Modern Asian Studies **49,** 5 (2015) pp. 1667–1673. © Cambridge University Press 2015
doi:10.1017/S0026749X14000614 First published online 4 June 2015

A Rejoinder to Tirthankar Roy

SHAMI GHOSH

Pontifical Institute of Mediaeval Studies, Toronto, Canada
Email: shami.ghosh@utoronto.ca

I am grateful to Tirthankar Roy for his prompt response, and his generosity in acknowledging the validity of some of my criticisms. It will be obvious to readers that there are some fundamental disagreements between us regarding what constitutes economic history. On this, and a number of other issues that Professor Roy has chosen not to address, it would be tedious to repeat myself; we must agree to disagree and invite readers of this journal to draw for themselves the conclusions they wish—and, ultimately, how fair I have been in assessing his book is something that readers will only be able to judge by reading it themselves. I focus below on the three core issues with which most of his response is concerned: 'reading the past with reference to the present', the importance of a 'region-focused approach', and the 'comparative approach'.

There is nothing wrong whatsoever with studying the past in order to understand how the present came into being; Professor Roy and I agree on this point. I agree also that it would not be acceptable to adopt a model for understanding the eighteenth century from which it is not possible to see the developments of the nineteenth century emerging. Both these points, however, do pose some problems, principally the dangers of anachronistic understandings of the past, and of a teleological approach to history.

By all means, let us ask seek to understand from the past how the present came into being. But in doing so, we have to be cautious of imposing later categories on an earlier period, because not all manifestations of economic organization and behaviour of the nineteenth century (or later) are appropriate for understanding the eighteenth century (or earlier). Furthermore, one should be wary of having too much faith in the existence of phenomena that look very much like antecedents of the present. In some cases, it is absolutely clear that there were no such roots: the concept of 'India' really does

https://doi.org/10.1017/S0026749X14000614 Published online by Cambridge University Press

not apply in any clear manner to the eighteenth century, so it would be difficult to justify its use as an analytical category. Lacking British intervention and the trauma of partition, there is actually nothing in the history of the eighteenth century that is 'consistent' with the modern geopolitical entity that is 'India'. This being so, of what value, for our understanding of the eighteenth century, are analyses of 'India', of percentages of the total population involved in a particular sector, of the relative importance of textiles within the gross domestic product of 'India', and so on?

It is incumbent upon us, as historians, to accept both that contingency plays a role in historical developments, and that paths can be interrupted, broken, or changed. For this reason, if we seek a path from the past to the present, it is better to begin at the beginning, and try and move forwards, rather than begin at the end and work our way backwards. We should seek to understand the eighteenth century in the first instance using categories that make sense for that period, *without* reference to later periods, and also, I suggest, without worrying too much about whether those categories that make sense for the eighteenth century are 'consistent' with the nineteenth. We need first to understand the internal dynamic of the period: only then can we try and grasp how, from what we can comprehend of the eighteenth century, what we know of the nineteenth might have emerged. If the path we can discern in the eighteenth century does not appear to lead to the nineteenth, we must be alert to the possibility of a change of course, and try and understand why or how what might appear to be a predetermined course was abandoned. We must not assume that any future history was already predetermined. Thus it is best to abandon the later period as a starting point and seek to move backwards in a straight line to some sort of origins; otherwise, we risk imposing anachronistic ways of understanding societies in past time.

Since 'India' did not exist in the eighteenth century, and there is no path we can trace backwards into the eighteenth century to find the roots of 'India' there, we must begin with regional entities that *did* exist, without reference to 'India'. There is a logic to studying a particular region within a particular period if one can see that that region in that period had its own internal dynamic and structure that marked it out as a coherent region. Where one draws the boundaries is naturally going to be a difficult issue and, once again, I certainly do not suggest that we should study past regions demarcated according to later boundaries: perhaps Dhaka and Calcutta should *not* form part of

https://doi.org/10.1017/S0026749X14000614 Published online by Cambridge University Press

the same region; perhaps it should be made even more explicit when referring to 'Gujarat' that something quite different from the modern Indian state is meant.

I thank Professor Roy for drawing attention to these and other fundamental issues regarding how one can define a region for the purposes of pre-modern economic history; I agree that the exercise comes with its problems, many of them difficult ones. What sort of serious historical study does not? If we wish to answer the big 'economic history questions'—whether these are the ones Professor Roy is interested in, or those that I have posed—surely we cannot be shy of complexity. The problems Professor Roy raises with regard to a regional focus are issues that other scholars working on other regions have faced, and have attempted to address. Naturally their efforts have met with greater or lesser success, but much can be learned even from studies that do not fully deliver on their objectives. South Asia was not the only pre-modern macro-region comprising many regions and micro-regions with diverse geographies and varying forms of relations among them. In Europe, for example, there is no linear path between the various pre-modern German-speaking regions and modern Germany with regard to any aspect of the history of these regions, nor do the pre-modern political boundaries always coincide very coherently with units that might, from our perspective, make economic and geographical sense. The regions of 'Germany' thus provide good comparanda, regarding which there have been a wealth of studies that have indeed tried to address precisely those problems of regional history that Professor Roy raises. (Germany is just the example with which I am most familiar; *mutatis mutandis*, one could certainly say the same of at least Italy and Spain.)

Of the many works of regional or micro-regional history that have broader theoretical consequences, let me refer again to Bas van Bavel's *Manors and Markets*, Govind Sreenivasan's *Peasants of Ottobeuren*, and Chris Wickham's *Framing the Early Middle Ages*, to which let me add also Tom Scott's *Regional Identity and Economic Change*, an excellent work on the region now split between southwestern Germany, northwestern Switzerland, and northeastern France.[1] The first of these is a study of a thousand years of the history of the Low Countries, a 'region' that

[1] Bas van Bavel, *Manors and Markets: Economy and Society in the Low Countries 500–1600* (Oxford: Oxford University Press, 2010); Govind P. Sreenivasan, *The Peasants of Ottobeuren, 1487–1726* (Cambridge: Cambridge University Press, 2004); and Chris Wickham, *Framing the Early Middle Ages: Europe and the Mediterranean, 400–800* (Oxford:

https://doi.org/10.1017/S0026749X14000614 Published online by Cambridge University Press

is marked by wide geographical and institutional variation—and the author is very sensitive to the problem of studying this area as a single 'region', but nevertheless can find (and, to some extent, explain) a number of similar developments throughout the Low Countries. The second is a micro-history of a single district that, despite its apparently very narrow focus, does indeed address 'economic history questions'. The third is a study that compares, over a four-century time span, a number of different large regions, and is quite sensitive to differences within those regions. The fourth is a study of an area the author defines as a economic region that was coherent precisely because of the geographical diversity that allowed for—indeed, perhaps brought into being—a certain level of interdependence and integration, which was then interrupted not because of any inherent economic logic, but rather because of political boundaries.

One could easily find more examples of stimulating studies of this sort on European regions; there have also been some very sophisticated regional studies of South Asia: for example, the work of Sanjay Subrahmanyam on the Coromandel coast, or Samira Sheikh on Gujarat.[2] The fact that neither Sheikh, nor Nadri in his work on eighteenth-century Gujarat,[3] cover the whole of the region as understood in modern terms is no real criticism: they are both explicit about what region it is that they study, and why it makes sense to do so. I do not mean to suggest that any of these works necessarily address all of the problems involved in doing regional history, nor that they are completely unproblematic. But surely we should be open to making a start somewhere and building on the work of others to make our regional analyses more sophisticated, rather than rejecting the very possibility of the sort of regional history I advocate because it is 'difficult'.[4]

Oxford University Press, 2006). See also Tom Scott, *Regional Identity and Economic Change: The Upper Rhine 1450–1600* (Oxford: Oxford University Press, 1997).

[2] Sanjay Subrahmanyam, *The Political Economy of Commerce: Southern India, 1500–1650* (Cambridge: Cambridge University Press, 1990); S. Subrahmanyam, 'Rural Industry and Commercial Agriculture in Late Seventeenth-Century South-Eastern India', *Past and Present*, 126 (February 1990), pp. 76–114; Samira Sheikh, *Forging a Region: Sultans, Traders, and Pilgrims in Gujarat, 1200–1500* (New Delhi: Oxford University Press, 2010).

[3] Ghulam C. Nadri, *Eighteenth-Century Gujarat: The Dynamics of Its Political Economy* (Leiden: Brill, 2009).

[4] Perhaps I am overly enthusiastic about these and other works of South Asian history that adopt regional and local perspectives, but it seems to me that Professor Roy is excessively dismissive in his view of such works, many of which have a more sophisticated approach to region-focused history than might be apparent from his

https://doi.org/10.1017/S0026749X14000614 Published online by Cambridge University Press

There is a further problem with regional history that Professor Roy brings up: that it is simply not possible because of a lack of (quantitative) data. I am not the only person to have suggested that more work needs to be done with the deposits in India's regional archives, but I am happy to concede that perhaps the material therein will not allow one to answer the questions I have posed. Have sufficient efforts been made for certainty on this point? Is it really the case that the kinds of questions I have raised have, for the most part, not been addressed by the scholarship because it is impossible to do so? Or is it rather the case that these questions have not really been asked? Professor Roy grants that his method is 'not the best way of doing the economic history of the eighteenth century', but his question, 'Is there a better way that works in practice?' suggests a negative answer. Perhaps one should attempt the practice more diligently before giving up.

Of course, even immersion in regional archives is unlikely to provide the sort of quantitative data Professor Roy would like to see: this point I concede entirely. But is it all really quite as hopeless as Professor Roy thinks? He criticizes my use of adjectives like 'dense' (networks) or 'high' (levels of commercialization): these mean nothing unless backed up by numbers. It seems to me that the numbers are in fact not entirely lacking—and this is presumably how Professor Roy himself can in his book speak of a 'vast grain trading network'. Many local studies (for example, those of Kumkum Chatterjee, Rajat Datta, and Dilbagh Singh)[5] do provide figures for how much trade went through local markets, the volume of goods traded, the percentage of people who would have had to be market-dependent, the percentage of output that reached urban markets, and so on; these are figures that suggest

comments; indeed, his view that 'by and large the archival research done on the eighteenth century does not ask economic history questions' seems a bit harsh.

[5] Kumkum Chatterjee, *Merchants, Politics and Society in Early Modern India: Bihar: 1733–1820* (Leiden: Brill, 1996); Rajat Datta, 'Merchants and Peasants: A Study of the Structure of Local Trade in Grain in Late Eighteenth-Century Bengal', *Indian Economic and Social History Review*, 23:4 (1986), pp. 379–402; R. Datta,' Subsistence Crises, Markets and Merchants in Late Eighteenth-Century Bengal', *Studies in History*, 10:1 (1994), pp. 81–104; R. Datta, 'Peasant Production and Agrarian Commercialism in a Rice-Growing Economy: Some Notes on a Comparative Perspective and the Case of Bengal in the Eighteenth Century', in Peter Robb (ed.), *Meanings of Agriculture: Essays in South Asian History and Economics* (Delhi: Oxford University Press, 1996), pp. 139–62; R. Datta, *Society, Economy and the Market: Commercialization in Rural Bengal, c.1760–1800* (Delhi: Manohar, 2000); and Dilbagh Singh, *The State, Landlords and Peasants: Rajasthan in the 18th Century* (New Delhi: Manohar, 1990).

https://doi.org/10.1017/S0026749X14000614 Published online by Cambridge University Press

that levels of commercialization were indeed 'high'. More rigour is, of course, required, but it is not impossible. We can, after all, arrive at some estimates regarding the sizes of urban populations, how much grain or cloth they would have needed, how much cloth was indeed produced, and so on. We have some knowledge of how far some of the grain and raw cotton travelled. The nature of surplus extraction, the numbers of local markets, the levels of rural market dependence are also not entirely opaque issues. All of this can be used to estimate levels of commercialization, the extent of production for the market, the scale of industry, and so on.

One could multiply such examples of suggestive information that we do in fact possess. Of course, we will never have the sort of detailed figures that are available for the nineteenth or twentieth centuries and, of course, much more of this sort of work needs to be done than has been the case to build up a more robust justification for claims regarding commercialization. But this work can actually be done, and it does allow us to come to some conclusions that are not entirely unreasonable. I suggest, therefore, that we should not yet despair and turn immediately to anachronistic means of explaining the past that, as I suggested in my article, ignore the indicators for precisely such 'dense' networks and 'high' levels of commercialization.

Finally, there is the issue of comparative history. Professor Roy generously concedes that it would be 'fascinating' to follow my call for studies of industriousness, proletarianization, local patterns of consumption, and so on, and to do so adopting a comparative perspective, but he believes that this will not be possible. Perhaps not; though I retain my doubts regarding the extent to which such study has been attempted. It is almost certainly the case that we will remain unable to understand every region equally well, but that is no reason not to try and understand at least some regions better than we do now, and be open to the perspectives that might arise from comparison. Of course, comparing 'core regions' in India with those elsewhere does indeed mean that we need to define each of the regions sensibly: as I say, while posing difficulties, this need not be an insurmountable problem.

But Professor Roy is hesitant about comparative history for another reason: 'which conceptual tool is portable cannot be settled either easily or quickly'. I agree, but this is surely no reason for abandoning the very possibility of comparative work. Or should economic history be a discipline only engaged in easy and quick projects? In any case, the objection applies not just to conceptual tools imported from another

https://doi.org/10.1017/S0026749X14000614 Published online by Cambridge University Press

region, but also from another period: Professor Roy has been very quick in applying paradigms derived from later periods to the eighteenth century, and perhaps in doing so he has been less attentive than he might have been to the difficulties involved in this process.

Professor Roy feels further that my suggestion that something might be gained from a comparison with the results of Rosenthal and Wong's recent work is over-hasty: that work is also problematic.[6] I agree; I have criticized it at some length elsewhere.[7] Nevertheless, this book does raise questions regarding the importance of state provision of public goods and the role of military expenditure in determining economic outcomes, and these are questions worth considering. Even more to the point is the fact that Rosenthal and Wong's book is not the only study of a non-Indian region I cited, and many of the other works I referred to have indeed 'survived' reviews by the relevant experts. How 'easily' and 'quickly' the stimulus from, for example, the works of the scholars referred to once again above (to which we might add Victor Lieberman's *Strange Parallels* project)[8] might be made useful for the study of India is a question that can only be answered if one at least makes the attempt to do so.

I agree entirely that doing regional and comparative history poses a number of problems, that it is not easy, and that it takes time. I cannot agree that these are reasons to dismiss such approaches or, indeed, the very possibility of stimulating the study of India by learning from some of the questions that have been asked of other parts of the world. Neither we should instead seek to understand the eighteenth century by imposing on it concepts that are anachronistic. It is all too easy to apply modern paradigms (regarding region, state, urban–rural relationships, causes for economic growth) to the past without giving attention to how inappropriate they might be. While doing so might produce quick results, the validity of such results for an understanding of the period in question remains doubtful to me.

[6] Jean-Laurent Rosenthal and R. Bin Wong, *Before and Beyond Divergence: The Politics of Economic Change in China and Europe* (Cambridge, Massachusetts: Harvard University Press, 2011).

[7] Shami Ghosh, 'The "Great Divergence", Politics, and Capitalism', *Journal of Early Modern History*, 19:1 (2015), pp. 1–43.

[8] Victor Lieberman, *Strange Parallels: Southeast Asia in a Global Context, c.800–1830, Vol. I: Integration on the Mainland* (Cambridge: Cambridge University Press, 2003); and *Strange Parallels: Southeast Asia in a Global Context, c.800–1830, Vol. II: Mainland Mirrors: Europe, Japan, China, South Asia, and the Islands* (Cambridge: Cambridge University Press, 2009).

https://doi.org/10.1017/S0026749X14000614 Published online by Cambridge University Press