is well-known that the 'Aligarh school' sees Abu'l Fazl as far and away the most important thinker of the epoch, and the Akbar Nama (and its segment the Ain-i Akbari) as the master text for an understanding of the period. The chapter on Rawâ'i-yi râzî in the Ain is presented as a 'rational' theory of kingship, based on the 'social contract', which hence enabled Akbar to cut loose from the religious elite. It also seems to be the case that Abu'l Fazl wished to replace the theory of succession as a right based on descent (which would still leave sovereignty as a shared attribute) to one based on a wider variety of qualities besides blood and descent. In a passage of the Akbar Nama, aimed directly at ridiculing, albeit ex post facto, the 'simple' Mirza Hakim's pretensions, he states:

Kingship is a gift of God, and is not bestowed till many thousand grand requisites have been gathered together in an individual. Race and wealth and the assembling of a mob are not enough for this great position. It is clear to the wise that a few among the holy qualities (sifât-i qudsî) are, magnanimity, lofty benevolence, wide capacity, abundant exuberance, exalted understanding, innate graciousness, natural courage

justice, rectitude, strenuous labour, proper conduct, profound thoughtfulness, laudable overlooking and acceptance of excuses .... Thanks be to God! The holy personality of the Shahinshah [Akbar] is a fount of perfect qualities, and a mine of holy principles.²⁷

Presumably the same could not be said of Mirza Hakim, who was hence destined to fall into the ‘whirlpool of destruction’! Now this idea, together with the theory of ‘divine effulgence’ (farr-i īzīdī) to the ruler, and of a notion of kingship whose raison d'être stems from a sort of social contract, make up the key elements of Abu'l Fazl’s formulation. It is certainly true that aspects of this formulation were adopted later by Jahangir, by Shahjahan (who referred to himself as the shadow of God, sāya-yi khudā, in dealings with the Deccan Sultanates), and subsequently even used in the latter half of the seventeenth century by the opponents of the Mughals (such as the Marathas) to question the legitimacy of particular rulers. Again, this type of formulation finds mention even in the early eighteenth century, in the context of dealings between the Mughals and their Hindu subjects over sensitive matters of religious practice. On the other hand, it is also often forgotten that the Ā’in-i Akbarī was produced rather late in Akbar’s half-century long reign. In the earlier decades, other ideological avenues had been explored. The unfinished and anonymously authored chronicle, Tārīkh-i Khāndān-i Timuriyya, begun in the 1580s, sought for example to stress precisely the Timurid aspects of Akbar’s patrimony, inexplicable if the ruler had been trying to divest himself of this ‘Turko-Mongol’ baggage.²⁸ This chronicle later attracted the attention of Akbar’s grandson Shahjahan, who—incidentally—was keen for his part to reassert his Timurid patrimony in the context of territorial expansion in the direction of Central Asia.

More important even perhaps than the chronicle mentioned above for a proper understanding of the evolution of ideology under Akbar is the Tārīkh-i Alfi, which finds but brief mention in Streusand’s work, possibly because it remains in manuscript form.²⁹ This chronicle was initially conceived of, in 1581–82, as a joint project of several authors under state supervision, but was eventually written up largely by two chroniclers: Mulla Ahmad Thattawi and Asaf Khan Ja’far Beg. As summarised by S.A.A. Rizvi, the Tārīkh-i Alfi was meant by Akbar to commemorate the millenium of the Islamic calendar (in 1591–92), and began not with the


²⁸ For the Tārīkh-i Khāndān-i Timuriyya see Maulavi Abdul Muqtadir, Catalogue of the Arabic and Pèrsian Manuscripts in the Khuda Bakhsh Oriental Public Library, Vol. VII (Indian History), reprint, Patna, 1977, pp. 40–48; for some illustrations from this text, see Mughal Art of Miniature Painting at its climax, Patna (Khuda Bakhsh Library), 1984.

²⁹ Streusand, Formation, p. 133; for an earlier brief mention, see Harbans Mukhia.
Timur (thus, it was unlike the *Tārīkh-i Khândân-i Timuriyya*), but with the rihlat (death of the Prophet Muhammad). Brought up to the reign of Akbar, but eventually abandoned in favour of the *Akbar Nāma* as the official chronicle of the reign, the *Tārīkh-i Alfi* presents us with something of a puzzle. If it is indeed, as is sometimes claimed, a purely ‘rational’ work of history, it is difficult to understand why the miracles attributed to Akbar find a place in it, in the sections written by Asaf Khan. From internal evidence, it appears rather to have been a work aimed at posing Akbar within the framework of Islam, as the *Bādshāh-i Islām*, as superior to other famed heroes of the Islamic tradition like Saladin, as a monarch disposed to resolving difficulties between Shias and Sunnis, as well making sure that ‘kāfirs are shouldering the burdens of Islam’.

This is a somewhat different image of Akbar than the ‘secular’ and radically-minded monarch who syncretised Islam and Hinduism; this Akbar is a preserver of tradition, who seeks to remove unauthorised innovations and new regulations in religious practice, and at whose command ‘wolves perform the task of shepherds’.

Such an image may have been meant to be divulged not within India alone, but even to Akbar’s competitors, the rulers of the Safavid and Ottoman states, and the Uzbek state of Mavarannahr. Of these the Uzbek factor is a crucial one, and therefore surprisingly neglected in the literature. We are aware that the Uzbek ruler ‘Abdullah Khan proceeded in stages to consolidate his power, over a period roughly conterminous with Akbar’s own reign. Between 1551 and 1556, he was challenged by, but eventually bested, his rival Nawruz Ahmad Khan. Then, in 1557, he captured Bukhara and made it his capital, going on between 1573 and 1583 to take Balkh, Samarqand, Tashkent and Farghana. It was only in 1583, however, that he formally assumed the title of ‘Khan’, in place of his father, Iskandar. Between the late 1570s and the late 1590s, ‘Abdullah Khan appeared to be a dangerous foe, a relatively orthodox Sunni monarch, who ran an increasingly tight fiscal system. Thus, once ‘Abdullah Khan had emerged dominant over rival clans, the Mughals had to treat with him; and it should be recalled that in the early 1580s, Akbar maintained a correspondence with the Uzbek ruler, which was intended partly to counter the latter’s territorial ambitions and dealings with Akbar’s half-brother, Mirza Hakim, who formed a sort of buffer between Akbar’s domains and those of Mavarannahr. The dealings with ‘Abdullah Khan also had another


dimension: as a gambit in Akbar's jockeying for status vis-à-vis the Ottomans (with whom too the Uzbek ruler had dealings), for in at least some of the letters, Akbar portrays himself as very much the Islamic ruler, writing of the need, for instance, to prosecute a jihād against the Portuguese, who were imped ing access to Mecca and Medina.\textsuperscript{31}

To understand the larger west and central Asian context in this period, we may equally note that in these years, on account of the approach of the Hijri year 1000, a millenarian consciousness gripped many in the Islamic world; these included prominent Ottoman intellectuals, who—in partial contrast to the Mughal situation—combined it however with a sense of political, economic and moral decline, seeing in the millenium a moment of apocalypse.\textsuperscript{32} In the Mughal domains too, millenarian feelings were not absent, and in the course of the sixteenth century, several minor millenarian (so-called Mahdawi) movements had been suppressed by Afghan rulers, especially Islam Shah Sur. While maintaining distinct theological positions, several other movements took advantage of the prevailing ambience to gain leverage and legitimacy. For example, the Naqshbandi Sufi, Shaikh Ahmad Sirhindi (1564–1624), while disassociating himself wholly from the Mahdawis, nevertheless claimed to be the mujaddid-i alf-i ṣānī, or 'renovator' of Islam of the second millennium.\textsuperscript{33} Sirhindi's claims are normally posed in the historiography as an orthodox ideological force that reacted to Akbar’s heterodoxy. But it is possible that as late as the 1580s, Akbar—despite having already committed himself to wooing his Rajput constituency—had not given up seeking an alibi for himself in Islamic terms. However, once into the 1590s, as the Uzbek threat receded and the international context seemed less propitious, he may have decided to take the rather safer tack offered to him by Abu’l Fazl’s formulation, and hence abandoned the Tārīkh-i Alfī project—as well as its larger implications.

All this is admittedly within the realm of speculation. But it is plausible speculation, of a sort that is largely absent in Streusand’s work, which instead remains content to reiterate the obvious on such points. Akbar’s


adoption of *ṣulḥ-i kull* (Absolute Peace) as a principle has attracted much attention, but we may recall that even this was propagated in part through recourse to a structure of disciples reminiscent of a Sufi order. The practice of terming the Mughal ruler *pir-o murshid* brings us echoes of the relationship between the Safavid Sultans and their Qizilbash followers (and more particularly the so-called *ṣūfiyān-i Lāhejān*). Thus, the larger west and central Asian context was never far, despite the implicit denial of borrowings across state boundaries that one often encounters.

**The patrimonial-bureaucratic state**

If Streusand’s writings offer us little that is startling or new in the sphere of state ideology, what of the structure of the state itself? I have already made mention of his nod in the direction of Burton Stein’s segmentary state formulation as a possible understanding of the Mughal state. But elsewhere, his views are more of a piece with Stephen Blake’s Weberian model of the ‘patrimonial-bureaucratic state’, which the latter defends at length in his monograph *Shahjahanabad*. According to Blake, small ‘traditional’ states, are often based on the idea of assimilating state to household, so that the ruler ‘attempted to administer, control, and finance the entire realm as if it were part of his own private domain’ (p. xii). As states grew larger, however, a ‘compromise of the patrimonial ideal’ had to be undergone, and a bureaucracy brought in, thus giving rise to the ‘patrimonial-bureaucratic empire’. In such empires between 1400 and 1750, moreover, one had a ‘sovereign city’ as a capital, which was ‘the kingdom in miniature’. In this city, everything was dominated by the imperial and noble households: the urban economy, cultural life, the structure of society (p. xiii). In brief, the Mughal state of Blake encompassed everything in the sovereign

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city, and since the city was the kingdom in miniature, it seems to follow that it encompassed everything in the kingdom as well. This model, if read literally, appears to be the Aligarh model of centralisation taken to its very extreme. Reconciling it with the ‘segmentary state’ hence presents us with more or less insurmountable conceptual difficulties. Streusand suggests a vague solution: a patrimonial-bureaucratic central structure, petering out into an increasingly weak hold over the countryside. But the two models are so diametrically opposed that the very idea of a ‘weighted average’ between the two seems untenable.

It is of course true that evidence in favour of viewing the Mughal state as ‘segmentary’ can be found. Let us consider a specific set of sources, namely the travel accounts of contemporary Europeans. Several of these writers, like the Dutch merchant Francisco Pelsaert (1595–1630), and the French physician François Bernier (1620–1688), can hardly be used to buttress the view of the Mughal empire as centralised at its ‘height’ (viz. the seventeenth century); in fact Bernier, in the quotation cited at the outset of this article, stresses the limited nature of Mughal power, as does Pelsaert in his Remonstrantie written somewhat earlier. However, while ignoring or suppressing this particular facet of Bernier’s writings, modern historians like Athar Ali and Irfan Habib are not in the least reluctant to accept a good part of his other observations on such issues as jagirdari and its implications, the billion inflow into India, the roots of crisis in Aurangzeb’s reign, and so on.

The key text that is used is Bernier’s letter to Louis XIV’s minister Jean-Baptiste Colbert (1619–1683), written in the late 1660s. This letter concerns four issues, (a) the ‘extent of Hindoustan’, (b) the currencies in use, and the ‘absorption of gold and silver’ there, (c) ‘resources, armies, and the administration of justice’, and (d) ‘the principal Cause of the Decline of the States of Asia’. In the latter half of this letter, we find the first articulation of the theory of the ‘agrarian crisis’ which stems from the inherently unstable nature of jagirdari; this view was first taken on board, among modern writers, by W.H. Moreland, and then subsequently by the historians we have mentioned above. It has, however, been pointed out

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on more than one occasion by other recent writers that Bernier's writings (like those of his contemporary Jean Chardin on the Safavids), have a specific rhetorical purpose in relation to his correspondents in France. In particular, Bernier wished to criticise revenue-farming (which also existed in contemporary France) as a principle, and equally to make an argument for the absolute need for security in private property in land, which he presented as more or less absent in the Mughal India of the period. By so doing, Bernier clearly intended to influence the policies followed by Colbert in France: Mughal India was thus the screen on which he presented his views of what would happen to France if certain despotic and tyrannical policies were followed.

But Bernier's conception also fits in quite neatly with the idea of the 'segmentary state'. Among the 'peculiar chiefs or sovereigns' who lived under Mughal rule, he counts a large list, starting with the ' petty sovereignties bordering the Persian frontier', and going on to the Pathans, the rulers of Bijapur and Golconda, and finally the Hindu Rajas—especially the Rajputs. As Bernier saw it then, the Mughal state structure could be divided vertically into two. First, there was a superstructure, comprising a 'tyrant', who however 'finds himself in a hostile country, or nearly so' (p. 209), and who hence has need of 'numerous armies', so that he can 'maintain himself in such a country, in the midst of domestic and powerful enemies'. But below this level is quite another, of the 'native princes' (princes naturels), subordinated on the one hand to the Mughals, and on the other hand to the Brahmin priests, who keep them subject to the Gentile religion. Thus we have (a) numerous centres or political domains, (b) differentiated political power and sovereignty, so that the 'native princes' wield 'appropriate power', and the Mughals 'full, royal sovereignty', (c) autonomous administrative capabilities and coercive means with the 'native princes', and (d) the recognition by these centres, through ritual and other forms, of Mughal authority. Since these four characteristics define, for Burton Stein, what is a 'segmentary state', Bernier's Mughal kingdom certainly is one.

But casting our net somewhat wider in terms of sources and testimonies, problems begin to arise. There is, first of all, the extensive use by the Mughals of prebends (jāgīrs), which has no clear place in the segmentary conception. Nor indeed do the directly-taxed Crown lands (or Khālisā), whose extent tended to vary quite substantially over time, and which could be quite dispersed. But if the segmentary conception is of little help here,
the ‘patrimonial-bureaucratic’ state proves something of a red herring as well. The \textit{khāālisa} lands scarcely represent a ‘patrimonial’ element, nor despite the association of \textit{jāgīrdārī} with \textit{mansabdarī} can the \textit{jāgīrs} be termed a recompense to a ‘bureaucracy’. A recent study by Chetan Singh with a very different perspective to that of Streusand and Blake, which considers the Mughal state from the viewpoint of a region (namely, Punjab), concludes that Crown officials may have tended as early as the seventeenth century to acquire local roots, modifying the notional ‘rule’ of periodic transfers which suggests a relatively bureaucratic system at work.\footnote{Chetan Singh, ‘Centre and Periphery in the Mughal State: The case of seventeenth-century Panjab’, Modern Asian Studies, Vol. XXII, (2), 1988; also see his \textit{Region and Empire: Panjab in the seventeenth century}, Delhi, 1991, pp. 31-44. My own examination of Sind, and the \textit{sūba} of Thatta, suggests that members of the family of Mirza Jani Beg persistently gravitated to the region as Mughal officials, until at least the mid-seventeenth century. Indeed, if the \textit{mansabdarīs} were ‘bureaucrats’, there is little sense in their depiction by ‘Aligarh school’ historians themselves (in particular Athar Ali) as the ‘Mughal nobility’! At best then, they were a sort of \textit{noblesse de service}.}

Besides, the ‘patrimonial-bureaucratic’ state is again bound up closely to the conception of a Mughal state structure which is already defined by the end of Akbar’s reign, and the logic of which works itself out through the seventeenth century. Like the segmentary state as used by Streusand, it has little room for the depiction of a painfully improvised process of state building, whether at the level of the shifting ideologies existent in different epochs, or the incorporation and modification of regional traditions, or the expansion of the agrarian frontier, of trade and manufacture, and the creation of new opportunities which had an impact on the essential character of the Mughal state.\footnote{For a reformulation that attempts to take these aspects into consideration, see Alam and Subrahmanyam, ‘State-building in South Asia and the Mughals.}\ In sum, these views are excessively focused on \textit{structure} and neglect the historical \textit{process}. In this sense, they cannot provide a fundamental challenge to the paradigm of the ‘Aligarh school’, which is again based on treating the Mughal state as a fixed structure, a ‘system’ created under Akbar.

**The omnipresent state**

Whether one agrees with his conception of the Mughal state or not, much remains that is potentially of interest in Blake’s \textit{Shahjahanabad}. After a brief opening chapter on Delhi’s other cities, and his ‘patrimonial-bureaucratic’ theory of state, Blake’s book passes on to the longest of the seven chapters, one entitled ‘Cityscape’. This is followed by others, entitled ‘Society’, ‘Economy’, ‘Courtly and Popular Culture’, ‘Aftermath of Imperium’, and ‘Comparison and Conclusion’. The first four of these chapters promise to provide insights into a Mughal city of a type that does not exist in the literature; only a handful of articles and the odd monograph...
deal with the urban history of the epoch. The centrepiece of the exercise is naturally a detailed map, which is to be found on pages 72–73, and which depicts Shahjahanabad in 1739. To one's surprise, however, one discovers that this map is in fact based on a mid-nineteenth century Survey of India outline, with details filled in from earlier evidence. Thirty-seven individual structures are identified within the city, and described rapidly (pp. 75–82), sometimes in one or two lines. A sense of change and chronology remains absent: modifications in the city between 1639 and 1739—the rise of new quarters and the fall of others, the shifting relationship between Shahjahanabad and its suburbs (such as Jaipura, or the Shia settlement, even today called Karbala, where Safdar Jang chose to be buried in the eighteenth century) are never plainly set out, even if the suburbs are described (pp. 57–66).

Nevertheless, the 'Cityscape' chapter is an important one, more carefully done than one can encounter in the normal writings of historians of Mughal India. No comparable study of Aurangabad or Lahore exists, though Fatehpur Sikri and even Agra are relatively well-served.44 It revisits a theme that Blake had already touched on in his paper in Delhi through the Ages, edited by R.E. Frykenberg, and also sets the stage for the chapters that follow, in the sense of providing the spatial context for social, economic and cultural interactions among the residents of the city.45 As in his earlier essay, Blake seeks to argue that 'the plan of Shahjahanabad reflects both Hindu and Islamic influences' (p. 32). This seems quite likely, but the author's belief that the architects of Shahjahanabad had access to specific Sanskrit treatises like the Manasārā (from the fifth to seventh century) is the purest speculation. Indeed, both Blake and Streusand have somewhat ahistorical views on what constitutes 'Hindu thinking' on statecraft and kingship in the period, and are for the most part content to assume that an immutable classical model (such as that set out by Ronald Inden), holds good for all time—even if no reference to it can be found in the texts of the Mughal period.46 Equally, the references to how Shahjahanabad's structure

44 On Agra, see I.P. Gupta, Urban Glimpses of Mughal India: Agra, the Imperial Capital (16th and 17th Centuries), Delhi, 1986, which for some reason does not appear in Blake's bibliography; on Fatehpur Sikri, see inter alia, Michael Brand and Glenn D. Lowry, eds., Fatehpur Sikri, Bombay, 1987. On Lahore, we have the useful but rather brief discussion in Chetan Singh, Region and Empire, pp. 177–84.


46 Cf. Ronald Inden, 'Ritual Authority and Cyclic Time in Hindu Kingship', in J.F. Richards, ed., Kingship and Authority, pp. 28–73. For a more historical approach to the issue of kingship in one set of 'Hindu' states, see Velcheru Narayana Rao, David Shulman and Sanjay Subrahmanymam, Symbols of Substance: Court and State in Nayaka-period Tamil Nadu, Delhi, 1992. Inden's own position on such matters as historicity seems to have changed somewhat in his more recent writings, cf. his discussion of the Rashtrakutas in Inden, Imagining India, Oxford, 1990.
reflects ‘traditional Islamic architecture’ seem laboured, and consist of citing general texts (Sayyed Hossein Nasr on cosmological doctrines, and von Grunebaum on Islam), as if these contain some eternal verities.

The chapters that follow (‘Society’, ‘Economy’, and ‘Culture’), are all uniformly marked by a single-minded focus on the state. Blake’s understanding of society in Shahjahanabad is defined, by his own admission (p. 83) through his notion that ‘the metaphors of sovereign city as mansion and patrimonial-bureaucratic empire as household’ are crucial. He chooses to examine the whole of the social fabric through the imperial household and the households of the amirs, with the rest of the population entering the picture only as the clients of one of these two. He assures us that ‘the patron-client relationships between the emperor and the great men and between them and the members of their households bound the entire city together in a kind of vast extended family’ (p. 103). The state here has, then, swallowed up society.

This leaves little scope for the economy, or for that matter culture. The economy is dealt with in a very brief eighteen pages, where changes in the economy over the century 1639–1739 find little place. Rather, the text is devoted largely to brief descriptions of workshops (kārkhanās), and to the appositeness to the Mughal case of a particular anthropological model of premodern economic organisation. Again, the state looms large. According to Blake, ‘in Mughal India, the emperor considered the economic resources of both city and empire his personal preserve’. To be sure, compromises had to be effected on occasion, but ‘the ideal of running both city and empire as one big household workshop remained and was never abandoned’ (p. 121). Thus, the ‘economy’ too is apparently little more than an appendage of the state. We are assured that ‘elite households, by virtue of their wealth and status, completely dominated the economic process in Shahjahanabad’; as if that were not enough, we are even asked to believe that ‘a substantial share of the wealth of the empire—over 40 per cent in fact—was concentrated in Shahjahanabad’ (p. 131)! The manner in which this conclusion is arrived at is simple: standard calculations of the share of the mansabdārs in jama‘ are used, together with the notion that if they maintained residences in Shahjahanabad, their entire ‘wealth’ must have been there!

There follows the chapter on ‘Courtly and Popular Culture’, where one might have expected to escape the shadow of the state. Of course, this cannot be the case in the chapter’s first half, on courtly culture, which deals with the upbringing and training of nobles, divided into ‘men of the pen’ (ahl-i qalam) and ‘men of the sword’ (ahl-i saif). Evidence is largely anecdotal, and intended to show that the two categories did not remain wholly watertight. The description of ‘popular culture’, which occupies the second half of this chapter is drawn almost exclusively from a single source, the ‘Risalah-i Salar Jung’, written by Dargah Quli Khan in the early
1740s. Now, the greater part of this text is simply not about 'popular culture', as generally understood. Khyal and Dhrupad singers, acting troupes which performed for the imperial household, and musicians who performed on the bin and other instruments of art-music, did so very often within the aegis of elite patronage, and Dargah Quli Khan was obviously himself one such elite patron. However, Blake is clearly unfamiliar with the traditions within which such activities can be located, which is further confounded by numerous misreadings of the manuscript. On occasion, Dargah Quli Khan does enter into what can truly be called the 'popular culture' of eighteenth-century Delhi; but to group art-music, as well as the poetry of Mirza Bedil and Mirza Mazhar Jan-i Janan under the head of 'popular culture' suggests either that Blake is providing a radical redefinition of the spheres of 'elite' and 'popular', or that from his perspective the 'popular' can be subsumed under the elite. Or, to put the matter somewhat differently, not even the 'popular culture' of Delhi can escape the clutches of the state in this treatment; the state is not merely omnipotent but omnipresent.

The last substantive chapter, 'Aftermath of Imperium', follows inevitably from this logic. It is argued that after Nadir Shah's attack on the city in 1739, there followed 'a dismal and dispiriting time', lasting till 1803, leading in turn to a period of 'peace and healing' (p. 161) which continued to 1857. Since it has already been argued in earlier chapters by Blake that the city is no more than a mirror of the State, it naturally follows that the first period—when the Mughal state entered into decline—would have led

Footnotes 105 to 175 of the chapter refer solely to this text (to which there had already been some twenty references in the first 100 footnotes). For some reason that remains obscure, Blake refers to the manuscript of this text in the British Museum, apparently unaware that at least two editions, besides translations into Urdu and English exist of the sections describing Delhi in this work, under the title Muraqqa'-i Dehli. For editions of Delhi sections of the 'Risāla-yi Sālār Jang', see Dargah Quli Khan, Muraqqa'-i Dehli, ed., Hakim Sayyid Muzaffar Husain, Hyderabad, 1926; the later edition, with an Urdu translation, by N.H. Ansari, Delhi, 1981; most recently, the translation into English by Chander Shekhar and Shama Mitra Chenoy, Muraqqa'-e-Dehli: The Mughal Capital in Muhammad Shah's Time, Delhi, 1989, which however has a rather awkward flavour and some inaccuracies. Had Blake consulted these editions, certain obvious errors could have been avoided, on which more in note 48 below. This is not the only instance in which Blake cites manuscript versions of Persian texts, while ignoring critical editions that collate several manuscripts. Other examples include Mirza Sangin Beg, Sair ul-Manāzil, ed., S.H. Qasimi, New Delhi, 1982; Bakhtawar Khan, Mirat ul-'Alam, ed., Sajida Alavi, Lahore, 1979; Muhammad Hadi Kamwar Khan, Tażkīrat us-Salātīn Chaghtā, ed., Muzaffar Alam, Bombay, 1980.

A few examples will suffice here. In the text, there are curios explications such as that khyāl (sic) is 'a kind of song', and that gawwāl (sic!) is 'a special kind of singing' (p. 157). As for misreadings, these are legion: the singer 'Rahimsen' appears as 'Jimsen'; a dancer 'Kali Ganga' appears as 'Kaki Kanka'; at one point, the description of two singers Jani and Ghulam Rasul is conflated with that of one Baqir Tamburchi (which appears next in the text); and a certain Hasan Khan Rehabi is found to be an 'orthodox Muslim', when Dargah Quli Khan says no more than that God should pity him in his poverty!
Shahjahanabad to decline, while the latter period—when British power established itself in the city—would bring 'peace and security to Shahjahanabad, and a slow, gradual return to health' (p. 169).

The missing merchant

One of the reasons why the state must carry such a burden in Blake's formulation is because of his belief in the extremely weak position of other sections in urban society, in particular merchants. Since cities in the period were usually major centres of exchange (with production probably being less important, relatively speaking), one expects to find in any urban history a detailed discussion of merchant activity, the more so since sources on the issue are far from absent. Thus, ideally, one would have expected to see Shahjahanabad as a centre, linked to smaller provincial towns in the sība of Delhi, to other major cities on the trade route which ran via the Punjab to central and west Asia, to the centres of the Gangetic valley, and via Rajasthan to the ports of the west coast and Gujarat. However, while speaking of a 'hierarchy of central places' in some sections, Blake's analysis does not take him in this direction. Instead, following what is by now a well-established (but seldom investigated) tradition in Mughal studies, he posits that merchant activity is not worth considering because a gulf existed between the worlds of trade and politics; the 'merchants of Mughal India and Shahjahanabad ... [were] lacking in power and influence' (p. 111). Not only this: 'In Mughal India, merchants were not protected by the Mughal government. They were subjected to illegal tolls and taxes, robberies were common, and arbitrary payments were often demanded. In fact, many of traders (sic) expected to be plundered by rulers' (p. 110). From this follows the familiar next stage in the argument:

It was not until the English East India Company began to extend its control over the subcontinent in the late eighteenth and early nineteenth centuries and introduced the principles of private property, sanctity of contract, and rule of law that a true market economy hospitable to merchants began to develop (p. 112).

Of course, not all clichés are false. However, in the instance at hand, Blake's views are of some importance for his larger constructs, for justifying his cavalier treatment of merchant communities in the city, and for his view of the exchange economy as no more than an appendage of the households of the ruler and amirs (pp. 116–20). It underpins blanket assertions, such as that 'the merchants who staffed these markets, many of whom were Khattris, should be seen as clients, members of elite households, and not as independent businessmen' (p. 117)—for which Blake cites no reference in contemporary evidence. In the final analysis, this view of
merchant activity seems to rest on a partial reading of secondary literature, and a single piece of evidence, showing that none of the dated Hindu temples in Shahjahanabad were built between 1639 and 1739—which apparently Mughal intimidation of Hindu merchants (pp. 110-11)! Nor is it free from internal contradictions. If the ruler and amīrs in seventeenth century Mughal India were so deeply implicated in trade that markets were offshoots of their households, it is hard to explain the ‘lack of interest in trade’ on the part of these very groups, that Blake also finds to be the case (p. 111). Instead of this rather unsatisfactory analysis, one could suggest an alternative line as follows. Dealings between the Mughal state, nobles, and the trading economy took several forms. First, mansabdārs themselves at times took a substantial interest in trade, owning shipping in ports such as Thatta, Hugli and Balasore, and on occasion practising privileged trade (saudā-yi khāṣ). Such was the case with Asaf Khan, Shayista Khan, Muhammad Sayyid (Mu'azzam Khan) and others. Members of the royal household, including Shahjahan himself, his daughter Jahanara, and others, also were engaged in maritime trade, as were Dara Shukoh and Aurangzeb as princes. Such trade required the use of agents, but also implied a view of the world in which trade was not irrelevant or marginal. Whether or not customs-revenues were a large proportion of land revenues is for the most part irrelevant in this instance, for it is not the fisc but the economy of mansabdāri households that is at issue here. At the same time, once we accept this view, we are better placed to understand Mughal expansion in Gujarat, Bengal and Sind, the capture of Hughli from the Portuguese in 1632, or the later campaigns against Chittagong."

There was a second form of interaction between the state and trade. Mercantile networks facilitated the flow of goods and liquid resources in a way that was potentially advantageous to the Mughals. There is no doubt that from the early seventeenth century, hundi networks between Surat and Ahmedabad, and Agra, were extensively used by Mughal officials. Later in the same century, the fiscal resources of Bengal also came to be transferred to the capital in the same way, through existing channels of trade. It is possible that individual jāgīrdārs used similar means to have money remitted from their jāgīrs to their personal headquarters. These mercantile networks brought in bullion to feed Mughal mints, helped promote the expansion of commercial agriculture and manufacturing for distant markets, and even linked together regions at moments of scarcity. Even though the Mughal state was not gerry-built on these networks (as has been argued by some), it was nevertheless not the case that autonomous mercantile activity was anathema to the Mughals. Historians often cite the

seventeenth century memoir, *Ardhakathānaka*, of a Jain merchant Banarasidas, as evidence of the hostility of the Mughal state to merchants.50 Yet, an examination of a number of contemporary European instances shows that it was not unknown for middle-level state officials to extract protection from merchants even in those states. The issue is whether such exactions acted as a substantial check on merchant activity in general, and on the accumulation of mercantile profits, and in general caused merchants to cower within the safety of four walls. Historians of trade in India will be inclined to view this portrayal, which is at considerable variance with recent research, with some scepticism.51

### Wider horizons

The writings of both Streusand and Blake are characterised by a common feature: the explicit desire to pose the Mughals in a wider context. This is a laudable objective, though obviously these wider horizons could have been as much within south Asia as outside of it. For example, given the recent rash of publications on the city of Vijayanagara in south India, it might have been an interesting exercise to compare it to Shahjahanabad.52 Again, in the case of Streusand’s analysis, it might have been of some utility to analyse ideas on kingship deriving from elsewhere in south Asia in the sixteenth century, rather than resting content with references to the ‘classical Hindu’ model. To take the example of Vijayanagara once again, during the period of Krishnadevaraya (r. 1509–29), an enormous literature was produced at the court, including some attributed to the ruler himself. Since many elements of the popular image of Akbar in north India, including the dealings between him and Birbal, and the idea of the navaratna, bear a close resemblance to myths about Krishnadevaraya’s court, this comparison remains an avenue with some potential.53


Blake in the concluding chapter of his book does not look so far south, but he looks both east and west. The cities with which he chooses to compare Shahjahanabad are, within Mughal India itself, Agra, to the west Istanbul and Isfahan, and to the east Edo and Peking. The comparison occupies less than thirty pages, and follows a set pattern. In each case, the general features of the states—the Ottoman Empire, Safavid Iran, Tokugawa Japan, and Ming and Ching China—are set out, followed by a description of the city, and then an assertion of the general applicability of the 'patrimonial-bureaucratic' model, and its concomitant, the idea of the ‘sovereign city’.

It would be tedious to discuss each of these comparisons once more in detail here, and so I shall confine myself to one case—that of the Ottomans. A curious feature of Blake’s comparative exercise is the quite limited literature he cites on Istanbul and the Ottoman empire. The description of Istanbul rests largely on the works of Bernard Lewis and the entry for Istanbul by Halil Inalcik in the Encyclopaedia of Islam; a particular omission is Robert Mantran’s general study of ‘daily life’ in the city in the sixteenth century. Mantran’s careful attempt, even in this semi-popular work, to balance Istanbul as an administrative city and seat of the Empire, against its other features—as a mercantile, religious and manufacturing centre—could well have served as a model for how to write a history of Shahjahanabad that talks of issues other than the court and the amīrs. Admittedly the sources for Ottoman urban history are far richer than those available for the Mughals, but the issue here is of conception as much as the deployment of evidence. For, Blake’s view of both the Ottoman state and Istanbul is reductionist in character, again neglects chronology and development for structure, and hence fails for example to locate in proper perspective the relationship between Ottoman expansion into Iraq, the Red Sea littoral, north Africa and eastern Europe, and the character of the state itself and of its capital.

A major problem with the comparison is that the relationship between Istanbul and other urban centres in the Ottoman domains remains in the shadow. Unlike Shahjahanabad, Istanbul was far and away the largest urban centre in the region, with other centres in Anatolia being quite small in comparison. This was not so with Shahjahanabad, for Agra—even after the court had shifted from it—continued to be comparable to it in order of magnitude. Further, Istanbul—as the former capital of the Byzantine empire—enjoyed a far greater and more continuous historically accumulated prestige than Delhi, again on account of the ‘rivalry’ with Agra.


Finally, and crucially (as Mantran reminds us), Istanbul was a maritime city, and had 'a particularly remarkable port, on which it depended for the greater part of the economic life of the capital'. Thus, Istanbul was far more than a sovereign-city (dār al-saltanat), only one of the many epithets applied to it. Indeed, this point can as well be made of Shahjahanabad as well.

Ottoman and, to a lesser extent, Safavid historiography undoubtedly has a great deal to offer the student of the Mughals. Urban history is certainly one of the fields in which Ottomanists are far in advance of their counterparts who study the Mughals. Besides Istanbul, excellent monographic studies exist of Cairo (by André Raymond and others), Jerusalem, Aleppo, Bursa, Izmir, Ankara, Kayseri (the last two by Suraiya Faroqhi) and a number of other towns. Yet, what all of these studies stress is the vivacity and complexity of urban life, which extends far beyond a mere discussion of the participation of the ruler and nobility therein. Even the study of building construction under state patronage has afforded to Ottomanists a view of the participation of social groups other than the elite in the process, just as the analysis of the Janissary revolts has ramifications extending far beyond the state. There is clearly a lesson to be learnt here.

If one of the major gains to be had from opening up comparisons between the Mughals and Ottomans is methodological, it is not merely restricted to urban history. Cornell Fleischer's study of Mustafa 'Ali (1541–1600), an Ottoman chronicler, administrator and ideologue, presents us with a model of sophisticated analysis, combining psychological insights, with social history and the history of thought on statecraft. It far surpasses any analysis that one can find on such figures in the Mughal case, where the biographies of even such fascinating figures as 'Abdur-Rahim Khan-i Khanan, the celebrated general, administrator, poet and patron of


58 Fleischer, Bureaucrat and Intellectual. Of course, even within the context of the Ottoman historiography, Fleischer’s work must be recognised as unusual, rather than representative.
the arts, are in the final analysis worthy but somewhat dull exercises.59 Much the same can be said of other figures such as Abu’l Fazl and his brother, Abu’l Faiz ‘Faizi’, on both of whom a wealth of material—including their letter-collections (insha)—exists. If indeed one wished to reopen the issues addressed by Streusand in his book, namely statecraft, ideological currents, and factional politics in Akbar’s reign, a thorough exploitation of these materials would be in order.

At the same time, an analysis of the ideology of the Mughal court and attempts to seek legitimacy cannot be achieved solely through recourse to Persian chronicles and correspondence. We may also seek to examine other images of the ruler produced and propagated by the court, be it in Sanskrit or in the vernacular literature of the epoch. Obviously, these may have been destined in part for a different audience than that which read the Akbar Nāma or the Tārīkh-i Alfi, but they are none the less significant for that. Among the important vernacular works, we may count the writings in Brajbhasha of Keshavdas (b. 1555) from Orcha, who in around 1612 wrote the Jahāngīr-yas‘-candrikā in honour of the Mughal ruler, in which—among other things—he praises Jahangir as ‘master of both faiths’ (duhūn dīn kau sāhib). It is significant in this particular case, that while Jahangir receives fulsome praise, the personage of Akbar is given short shrift, reflecting the political pressures of the moment on the poet (whose main patron, Bir Singh Deo of Orcha, had been responsible for the assassination of Abu’l Fazl)60! Thus, what is evidently in order is a far closer coordination between the fields of literary, social and political history, of the sort that Fleischer is able to accomplish in the Ottoman case.

**Summing up**

To conclude then, it may be premature to state that fresh winds have begun to blow from the west through the Mughal historiography. Indeed, the


principal conclusion of the present essay has been that the works that have been considered here often echo, even in exaggerated ways, the orthodoxies of the past three decades and more, on the history of the Mughal state. The major problem, it would seem, is the excessive preoccupation with identifying an essential structure; this runs parallel to existing tendencies to speak constantly of the ‘systems’ in operation under the Mughals, such as the ‘agrarian system’, the ‘imperial monetary system’, the ‘mansab system’, the ‘jagir system’, and so on. This preoccupation has in turn led to a seeming conflict that has been fought out in the arena of models, where the ‘patrimonial–bureaucratic’ has jostled with the ‘segmentary’, and the ‘uniquely’ (perhaps semi-feudal?) Mughal model. I have attempted to argue that the acceptance or rejection of these (usually pre-fabricated) models does little to enhance our understanding of the Mughal state and its history. This is, I believe, not a reason for despair, but rather one for hope. It opens the way to an alternative approach to the Mughal period, one that is more even-handed in its treatment of social groups, source-materials, regions, and epochs.

Several possible avenues thus appear open for researchers on the sixteenth and seventeenth century history of Mughal India who wish to leave the beaten track, as it has been defined in the past three decades. One possibility is to investigate in a systematic fashion the relationship between the institutions of the later Afghan states of northern India (in particular the Lodis), and what came to be the usage under the Mughals. A second avenue would be to look at the Central Asian roots of the Mughals, as well as their relationship with other Timurid rulers, with the Uzbek Khanate, and with the west Asian states in the sixteenth century. To do so would require a serious consideration of the Uzbek chronicles (not only the ‘Abdullāh Nāma, but Mushfīq Bukhārī’s Jāhān Nāma or Tārikh-i Aḥmad Khān), and the diplomatic correspondence of the period.

These are themes that have so far been little explored. To enter into them would imply, on the one hand, a deeper examination of the Indian context, and on the other hand, a wider approach to the larger Asian context within which the Mughal state was formed. Further, once into the

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61 Cf. an earlier critique in Frank Perlin, ‘Concepts of Order and Comparison, with a diversion on counter-ideologies and corporate institutions in late pre-colonial India’, in T.J. Byres and H. Mukhia, eds., Feudalism and non-European societies. London, 1985, pp. 87–165; also his article ‘Money-Use in late pre-colonial India and the international trade in currency media’, in J.F. Richards, ed., The Imperial Monetary System of Mughal India. Delhi, 1987, pp. 232–373, which implicitly provides a thorough-going critique of the very idea of an ‘imperial monetary system’, as set out by Richards! I should stress once more that the replacement of these ‘system’-oriented approaches with other essentialist constructs like fitna (see note 2 supra) is to trade King Log for King Stork.

62 For some glimpses of the possibilities in this direction, see Farooqi, Mughal-Ottoman Relations, cited in note 31 supra, also Riazul Islam, ed., Documents on Indo-Persian relations, passim.
late sixteenth century, the question of ‘region and empire’, set out by Chetan Singh in his recent monograph on Punjab in the seventeenth century, merits exploration. To what extent did Mughal conquest fundamentally affect the institutions and political culture of different regions in the sub-continent? Did the notion of the region itself change as a result of the process of incorporation into the empire? These questions can, however, only be addressed if we see the Mughal empire not as a finished product in 1600 (or at the death of Akbar), but as a state that was still evolving, and struggling to come to grips with a variety of local and regional institutional regimes. At the same time, to approach these issues means far more than producing parochial local histories, or paraphrasing the apocryphal chronicles of particular zamīndār families. It means writing histories that share neither the structural presuppositions of Bernier, nor a simplistic vision of the Mughal Juggernaut, the medieval road-roller that reduced the sub-continent into an institutional flatland.