



Universiteit
Leiden
The Netherlands

Mathematics, Mao, and many reasons: an interview with Kapil Raj

Sinha, N.; Susanto, M.; Sysling, F.H.

Citation

Sinha, N., Susanto, M., & Sysling, F. H. (2024). Mathematics, Mao, and many reasons: an interview with Kapil Raj. *Itinerario*, 48(Special Issue 1), 1-9.
doi:10.1017/S0165115324000111

Version: Publisher's Version




License: [Licensed under Article 25fa Copyright Act/Law \(Amendment Taverne\)](#)

Downloaded from: <https://hdl.handle.net/1887/4180224>

Note: To cite this publication please use the final published version (if applicable).

INTERVIEW

Mathematics, Mao, and Many Reasons. An Interview with Kapil Raj

Neilabh Sinha , Melinda Susanto  and Fenneke Sysling* 

Leiden University, Leiden, Netherlands

*Corresponding author. Email: f.h.sysling@hum.leidenuniv.nl

Keywords: history of science; 1968; historical method; archives; global history

Kapil Raj is a research professor in the history of science at the École des hautes études en sciences sociales in Paris. He writes on intercultural encounters and the making of scientific knowledge, with a focus on South Asia and its connections to the rest of the world. His book *Relocating Modern Science: Circulation and the Construction of Knowledge in South Asia and Europe, 1650-1900* (2007) is widely known and is considered a landmark in the history of science.

Central to the book is the idea that science was not brought to South Asia from Europe, nor did Europeans simply collect knowledge from South Asia. Instead, what we now understand as “Western” science was actually co-constructed between the West and the Rest. Raj’s use of the term ‘circulation’ emphasizes how knowledge was negotiated and reformulated as a result of intercultural encounters. Raj’s research in this book already showed the importance of indigenous actors in the making and circulation of scientific knowledge, which recurs as a theme in *The Brokered World: Go-Betweens and Global Intelligence* (2009), a book he edited with Simon Schaffer, Lissa Roberts and James Delbourgo. He is also not afraid of polemics, always friendly and with a sense of humour.

Your CV tells us that you’ve had a very exciting career and made some interesting switches. You started with a BSc (Bachelor of Science) in mathematics. And now everyone knows you as a historian who studies science at the juncture of east and west. How did you become a historian? To be precise, how did you get from mathematics to history?

This was actually an epiphany which happened quite late in life. I only started becoming a historian when, at the age of 35, I stepped into the India Office Library, then on Blackfriars Road, not far from Waterloo Station in London. I asked the inquiries desk for documents about the history of the introduction of western science into India, a request that befuddled them, enough, in fact, to call their boss. The director of the library, Dr. Richard Bingle, was a kind and very patient man who on listening carefully to my request, said: “You evidently have no idea as to what an archive is. Let me introduce you to the India Office Records and how they’re organized.”

To go back a bit in time, I started with mathematics at the University of Delhi and abandoned further studies during my master’s to teach in a village in India, in the western Himalayas, as a Maoist militant. But with a difference – unlike my other colleagues and friends, I did not believe in participating in violence. Not for me. What had attracted me to Marxism was its scientific explanation of history. I had never until then liked history because all I ever learnt was dates and names, and I never got them right. It was

timeline history. Reading Marx, I saw meaning to history and in its being alive inside of society, leading inexorably towards a less exploitative society. This is what seduced me to Maoism which was then on the rise in India.

But three years of being out in the countryside finally rattled my convictions. My way of communicating the revolution to others was through Reason and through literacy. You didn't need to teach people to chop off your landlords' heads, they would know if, when, and how to do that themselves once they could think (capital-R) Rationally. But I realized peasants were savvy already, but in a different way. They had another kind of reasoning even though their actions, ways of doing – performing daily tasks in the field and at home – were recognisably similar to others in the world. That's what led me back to university with the question: is it possible to have many ways of reasoning?

For me, as an undergraduate student there was just one Reason which was modelled on mathematics. Mathematical reasoning, like the construction of a Euclidian proof in geometry, was the way in which I conceived that all of society ought to be working. And Marx provided a similarly rational framework to understand the course of history. However, confronted with another reality – that of the inhabitants of the Himalayan village – things weren't so easy anymore. I realized that the contrary of mathematical and Marxist thinking was not necessarily superstition. To explore this question, I wanted to go back to study, to the university, but I had no idea of where to go or which discipline would provide answers.

The anthropologists at Delhi University did not show any interest in my question, but the philosophers at St. Stephen's College did, so I enrolled there for a master's degree with a specialization in philosophy of science, which I later pursued in Paris, at the Sorbonne (*Université de Paris 1*) for my doctoral research.

The first suggestion of a major reorientation in my research came on the day of my defense. The *rappporteur* who assessed my thesis was a historian of science. At the reception after my defense, he took me aside, and said: "You know, philosophy cannot provide you an answer to the question of the possibility of many reasons. You need to look at history instead." I then followed his seminar and would see him regularly to discuss the evolution in my thinking. Thus, one day, when I put it to him that another way of formulating my question is: "How come that while both communities are Indian, my social group is different from those I interacted with in the village? What happened?" And he said: "Now, this is a historical question! It's about looking for if and when change happened and the processes that led to them." He then helped me get a grant from the British Academy to spend some months in the British archives to study this question.

Now, back to the India Office – I was really fortunate to have had Dr. Bingle introduce me to what this archive – or any archive for that matter – was all about, above all conceived of as an instrument for administrators. He explained to me its organization, the catalogues, the successive ways of cataloguing over the three-and-a-half centuries of British contact with, and rule over, India. The archives weren't organized in the same way in the beginning, in the 17th century, as they were in the end, in the mid-20th. But no matter what, the archive was always organized according to the logics of administrators, and not to answer historians' questions. So, you had to understand the logic of administrators at any time in order to understand how they classified their documents, their information. Only then could you translate your questions into their logic and look for the corresponding records. This then was my introduction to history.

What attracted you to go to France for your studies and not somewhere else? A lot of Indian scholars around that time moved to the United Kingdom or the United States. Why did you choose differently?

It had to do with the same thing that attracted me to Maoism in the first place, which was what was happening in the world at the time. The first eye-opener was the Bihar famine of

1967. Many collegemates who went there to help came back with first-hand accounts and pictures of the absolute horror of not only abject poverty, but also of people starving, dying, and skeletal children with bloated tummies. That was the same year as the Six-Day Arab-Israeli War. At the same time, through the African students in my college and at Delhi University, many of whom came from the East Coast, but also from Ghana and Nigeria, we learned about the Biafra war in which many western countries supported the Nigerian government to crush the rebellion. And then within months the U.S. launched the Tet Offensive in Vietnam, and that was just next door to us. Meanwhile, the Soviet Union rolled tanks into the center of Prague. In the circumstances, one couldn't *not* become political. It was this reality which hit many of us the world over at the same time. Remember 1968 in Berlin, in Paris, in the U.S., campus revolts, Black Power, civil rights movements... and the Cultural Revolution in China. This was a world in ebullition. It wasn't so much a revolt against things, but a strong belief that we could change this world for something else, something different, something better – which led us to start reading and discussing Marx and Engels and Lenin and Mao. And suddenly, history became different from dates. History started becoming meaningful, with a purpose.

After my MA then, I was looking to both pursue my intellectual quest and for a place, an environment, where others might also be questioning their wider political experiences, where we could discuss our own failures and perhaps look for other forms of political action. Post-1968 France was an obvious answer. My partner then was finishing her PhD in physics and for her France too was an exciting place to go to for her specialism.

Could you share with us the process behind writing your first book, *Relocating Modern Science*?

Well, the history of modern science has been commonly presented in India as a story of an imposition by the British or Europeans and their way of looking at the world on Indians. That is, Western science was taught at the expense of traditional modes of learning and teaching. My experience as a village schoolteacher had already made me question this view. Many of those I taught had been to school, and I did not see any traces of an imposition, especially since I'd seen how tenacious and resistant people were to proselytization. I also observed that people there learn in a certain way. What we now call mathematics constitute ways of reasoning that are not the same ways of quantitative reasoning of other people, even though they arrive at the same answers. That idea was reinforced when I came to France. For instance, I noticed that the French way of mathematical thinking and working out mathematical problems differed not only from the Indian way, but also from the British way! I can say more about this later. I thus set out looking for possible details in the historical records, and my encounter with the India Office archives.

For a start, I chose the introduction of western science at the Hindu College in Calcutta, inaugurated in 1817, as one case study. To my great surprise, it was not the British who imposed it, but the emerging Bengali Hindu elites who were active demanders of modern science in the early decades of the 19th century, long before it was widely taught even in Britain. As it turned out in the course of my research, this institution was a significant hub where almost all the teachers in late 19th century India came from – it was a really crucial hub. But then, as my research progressed, my questions became more focused, more clearly defined. Rather than studying a single case, or process in and of itself – say for instance the history of science education – my questions became more directed towards providing broader understandings based on a set of several case studies of scientific practices in early modern and modern India. I also decided to focus on the cases that stood out, that constituted key moments in the history of science, and many are well-known – William Jones' theory of an Indo-European family of languages, or James Rennell's first

map of India, which also happened to be the first large-scale terrestrial map made by the British, etc. Contrary to received wisdom, I discovered that these actually emerged through processes of intercultural encounter between European and indigenous practitioners.

It sounds like your research and writing process is more question driven than source driven. Would you agree? And could you perhaps tell us about a couple of these cases that you've just mentioned and how you approached them?

True, my research is essentially question driven. After years of believing in Marxism as an explanatory framework, I was sufficiently inoculated against theory-driven history, of shoe-horning evidence into an ideologically or theoretically determined boot. As I said earlier, I entered the archives with a question, and had to pick my sources according to their relevance, that is to construct my own archive, rather than go through a pre-arranged set of records. Starting with well-known events, I delve into the archives and the archives lead to people whom I then follow in the records, sometimes even in different archives, or sometimes in printed sources – I'm not an "archive snob". This at the same time throws up further questions. From a single question about a single person, you start following their trajectory through traces in various sources, in archives as much as in printed and secondary sources. It's very different from the kinds of questions that I was asking right in the beginning, which were about educational institutions. But in all cases, it's all about building a picture through clues and traces that appear in the records and related writings.

Take for example the last chapter of *Relocating Modern Science*, about the exploration of Tibet. I got the idea for the chapter quite by accident. During a long wait at Heathrow in the mid-1990s, while looking for something to read, I spotted a copy of one of my favourite books as a teenager – Rudyard Kipling's *Kim*. Rereading the book, my curiosity was suddenly kindled, and as the events described chimed with my other readings about the Great Game and the exploration of Tibet and Central Asia in the 19th century, I began to think, "This can't possibly all be fiction. There must be a strand of reality here". The Great Game and even *Kim* had been widely written about, but no one had done so from the point of view of the history of science, which is what I decided to do. What I found intriguing was that the Survey of India has been considered the standard bearer of accuracy and objectivity in geodesy all through its existence, especially in the 19th century. Yet the British geographers seemed to be using outmoded techniques of surveying, sending members of different Indian tribes to walk around all over the place and expecting to make maps of the same accuracy as the best they produced using the most sophisticated instruments of the day. But then, I ran into a lot of trouble trying to access related documents at the National Archives of India because they were considered sensitive. I had to get around the problem by resorting to the India Office and Royal Geographical Society records in London, which are not always the original records or reports of the Survey's activities but rather often reports about reports. I then had to innovate, to think of ways of trying to answer my questions, by looking at other documents within other archives to confront all these and piece together a more complete picture. I must add that I was always fortunate in getting invaluable guidance from archivists and specialists of the respective collections, especially from Dr. Andrew Cook of the India Office's map collections.

Let me give you another example. This is about serendipity, chance encounters, in my research. In 1998, I was contacted by the *Muséum national d'histoire naturelle* in Paris. They were looking for details about a hand-painted illustration in their manuscript collections, which is incidentally the front cover image of *Relocating Modern Science*. They were organising an exhibition to commemorate the second centenary of Napoleon's expedition to Egypt and hoped the painting might be Egyptian. Although I was able to instantly

disillusion them about its Egyptian origins, I could not say more for want of time, since I was then working on my Tibet project. When I finally got the time, I went back to the illustration which happened to be the frontispiece of a big but largely illegible, and anonymous, manuscript. I was told that it was a part of a set of fourteen volumes. On looking through them all, I discovered that the first two volumes were only textual, but the remainder, to my greatest surprise, consisted exclusively of paintings of almost 750 Indian plants and trees. I asked the *Muséum* for photographs of some of the images – this was before the time of scanners, digital photography, or digitised images! These I sent to art historians in India, who confirmed that the style was broadly Indian but could not really provide any more relevant information about place or date.

One key piece of information, however, came from Rosemary Crill, then a senior curator at the Victoria and Albert Museum in London, who immediately dated the frontispiece to around 1710, from the European's style of dress. I then went back to the text. I had never had any formal historical training as you know, so I took lessons in palaeography and also began to look more closely at the manuscript itself. First, the whole manuscript was written on French-made paper and bound in India in the style of East India Company ledgers. This pointed to its probable production in a European factory or port, in all probability not by an itinerant savant. Secondly, certain words in the text had a religious connotation, leading me to South Asia-related materials in Catholic missionary institutions in France. Here I hit a jackpot: a trove of letters in the same handwriting as the manuscript! That's how I slowly began to unravel the story behind it and identify the author – a French East India Company surgeon who was stationed in Chandernagore in Bengal in the first half of the 18th century who had the plants (mainly medically or commercially useful) collected from all over north India and then painted by local artists who normally painted chintz fabrics for the European market. However, another question bothered me – why was it never published? After years of searching, I found a contemporaneous report by the great 18th-century French botanist, Antoine de Jussieu that dismissed it as worthless because they didn't square with the theories on botany in vogue in France at the time.

The long and short of it is that each case study has been unique. And I mean that it was never really possible to apply a single model that had worked successfully in one case to another case. But, if not a single model, I do have a single method. Every time I start a new project, I have to start over afresh. Every time, I need to ask specific questions, to create a new set of sources for myself to provide answers to these questions. A lot of it is also perseverance. Once, I had spent many a barren month finding nothing – it was so frustrating. Then I woke up one night with a clue, and the next day, I was on track again! Serendipity has also led me to my cases, to the answers I have finally been able to provide. However, a bigger picture has slowly emerged, of the crucial importance of intercultural encounter and interaction in the making of large and important parts of modern scientific knowledge.

That sounds extremely encouraging for those of us that sometimes feel lost. You have also worked at other places before the EHESS, including a telecommunication engineering school and a physics laboratory—again, a very exciting career trajectory for a historian! Have these other experiences contributed to your views as a historian of science, and if so, how?

Of course. I had learnt about archives and how they worked, but I also lived with a physicist, and I was teaching future engineers at the *École Nationale Supérieure des Télécommunications*. This constant immersion in the everyday world of science, its nitty-gritty kept reminding me of the practical mindedness of scientists, their distance from the

world of theories of knowledge – and, surprisingly, their proximity to the mundane world of the peasants in the Himalayas!

Also, I was interested in how physics in India worked, so I spent time as a participant-observer in a laser physics laboratory in Bangalore. This is how I began to interact with the then budding Social Studies of Science community who were studying – mainly western – science from a historical, sociological and anthropological perspective. For them too it was questions first and disciplines second. I thus began to identify myself with this group, bringing to it a new set of questions from outside the west.

Your work touches upon colonialism, imperialism, indigenous scientific practices; moreover, you were a convinced Maoist for some time. How do you relate to the subaltern studies group, who were also inspired by Marxist thinkers such as Antonio Gramsci?

I could not understand how the Subaltern Group decided that they were the spokespersons for the so-called subaltern. What legitimacy do they have to ventriloquize the latter? And who are subalterns anyway? Strangely, the working class and its movements is all but absent from their reflections as few of them write about them or, indeed, about work. For Gramsci, the working class formed a significant part, to say the least, of his subalterns. For him, the duality hegemony-subaltern was relational, and subalterns, though not hegemonic, were not relegated to the margins of history; they were struggling to become hegemonic. Nonetheless, Gramsci shared with Marx an agonistic vision of history. Their respective concepts of “manufacture of consent” and “false consciousness” deny the “working class” or the “subalterns” any ability to negotiate consensus without totally renouncing their own ideas and values and, vice versa, without the dominating classes abdicating or, “consensually”, imposing theirs. In other words, they deny any form of *negotiation* agency which could function in the presence of hierarchies and power asymmetries. And things haven’t improved with the Subaltern Group’s Saidian turn which, taking succour largely from literary theory, prioritizes representation over practice and materiality. From my experience on the ground as well as from my reading of historical records, I began early on to conceive of society as far more complex than a unidimensional struggle of one class or group against another. Time and again, records showed individuals and groups negotiating their way into let’s say a “third”, or negotiated, outcome, one that is certainly not the ideal outcome for both sides but one that is mutually acceptable. History is not just black and white!

To make a detour, back to my initial question: How did one part of Indian society become “westernized”? And what did this “westernization” actually mean? This question was further complicated once I came to France and realized that what I called “westernization” was not the same as seen from France. Teaching in an engineering school, I began to realize that there were big differences between French and English in the way they taught mathematics. Fundamental operations like multiplying, dividing, and so on, were not put on paper in the same way. In other words, they were not reasoning and doing calculations in the same way as me, or the English students on exchange programmes. Another example, to give you your money back French shopkeepers calculate differently compared to India. This made me realize that, even though our ways of thinking, the steps, the operations of reasoning, are very different, we can communicate with each other. Despite differences, our worlds were not incommensurable. There seemed to be a pragmatic world of communication which looked to build bridge, rather than focus on “epistemic divides”.

So, instead of perpetually seeing the world in terms of incommensurability and resistance, or consensual submission, I see my task as having to make sense of both the workings *and* the resistance *and* – as a historian of science – the outcome, that is the novel

configuration that emerges through negotiation however asymmetrical, a configuration that often has no precursors on either side of the so-called divide.

You insist on being called a historian of science, and not a historian of knowledge. Why is that? Perhaps this is another way of asking: how do you perceive the difference between knowledge and science?

The history of knowledge is a budding discipline, increasingly popular notably in the German-speaking world. My problem with it is: what does one mean by knowledge? It can mean anything at all, it's too general: an artisan has knowledge; a cook has knowledge; a writer has knowledge, politics is also a form of knowledge; in fact, all human activity is founded on, and even produces knowledge. Our knowledge comes partly from our manual skills, our cultural and tacit knowledge, our social skills, etc., and of course we ought to be studying them. But, the blanket term history of knowledge begs the question as to what is *not* knowledge? How do you use an umbrella term like this? A word, any word, has, and needs, boundaries.

On the other hand, science, although part of the broad domain of knowledge, has a much more circumscribed meaning. Over the past century and a half, capital-S Science has come to occupy a crucial role as the principal arbiter in every aspect of human life and public concern – material, cultural, economic, political, social, environmental, and biological. It is both the emblem of modernity and the yardstick for measuring economic and social development internationally. The history of modern science is thus indispensable to an understanding of the contemporary world. Ironically, even religious fundamentalists and ultra-nationalists, like the current dispensation in India, have to flag – or even invent – certain knowledge achievements of their ancestors using capital-S Science as a reference.

Science is then a label conferred on those at the apex of forms of knowledge. But, while this bouquet of knowledge fields is historically contingent, it is almost invariably deemed to originate in the European intellectual tradition, implying geo-political considerations as well in the process of hierarchization. Other elite knowledge practices from elsewhere in the world are condescendingly labelled “non-Western” science – I’ll come back to this in a minute.

So, by looking at knowledge simply as *knowledge*, assuming that one form of knowledge is on a par with another, without any relational hierarchies, one elides questions of authority, of hierarchy, of power relationships within the world of knowledge, of skills. A label, like science, is conferred on certain forms of knowledge, which gives them certain rights and authority, while others don't have that authority. But if you level everything out as an historian of knowledge, you've thrown the baby out with the bath water. Everyone at one level is a historian of knowledge. But some do the history of elite knowledges which stand above other forms of knowledge, and which have authority over them, as well as power to name things. Since the contents of science change over time – for instance, mathematics and physics enjoyed prime status for most of the 20th century, giving way to the climate, neural and life sciences in the past decades – historians of science thus also have to account for the changing hierarchies and authority, bring to light the complex processes underlying these changes. It is also crucial to remember that what we today call the humanities were an integral part of elite sciences until the turn of the nineteenth century – often it was the very same people who were practicing both natural and human sciences. At the same time, they also have to study the inextricable relationships between the natural sciences and other forms of knowledge – artisanal, commercial, bureaucratic, and even culinary. In recent decades, questions of gender and the relationship between gender and the content of science itself have taken a significant place in the

history of science. But this recognition has been the result of a long struggle by feminist and gender conscious historians.

In a similar way, and in spite of sharing with my history of science colleagues fundamental ways of understanding the making and spread of science, a common language, problematics and methodological perspectives, I too had to struggle and fight hard to get the history that I do, a history of forms of knowledge to be recognised as the history of science. For, as I was saying, my initial question was about rationality: Why are certain ways of doing and thinking – certain knowledges – called science, and others not? This then is typically a question for the historian of science and not for the historian of knowledge. Then, as my questions evolved, they shifted to the making of crucial parts of what is labelled modern science outside the West, through the kinds of intercultural encounters between Europeans and non-Europeans that I spoke about earlier. However, these constructions can neither be properly classified as western nor non-western. Here again, I have had to confront the well-entrenched dogma among my colleagues of the Western origins of science, encapsulated in the title of my book, *Relocating Modern Science*.

To sum up then, while science is a form of knowledge, and all historians of science are trivially historians of knowledge, they additionally focus on the relationship between different forms of knowledge and their changing status and hierarchies in history.

Are there other labels besides ‘historian of science’ that you identify with?

Of course, yes, like many historians of science focusing on case studies, I’ve been deeply inspired by microhistory. I’d also say I’m a cultural historian, as well as a historian of empire in that I examine the interrelationship, the organic interdependence, between modern science and empire, which is political history too, it is central to the politics of European expansion and imperialism as much as it is to the functioning of other early modern empires, like the Ottoman empire, the Qing empire, the Mughals too.

Could you expand a bit about that, how science is politics?

Science is politics for a number of reasons. Firstly, the labelling of any knowledge form as science, as I just said, is itself a political act in the etymological sense of the word, in that it is a result of a group decision that confers a status of power to it. Secondly, the advancement of knowledge in any field requires the allocation of resources, human and material, and these too are political choices. For instance, we are all aware that investing in fossil fuel research or in environmentally friendly research for energy needs is an essentially political decision. Resources being finite, choosing one option necessarily closes the door for others, opens the door to one set of research problems in preference to another.

Would you have any final words for aspiring historians?

Are you familiar with L.P. Hartley’s novel, *The Go-Between*? His very first sentence is: “The past is a foreign country, they do things differently there.” Yes, they do things differently, but not so differently that we as historians cannot understand them. These are not people living on another planet, or in another solar system – these people were living on earth, we still belong to the same species. We are only going back in time, just as anthropologists are going out into a different space.

However, both the anthropologist and the historian are situated in time and in space. So, although we have to understand the past, we also have to assume that we ourselves are situated in our own present, that our questions and our understanding themselves are historically dependent. They are not transcendent. Also, we are situated within our own professional communities – we share a language, conventions, problematics, etc.,

with our colleagues, and are constantly engaging with them. It is crucial thus to also situate other historians' writings within their own web, so to speak. The historian is not above history. History is not judgmental. Nor is it about proving anything or following theoretical models.

That said, there is still such a thing as a historical method: although you have your historically, and politically, situated questions, the sources that you constitute, your sources have to be open to public scrutiny as should your analysis which demands rigour. You must be very careful not to let your politics influence your reading and interpretation of the sources to suit your politics. Always remember that your initial question is a question that seeks answers, not an assertion that seeks proof. History is about asking questions and, through a contextual reading of available sources, making sense of the past, about understanding it, in a plausible way, even if the resulting picture is not a neat and clean one. As Salman Rushdie once remarked, "If history creates complexities, let us not try to simplify them."

Neilabh Sinha is a PhD student at the Institute for History in Leiden, working on early modern Eurasian empires and their courtly cultures.

Melinda Susanto is a PhD student at the Institute for History in Leiden, working on the circulations of medical knowledge and goods between the Indian Ocean and the early modern Dutch Republic.

Fenneke Sysling is an assistant professor at the Institute for History in Leiden, specialized in the history of science and Dutch colonialism.