We need to find what we are not looking for

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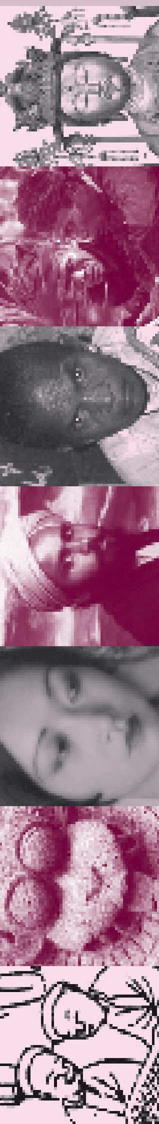
Cutting near Voi Station. 1890. Courtesy: Railway Archives. Nairobi (see p.18-19)

43 Comparative Intellectual Histories of Early Modern Asia

NEWSLETTER



Have a taste of an IIAS Masterclass with Sheldon Pollock's theme on Intellectual Histories. *pp. 1-13*

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Don't go don't go, stay back my friend... but they did go. Punjabi diaspora in East Africa. Photo Essay *pp. 18-19*

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We need to find what we are not looking for

The Master Class on "Comparative Intellectual Histories of the Early Modern World" was held at the International Institute for Asian Studies in Leiden in May-June, 2006. The idea of a master class – assembling a team of scholars to discuss recent advances in a field with doctoral and postdoctoral students – is the brain child of IIAS's former director, Wim Stokhof, and I express my thanks to him for his vision and energy in making this intellectual experiment possible.

I say experiment because none of the participants, the instructors included, had ever engaged in this kind of comparative intellectual-historical conversation. As Michael Cook confessed, although he works with Benjamin Elman in the very same building at Princeton University, the two had never previously exchanged ideas on problems shared across their regions. It was just this sort of non-communication – fallout from the division of the world of knowledge into studies of areas – that the class was designed in part to address.

Sheldon Pollock

To be sure, knowledge always begins in specific places, and one of our aims was to share new knowledge about ideas and intellectual practices in the places we study. But more crucial and challenging was it to address the three critical problems embedded in the title to the class, problems that are either only now coming under study, or are understudied, or even unstudied:

- What sense does it make to speak of *early modernity* in the sphere of mental life outside the early modern West—that is, in Asia in the several centuries preceding European expansion? What problems do we face in defining such modernity? Is 'early modernity' a useful concept in writing the history of Asian thought?
- What are the special tasks, methods, or theoretical commitments that constitute *intellectual* history as a separate and valid form of knowledge? Does the intellectual history of early-modern Asia have tasks, methods or theoretical commitments that differentiate it from the study of intellectual history as developed from European materials? Is there an unacknowledged link between the events of European intellectual history and what are seen as 'general' methods of intellectual history?
- What are the aims and methods of a *comparative* intellectual history of the early modern world? How do we do it, and what precisely are we trying to discover when we do do it?

I can't address all these questions – the assembled essays here collectively do so in their different ways – but will offer only a summary of my introductory remarks. I can be relatively brief about 'early modernity' and 'intellectual history' since our specific challenge was coming to terms with the problem of comparativism.

The uses and abuses of 'early modernity'

Early modernity has been a much disputed topic of conversation among scholars of Asia for the past decade, both regionalists and generalists. Many object to the apparent teleology of the idea, committing us as it is supposed to do to some inevitable developmental goal (so Randolph Starn). Of course,

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our inquiry is perforce 'teleological' in the sense of aiming to understand what occurred in the past that enabled us to get us to the telos – if that is still the right word here – we have gotten to. There is no way to forget the end of this story just because we concentrate on the beginning – indeed, we wouldn't even know where to begin the story if we didn't know how it has ended because we wouldn't know what the story was. Others object that many so-called early modernities never became full modernities except when mediated through western modernisation. But what if western modernisation short-circuited other processes of transformation? No given present was *bound* to come out of any given past; but our present has come out, and we want to know how and why it has.

Few deny that over the three centuries up to 1800 Eurasia as a whole witnessed unprecedented developments: the opening of sea passages that were global for the first time in history, and of networks of trade and commodity-production for newly global markets; spectacular demographic growth (the world's population doubled); the rise of large stable states; the diffusion of new technologies (gunpowder, printing) and crops from the Americas. If this is a list (borrowed from John Richards) of what is supposed to make life 'modern' rather than just new or different, what part of the world failed to experience early modernity? On the other hand, if we descend from that broad definition of the early modern to the narrow – the presence of fossil-fuel technology, constitutional governance, and religious freedom and secularisation (Jack Goldstone's view) – there will be no case of early modernity aside from Britain. We may instead argue that modernity is additionally, or exclusively, a condition of consciousness. But what kind of consciousness? If we stipulate this a priori in light of European experience – a new sense of the individual, a new scepticism, a new historical sensibility, to name three Master Categories – and go forth to find them, we are likely to succeed, since you usually find what you are looking for. Or conversely, if we set out to find them – an Indian Montaigne, a Chinese Descartes, an Arab Vico – and somehow do not, well, too bad then, there will be no pre-European Asian modernity at all.

A good deal of current discussions of early modernity is irrelevant, I suggest, for the purposes of our master class, or even an obstruction; as Frederick Cooper argues, the notion of modernity has had an important historical role in making claims, but is virtually useless as an analytic concept. We are therefore perfectly justified in seeking to understand how various the world was *at the moment before* what would become the dominant form of modernity – colonial, capitalist, western – achieved global ascendancy (even if that question can only be posed in the moment after). We can call it 'early modern' simply in the sense of a threshold, where potentially different futures may have been arrested (or retained only as *masala* for that dominant form). But we can go further. Since the material world changed dramatically during the few centuries before this threshold, and changed universally, there is good reason to ask how the systems devised for knowing the world responded – or why they failed to respond – to the world that was changing objectively between these dates. At the same time there is good reason to resist the teleology – here indeed an infelicity – in the term '*early* modern' and so refuse to assign this period any shared structure or content a priori, let alone to insist on finding in it western modernity in embryonic form (the Chinese Descartes). The trap of definitional consistency is precisely what we need to avoid, as my remarks on comparison will make clear.

In short, the period constitutes an entirely reasonable framework for a comparative intellectual history, without leading us to posit any necessary uniformity in the history of intellection that transpired. Everyone began to participate in a new world economy, to live in new, larger, and more stable states, to confront a demographic explosion, a diffusion of new technology, vaster movements of people in a newly unified world. How did people experience these transformations in the realm of thought? That is what we need to discover. I think there may be remarkable parallels awaiting discovery, aside from the shocking parallel that the period – empty vestibule, it has been thought, between high tradition and westernization – is all but unstudied everywhere. But we should not worry if they are not found. A 'negative' outcome, say, of stability in the face of dynamic change elsewhere – producing a global version of what Ernst Bloch saw as modernity's constitutive 'Gleichzeitigkeit des Ungleichzeitigen' (a multiplex simultaneity of things that are non-simultaneous) – would be as important as a 'positive' one, since we are interested in knowing why people may wish to preserve forms of knowledge in the face of changing objects of knowledge no less than in knowing why they may be prepared to change them.

The life and death of intellectual history

Probably no subfield of the discipline of history has experienced a more precipitous decline in the past generation than intellectual history. In Chinese studies, the retreat from intellectual into social history seems widely symptomatic of a broader trend. (In Indian studies intellectual history never really existed as a theorised scholarly practice, so there was nothing to retreat from.) *Comparative* intellectual history has fared even worse – in fact, it is hard to claim the practice even exists in any acceptable, *historical* form (comparisons of Shankara and Heidegger, for example, in this sense fall entirely outside intellectual history). Notwithstanding the relative indifference toward it, intellectual history and what I will suggest is its necessary complement, comparative intellectual history, constitute an important new horizon on the terrain of early modern Eurasian history and, I would even claim, the foundation for any future study of modernity or colonial transformations. We cannot possibly understand what changes colonialism and capitalist modernity wrought in Asia – in the social, political, scientific, aesthetic or other sphere – without understanding what was there, in the domain of concepts, to be changed.
One measure of the relative health of a field is the state of its self-reflection. In intellectual history so little work of this sort is being done these days that the case might seem beyond hope. Certainly scholars continue to write about Bacon, Hobbes, Locke and Rousseau. But no serious new conceptualisation of what it means to do intellectual history has appeared (so far as I know) in several decades, only modest expressions of concern (by Donald Kelley, for example, or Anthony Padgen), or restatements of older positions.
One such restatement has recently been published by the master of the history of early modern British political thought, Quentin Skinner. Intellectual history remains, according to Skinner, a poorly articulated field of research. In part this is because of its dispersal across the disciplinary landscape. In terms of objects, Skinner reasserts the centrality of the great texts – it is mere philistinism to demand that Delft tiles be studied equally with (let alone instead of) Vermeer's paintings. In terms of method, he reasserts contextualism against reception history – the meaning that counts in the author's intentional meaning – and the history of ideas against social history – the impact of a text or the breadth of its dissemination is entirely irrelevant to its intellectual history; in other words, the measure of the importance of an idea is independent of the power it historically exerted. Lastly, the understanding of ideas is not about capturing some transhistorical essence of meaning; it must be resolutely historicist and centered on their discursive deployment in their original context. This alone enables us to see the web of what Skinner describes as the contingencies that produced the understandings with which we ourselves now operate, and to enable us to frame new ones.
Now of course these are the ideas that Quentin Skinner has defended so brilliantly for the past 40 years. And they are good ideas, to be sure – who doesn't accept contextualism these days, if they are at all serious about historical knowledge? But they are old ideas that have become static, old answers to old questions. Furthermore, they are not the only ones on offer, and the dismissal of these others may have something to do with the gradual erosion of intellectual history itself for the perception is widespread that intellectual history is arrogantly elitist, brutally historicist, narrowly textualist, unreflexively great-man-ist (and great-*man*-ist), and of course, preternaturally idealist. Some of this critique is clearly unfair – intellectual history is by definition concerned with texts and ideas, after all – but others hit closer to home. Gadamer, for example, makes the reception-history of a text an essential – if not *the* essential – dimension of meaning, and integral to this process is what he calls the 'application' of the text, its truth for us. Foucault almost completely erases agents and their intentions from intellectual history, to say nothing of demonstrating the value of supplementing the great texts with the most pedestrian kinds of data. And measuring the importance of an idea independently of its historical power (though in fact we only read Hobbes because he in fact exerted historical power) is, as Pagden has observed, precisely the position that would be contested by social history, which has sought to substitute the history of mentalities (the real thought worlds of ordinary people) for the textualised thoughts of the elite.

If Padgen is right in saying that intellectual history is at a kind of crossroads and needs to establish a secure identity in order to advance, it will be useful for us to remain as conceptually alert as possible about what we are doing in our master class. Overcoming western national traditions of scholarship by globalizing the conversation and overcoming Europe by including the non-West (so Wilhelm Schmidt-Biggemann) are obvious moves, though whether there are specific methods for achieving such overcoming is open to serious discussion (one that has so far yet to be opened). In addition – though this is hardly a very revolutionary suggestion – intellectual history has to mean exploring (textualised) thought in relation to historical change not just in relation to change in other thought, which seems to me only part of the story, but in relation to change in the society and polity within which that thought occurs. Intellectual history is concerned with more than speech acts and authorial intentions; it is also concerned with social practices. And its context is more than linguistic and intellectual; it is also institutional and political.

This linkage brings intellectual history closer to Reinhart Koselleck's *Begriffsgeschichte*. This proximity is a good thing. For starters, intellectual history can derive support from Koselleck's arguments for the central place of conceptualisation in social practices: 'Nothing can occur historically that is not apprehended conceptually.' In other words, you cannot do history and not do intellectual history. But more than this, there is a mutually constitutive relationship between the history of thought and the history of social action; social history itself is in part the story of the redefinition of the concepts that make present social life intelligible. The way forward in both intellectual and social history may lie in recognising that we must not choose – and indeed, typically do not anyway choose – between them. This seems to be a lesson the best intellectual historians today have learned without perhaps having the faintest idea what the term *Begriffsgeschichte* means.

To compare or not is also not a choice

Like intellectual history but perhaps even more so, comparison in the human sciences experienced a stunning decline in popularity in the past generation. The reasons for this, too, are not far to seek. They are related to a general antipathy toward master narratives, hard laws, reified categories, which are statically unhistorical, falsely evolutionist, and regressively universalist. Such resistance to comparison, however, is based on an overly narrow view of what comparison is for – and perhaps even on an illusion, namely, about whether we even have a choice whether or not to compare. In fact, I am becoming persuaded not only that we cannot not do intellectual history, but when we do do it, it must be comparative.
That comparison is a cognitive necessity is becoming increasingly obvious to scholars, though a full-scale exposition remains a real desideratum. It is not only intrinsic to social analysis but to lived social experience (so Rogers Brubaker). Inequality, for example, is a social category that rests entirely on comparative grounds. True, one danger in comparative work is the naturalisation of the unit of analysis (eg, the nation-state), but this is neither inherent in nor specific to comparison. However, another danger that is specific to comparison is the often unreflective generalisation based on a single case. In the very act of generalising that case as the unit of analysis, you are already suppressing, or potentially suppressing, elements of difference. But this may be nothing more than a variant of the hermeneutic circle, and not a necessarily vicious one, which we can correct as we tack between the first and second order case.

If comparison is everywhere, we need to make our inevitable but implicit comparisons explicit and to try to explain what role they are playing in the interpretation of our primary object. In the case of 'early modern knowledge', the comparative instances we typically foreground, or at least those that Ben Elman and I do (see 'Further Reading'), are western European, for two, unequal reasons: first, those instances are in the heads of those two particular observers, and inexpugnably so, when approaching China and India; they are the embedded comparative other. Second, through the force of colonialism and modernisation, western knowledge in many domains has been victorious, and we want to try to figure out what secured this victory.
Yet that is not the only comparative move we want to make. Comparison of non-western forms of knowledge in the early modern world has additional goals, to which we can proceed only by way of intentionally bracketing the western model. The cases that constitute the objects of our *intellectual* history are forms of systematic thought that are found everywhere literate culture itself is found. Our comparative intellectual *history* posits the importance of synchronicity among these cases but makes no a priori claims that synchronicity entails symmetry; in fact, asymmetries are as important and revealing as anything. How comparable forms of thought change in time, change differently, or do not change at all, and why they do or do not change, is what this kind of historical inquiry seeks to understand. Not only is chronology central to our comparative practice, but no models should be held to be universal, as instances of *necessary regularities*. On the contrary, what we want is comparison without hegemony.

It is vitally important that the synchronicity grounding comparative intellectual history contain no necessary content of this or any other sort. We make no assumption of unidirectional change and do not look for it; we make no assumption of a world system of intellectual modernity in which everyone participated, as some believe was the case with the world system of capitalism. Indeed, economic and intellectual history are not necessarily isomorphic. We might set out to write a history of early modern capitalism but it would be wrong-headed to set out to write a global history of 'early modern thought' as if we knew in advance what that singular entity was, and as if the descriptor 'early modern' was not just a temporal marker, but also a conceptual marker. This is precisely the defi-

nitional trap that we saw lies in wait. Avoiding it and its hegemony means avoiding the one model of modernity that chanced to succeed; it means redefining modernity so that it is not about fossil fuels, parliamentary government, and secularisation, but a completely open category waiting to be filled with local content generated by empirical work.
When we compare the intellectual histories of the early modern world, what is it precisely that we want to know or do? Validate a hypothesis over N cases? Develop causal accounts of big structures and processes? Differentiate cases? The first is the goal of comparative history; the second, the goal of comparative sociology. For us the most effective comparative intellectual histories are going to be of the last type, which (as Peter Baldwin explains) ignores generalisation and seeks to capture similarities and differences across a limited number of instances in order to understand the cases under discussion, to isolate from the incidental what is 'crucial' and possibly, though less likely, what is 'causal'.
The world that intellectuals across the globe inhabited and sought to know changed indubitably and radically in the period standardly called early modern. The master class participants want to know how those intellectuals responded, how their responses might compare with each other in different places, how similarly or dissimilarly their responses transformed the great intellectual traditions to which they were heir. The question to ask is not 'How modern is it?' – that's the hegemonic comparison we need to consciously bring to the table and examine critically. The question to ask instead is whether intellectual modernity may have had different characteristics and histories in different parts of the world, including the history of *kaozheng xue*, 'evidential scholarship', in China, of *tajdid*, 'renewal', and *tahqiq*, 'verification', in the Middle East, and of 'newness', *navyata*, in India; and more, whether in those histories possibilities for a modernity different from the capitalist variety may once have been contained. <

Suggestions for Further Reading
- Peter Baldwin, 'Comparing and generalizing: why all history is comparative, yet no history is sociology', in *Comparison and history: Europe in cross-national perspective*, ed. Deborah Cohen and Maura O'Connor. New York: Routledge, 2004.
- Rogers Brubaker, 'Beyond Comparativism', *Theory and Research in Comparative Social Analysis*, Department of Sociology, UCLA Year 2003 Paper 1, pp. 1-8.
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- Benjamin Elman, *From Philosophy to Philology: Intellectual and Social Aspects of Change in Late Imperial China*. Los Angeles: UCLA Asian Pacific Monograph Series, 2001.
- André Gingrich, ed. *Anthropology, by Comparison*. London: Routledge, 2002.
- Jack Goldstone, 'The Problem of the "Early Modern" World'. *Journal of the Economic and Social History of the Orient* 41, 3 (1998): 249-83.
- Sudipta Kaviraj, 'Outline of a revisionist theory of modernity'. *European Journal of Sociology*, 46, 3 (2005): 497-526.
- Donald Kelley, 'Prolegomena to the Study of Intellectual History'. Intellectual News 1 (1996): 13-14.
Anthony Pagden, 'The Rise and Decline of Intellectual History'. *Intellectual News* 1 (1996): 14-15.
- Sheldon Pollock, *The Ends of Man at the End of Premodernity*. Amsterdam: Royal Netherlands Academy of Arts and Sciences, 2005.
- John Richards, 'Early Modern India and World History', *Journal of World History*, 8, 2 (1997): 197-209.
- Wilhelm Schmidt-Biggemann, 'Was ist "Intellectual History"?', *Intellectual News* 1 (1996): 15-17.
- Quentin Skinner, 'On Intellectual History and the History of Books', in *Contributions to the History of Concepts* 1,1 (2005): 29-36.
- Randolph Starn, 'The Early Modern Muddle', *Journal of Early Modern History* 6, 3 (2002): 296-307.

A number of ideas in the foregoing essay are developed in greater detail in 'Introduction', in *Forms of Knowledge in Early Modern South Asia*, ed. Sheldon Pollock (Durham: Duke U. Press, forthcoming) and in 'Comparison without Hegemony: The Logic and Politics of a Comparative Intellectual History of Early Modern India', in *History and Indian Studies*, ed. Claude Markovits et al. (forthcoming).

Early Modern Classicism and Late Imperial China

Most historians treat late imperial China, 1400-1900, as a time of fading and decay. Indeed, viewed backwards from the Opium War (1839-1842) and Taiping Rebellion (1850-1864), events before 1800 appear to have left China unprepared for modernity. But the 17th and 18th centuries can be considered not only as a 'late imperial' prelude to the end of traditional China, but as an 'early modern' harbinger of things to come.

Benjamin A. Elman

By 1650 leading Chinese literati had decisively broken with the orthodoxy entrenched in official life and tipped the balance in favour of a 'search for evidence' as the key to understanding China's past. Like Renaissance Latin philologists, Chinese philologists exposed inconsistencies in contemporary beliefs. They were also prototypes of the modern philologist as moral reformer – radical conservatives who attacked the present in the name of the past. As scholarly iconoclasts they hoped to locate a timeless order in and prior to the classical antiquity of Confucius (551-479 B.C.E.).

Until 1600, the ideal that motivated Chinese literati was sagehood. If every literatus was a virtuous exemplar, then society would prosper. Knowledge was equated to action, and political and cultural stability depended on each individual's moral rigour. To buttress such claims, Chinese had by 1200 developed an interactive account of the heavens, earth, and human concerns. Ideally each person was a pivotal factor in a morally just and perfectly rational universe.

By 1750, however, the heirs of this entrenched moral orthodoxy formed a relatively secular academic community, which encouraged (and rewarded with livelihoods) original and rigorous critical scholarship. In contrast to their predecessors, late imperial literati stressed exacting research, rigorous analysis, and the collection of impartial evidence drawn from ancient artefacts and historical documents. Personal achievement of sagehood was by now an unrealistic aim for the serious classicist.

This philological turn represented a new, early modern way to verify all knowledge. The creation and evolution of this new scholarly community led to fresh intellectual impulses that recast the place of the literati scholar from sagely Mandarin to learned researcher. The major figures called what they did 'evidential research' (*kaozheng* 考證, lit., 'the search for evidence'), and for the most part they resided in the wealthy and sophisticated provinces in the Yangzi River delta. There they received, rediscovered and transformed the classical tradition.

Their precise scholarship depended on a vibrant commercial and educational environment that rewarded cutting edge classical studies with honour and prestige. Academic work as collators, editors, researchers, or compilers depended on occupationally defined skills that required thorough mastery of the classical language and a professional expertise in textual research. Practitioners were bound together by common elements in education and shared goals, which included the training of their successors in scholarly academies.

Classicism and commercial expansion after 1550

Besides academies and patronage, evidential scholars also contributed to a growing network of bibliophiles, printers, and booksellers who served their expanding fields of research. Libraries and printing were pivotal to the emergence of evidential scholarship in the Yangzi delta. Scholars shared a common experience in acquiring philological means to achieve classical ends. This experience touched off differences of opinion and led to reassessments of inherited views. Supported by regional commerce and local trade, early modern communications grew out of the publishing industry in late imperial China. As China's population grew, the reach of the late imperial bureaucracy declined. Many literati wondered whether the classical orthodoxy still represented universal principles at a time when goods and art were financially converted into objects of wealth paid for with imported silver. Late imperial literati were living through a decisive shift away from their traditional ideals of sagehood, morality and frugality. Landed gentry and merchant elites transmuted the classical ideal of the impartial investigation of

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Setting movable type in the Qianlong Imperial Printing Office. Qinding Wuying dian juzhen ban chengshi (Beijing, 1776). Elman, Benjamin A. 2005. On their Own Terms: Science in China, 1550 -1900. p. 18