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ARTICLE

Questioning Basalla's question (yet again): The view from cognitive history

Subrata Dasgupta[1]

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Abstract
In 1967, George Basalla posed the question: "How did modern science diffuse from Western Europe and find its place in the world?" Here, we consider this question particularized to India: "How did modern science find its place in India?" In answering his own question Basalla posited a 3-phase model–one that has been severely criticized by some prominent social historians and sociologists of modern Indian science. In this paper, we question Basalla's question anew from the perspective of cognitive history, wherein the focus is on specific, individual scientific productions as knowledge-consuming/knowledge-generating creative phenomena. Drawing on Asiatic Society records on the work of British scientists in nineteenth century India and contributions made in the same period by five Indian pioneers of science, namely, Radhanath Sikdar, Yesudas Ramchandra, Mahendra Lal Sircar, Jagadis Chandra Bose and Prafulla Chandra Ray–each of whom is presented here as representing a distinct aspect of science–we argue that: (1) the biographical records of the British scientists in nineteenth century India does not resonate in important ways with phase I of the Basalla model; (2) with one notable exception, the work of the British scientists and that of the Indian protagonists ran on essentially parallel tracks; (3) the distinction between Basalla's phase II 'colonial scientist' and phase III 'independent scientist' dissolves in the case of our Indian protagonists; and (4) most importantly, we answer the Basalla question by way of identifying specific and distinctive contributions made to the knowledge-consuming/knowledge-producing enterprises constituting creative science. In the final analysis, we suggest that the Basalla model played virtually no role in understanding or explaining the genesis of modern science in India as practiced by our protagonists.

Keywords Basalla's model · Colonial science · Independent science · European science · Centre-periphery binary · Field sciences · Laboratory sciences · Asiatic society · Cognitive history · Creative phenomena

1 Basalla's question

In 1967, the American historian of science and technology, George Basalla published a paper in the journal *Science* wherein he posited a qualitative three-phase model to describe how modern science was introduced into non-European countries (Basalla, 1967). Within a decade, the Basalla model would enter the historiography of science in colonial India as, in the words of Warwick Anderson (2018), "a handy straw man" to be critiqued and refuted for its alleged naivety, its un-nuanced character, its sweeping generalization concerning the domain of science itself and, perhaps most damningly, its picturization of eighteenth and nineteenth century India as a passive repository of Western scientific knowledge.

Basalla had posed the question: "How did modern science diffuse from Western Europe and find its place in the rest of the world?" (p. 611). Let us instantiate this question to the particular domain of India: *How did modern science find its place in India*?

Our aim in this paper is to revisit this question. We first reexamine some of the criticisms Basalla's three-phase model has received, mainly from Indian social historians and sociologists of Indian science. We then question the Basalla question anew but now from the perspective of *cognitive history,* thus making a transition from the general to the particular. Drawing on Asiatic Society records on the one hand, and the contributions made in the nineteenth century by five especially significant Indian scientists on the other,

✉ Subrata Dasgupta
subratadasgupta1044@gmail.com

[1] School of Computing and Informatics, University of Louisiana at Lafayette, Lafayette, LA 70504, USA

we argue that: (a) the biographical record of the British scientists working in nineteenth century India does not resonate in an important way with 'phase 1' of the Basalla model; (b) with one notable exception, the work of the British scientists and that of the Indian protagonists run on essentially parallel tracks, the former having no bearing or influence on the latter; (c) the distinction between the phase 2 'colonial scientist' and phase 3 'independent scientist' in the Basalla model dissolves in the case of the five Indian pioneers; (d) most importantly, we answer the Basalla question by way of the *contributions made to science in India* by our protagonists. Toward this end, we focus on these protagonists' actual *acts of production* in the realm of science. We argue that collectively, they contributed to the *problem-solving/knowledge-generating practice* characterizing modern science. In the final analysis, our cognitive-historical investigation suggests that the Basalla model plays no meaningful role in understanding or explaining the genesis of modern Indian science as manifested by these Indian pioneers in the second half of the nineteenth century.

2 The Basalla model recapitulated

The three phases of Basalla's model overlapped to some extent. In *phase 1* "the nonscientific society or nation provides a source for European science" (p. 611). Basalla carefully notes that by "nonscientific" he means the absence of *modern* science and not the sciences of antiquity or of the medieval age of the kind found in India or China. In this phase, Europeans visit the 'nonscientific' countries, survey their flora, fauna, terrain, and peoples, and take this knowledge back to their home countries. The 'field sciences'—botany, zoology, geology, geography, cartography, geodesy, meteorology, and, perhaps secondarily, anthropology and ethnology—are the relevant modern sciences at work in this phase. The 'laboratory sciences' are absent, notably physics, chemistry, and experimental biology. As for mathematics and astronomy, their presence manifests in applications such as geodesy, geography, and cartography.

Phase 2 is a period of what Basalla characterizes as 'colonial science'. This overlaps in time with phase 1, but at some point, it becomes dominant after phase 1 ends. If natural history and the field sciences dominate phase 1, they still continue to prevail in phase 2, but the range of scientific activity is expanded to include the laboratory sciences. Basalla introduces the term *colonial scientist* to refer to the practitioners of phase 2 science. This is a new scientific breed: a "native or a transplanted European colonist or settler", perhaps (though Basalla does not mention this), a teacher or professor in newly established colleges teaching science, medicine, and—a later entry—engineering (Ramnath, 2017; Arnold, 2000, esp. ch. 4), as was the case in mid-nineteenth century India. But—and this is significant in Basalla's model—he is scientifically educated and trained in European institutions or, if not formally so trained, had studied the texts and works produced by European scientists. The work of the colonial scientist is founded largely "on institutions and traditions of a nation with an established scientific culture" (p. 613).

Basalla suggests a kind of evolutionary relationship between phases 1 and 2: the latter comes into being when "a small number of native workers or European settlers" who had been phase 1 participants "*gradually shift their interest* to a wider spectrum of scientific activity" (p. 617; my italics).

Interestingly, Basalla's exemplars of "the heroes of colonial science" (p. 614) are Benjamin Franklin in America and Mikhail Lomonosov in Russia. A Japanese physician, Hashimoto Sokichi is the one figure mentioned from a non-white society. There is no mention of colonial scientists from India.

In *phase 3*, the period of the "independent scientific tradition" (p. 617), scientists in the non-European realm seek to establish 'a science of their own'. The urge to do so, Basalla claims, is partly driven by nationalistic sentiments, but there are also other "more fundamental forces" at work (p. 617). The preceding phase of colonial science itself is such a force, for the colonial scientist desires to establish a social infrastructure—scientific societies, journals, and so on. In phase 3, the colonial scientist metamorphoses into an independent scientific practitioner. However, this metamorphosis, according to Basalla, demands certain social, institutional, and educational conditions to be met.

As in the transition from phase 1 to phase 2, Basalla hints, there is an evolutionary relationship between phases 2 and 3: "Colonial science contains in an *embryonic form*, some of the essential features of the next stage" (p. 617; italics mine). Yet Basalla hedges in elucidating the actual nature of this 'embryonic form'. The reader must infer that it entails such goals as the creation of such "institutions and traditions"—"a modest amount of scientific education", "native scientific organizations", the "establishment of a home-based scientific journal"—that will lead to an "independent scientific culture" (ibid). Presumably, such goals had already taken shape in a preliminary sort of way in the mind of the colonial scientist, and their explicit identification became a marker of the emergence of phase 3.

But are the phase 2 colonial scientist and the phase 3 independent scientist the one and the same person? Did the former *evolve into* the latter, or are they of different kinds? Basalla remarks, somewhat cryptically, that "The colonial scientist... oriented toward an external scientific culture *is to be replaced* during the course of phase 3" by a scientist

whose social, educational, intellectual, and cultural ties are with the "country in which he works" (p.617; my italics). Again, the reader must make his/her own inference as to what "replaced by" might mean.

Before leaving this section, one point should be noted. Basalla cautions the reader that his model is "preliminary", a "heuristic device" to help inaugurate "discussion of a neglected topic in the history of science" (p. 611).

3 The Basalla model and its (mainly) Indian critics

Notwithstanding this last caveat, historians and sociologists of Indian science have poured much scorn on what has come to be called Basalla's 'diffusionist' model. In the words of Deepak Kumar (1997), "Basalla generalizes with a sweeping flourish". "He is cavalier with both time and space... assumes uniformity and homogeneity where none exists" (p. 3). Basalla's model, Kumar notes, is "passive"—knowledge flowing from the colony to Europe in phase 1 and the converse in phase 2, the colonial scientist being dependent on and yet not a fully participating member of the European scientific culture. "The umbilical cord never gets snapped... in a Eurocentric model" (p. 4).

David Arnold (2000) concedes that in the absence of a better model, Basalla's scheme continued to serve as a framework for discussion, though not without qualification. Furthermore, the model was one of the first to situate colonial science in the context of the appearance and maturation of modern science in the non-European colonies. For Arnold, the major flaw was that the monolithic assumption underlying the socio-cultures of such countries as Japan, India, and Australia rendered the "standard trajectory" posited by Basalla highly improbable (p. 11). Moreover, Arnold argues, it is difficult to imagine how Western science could have functioned in such domains without the intervention of local knowledge and the agency of the local peoples—"local savants, scribes, interpreters... artists, fishermen, and forest-folk" (p. 13).

Perhaps the severest and most extensive censure was by Dhruv Raina (1999). His objection lay on a number of fronts: (a) in Basalla's failure to recognize the heterogeneity of cultures and countries; (b) in his inability to distinguish between settler-colonies (as in Australia and America) and "non-settler outposts of empire" (such as India)—thus "We are forced to concede that the term colonial was [for Basalla] no more than a euphemism for any 'other' of Western Europe that produced modern science" (p. 507); (c) in Basalla's oblivion to the variable meanings ascribed to 'science' across cultures and historical time; (d) in his failure to recognize what Raina labels the "politics of knowledge" in the diffusion of science from West to non-West—meaning the role of imperial strategies in deciding what branches of science should or should not be pursued in the colonies. Here, Raina cites one of the pioneer historians of science in India, S.N. Sen (1966), who noted that while the field sciences were imported into India in the nineteenth century as being relevant to Britain's imperial and colonial interests, the laboratory sciences, being of lesser political import, entered only incrementally. (e) And finally, Raina notes, there was Basalla's Eurocentrism.

Another modern historian Kapil Raj (2007), while describing the Basalla paper as "epoch-making" and acknowledging that in its own way it has "dominated thinking amongst historians and sociologists of science working... outside the West" (p. 3), devotes much of his attention to what he describes as the "spaces of circulation" of knowledge between South Asia and Europe, restricting his focus to the field sciences and the "co-production of the local and the global" (p. 22).

4 An elephant in the room

There is an elephant in the room in Basalla's discourse which Raina mentions but almost as an aside. This is the 'center-periphery' binary. Because of Basalla's perceived Eurocentrism, because in his model science 'diffused' from West to non-West, the West is de facto the center and the non-West the 'periphery'. In such a bipolar universe, the periphery is by and large the passive recipient of scientific knowledge created at the center. Furthermore, Basalla argues, this center-periphery binary also creates a social situation in which colonial science (in phase 2) is inevitably inferior to the science at the center.

Blanket assumption of this unidirectional flow of knowledge from West to non-West has, however, been contested by Pratik Chakrabarti (2004). He admits the reality of Eurocentrism in scientific practice and knowledge production. But he also notes the richness of the knowledge flow from India to the West in the field sciences and has documented the sense in which the periphery had its own center, notably the Asiatic Society established by William Jones in Kolkata in 1784. What is the center, and what is the periphery? Chakrabarti remarks that it may be a contextual matter rather than an absolute. The instance of the Asiatic Society and the investigations conducted on its behest serve to suggest, Chakrabarti tells us, that "science in the colonies no longer seemed a mere extension of European methods" (p. 59). We will return to the case of the Asiatic Society shortly.

Very recently, the center-periphery binary implicit in the Basalla model has been addressed forcefully by a philosopher of science, Deepanwita Dasgupta (2021). Like Arnold and Raj, she admits that while this model has been

rejected by many, "nothing adequate so far has been put into its place" (p. 4). Even those that some Indian historians have proposed—she cites Deepak Kumar (1997), Gyan Prakash (1999) and Rajesh Kochhar (2005) as exemplars—retain the Basalla 3-stage diffusionist model as a pentimento. The peripheral researcher's agency in the creation of scientific knowledge is thus obscured, and "we are thus encouraged to see such science only in terms of a copy or an imitation, and not as any kind of an active cognitive response" (p. 95).

5 Some further elements of the schism

The schism between the Basalla model and its critique deserves some further commentary.

One. In contesting the Basalla model, his critics chose to largely ignore the principal *experimental* sciences that came into their own in the 17th through 19th centuries and achieve a kind of leadership in the realm of the sciences, notably, physics, chemistry and biology (the latter to be distinguished from natural history). Basalla himself was unequivocal in this regard. By 'modern science'—his concern—he meant "the philosophical viewpoint, experimental activities, and social institutions" that emanated from the Scientific Revolution in Western Europe in the sixteenth and seventeenth centuries (p. 611).

Once these experimental or laboratory sciences are admitted into the discourse a very definite *methodological apparatus* is concomitantly admitted—an apparatus without which the very idea of 'modern' science is meaningless. This apparatus, embracing both in situ and in vitro activities—entailing both observation and experimentation, conjecturing and refuting, operating within research traditions and paradigms, pursuing research programmes—must therefore be taken into account in any critical discussion of the Basalla problem.[1] In particular, it imposes a filter determining which of the field sciences—and in what fashion—are admissible into the discourse on modern science. For example, such fields as geography, anthropology, and ethnology recede to the periphery of 'modern' science while others such as geology, zoology, physiology, and botany entail, in addition to observation, analytical and theoretical inquiry, consistent with the demands of the methodological apparatus. In other words, I am suggesting, Basalla's critics' idea of 'modern science' may be at odds with what Basalla meant by this term.

Thus, for instance, Raina's observation of the variable meanings of 'science' across cultures and historical times seems problematic in relation to the idea of *modern* science, as the term was understood by mid-nineteenth century, particularly as Raina offers no examples. And while, in Basalla's phase 1, the 'local knowledge' possessed by the 'local peoples' described by Arnold—"local savants, scribes, interpreters... artists, fishermen, and forest-folk"—may indeed be invaluable sources of *information* and *data* for the Europeans, they do not necessarily become scientific *knowledge*.[2] As yet another example, Gyan Prakash (1999) sees 'colonial science' as largely referring to ethnology—the colonial science of native man, India's "tribes and races" (p. 29)—but then his 'colonial science' also meanders into the realm of what Alan Sokal (2008) characterized as 'pseudoscience', notably mesmerism.

Two. Basalla, though writing about *colonial* science and the high age of European imperialism, seemed to have ignored the tide of postcolonialism and its reshaping of many matters of intellectual thought—including science. It is true that some of the most influential literature on postcolonialism came into being from the decade following the 1960s when Basalla's article appeared. And herein lies perhaps an important element contributing to the schism between Basalla and his critics: *he* was writing as a pre-postcolonial historian; *they* were writing as postcolonial scholars. Basalla's universe of discourse, *ca* 1967, and that of his detractors, *ca* 1990 and after, were perhaps disjoint. This postcolonialist influence might explain Raina's notion of the time-and culture-variable meaning of 'science'; or Arnold's notion of the intervention of local knowledge in the practice of science in the non-West; or Raj's remark concerning the making of scientific knowledge as demanding the "co-production of the local and the global".

Three. Perhaps a more fruitful consideration is to situate Basalla's model within the analysis offered by S.N. Sen (1966) in the year *before* Basalla's paper appeared. One of Sen's insights was that the period corresponding to Basalla's phase 1 *had nothing to do with the spread or introduction of Western science to India.* Rather, Sen suggested, the Europeans' work, limited to the field sciences, was simply *the extension* of the European scientific project; and it was the field sciences that were of their concern because of their economic, political and imperial importance. The laboratory sciences, not being of strategic significance, were not part of the science the West pursued in India, either in phase 1 or 2 in the Basalla scheme.

Four. While not referring to Basalla at all, sociologist Joseph Ben-David (1984) offers an explanation for the more general question of the tardiness and irregularity of the growth of scientific knowledge in certain traditional

[1] Here I am, of course, referring to the various models of scientific methodology as proposed by Popper (1968), Laudan (1977), Kuhn (2012) and Lakatos (1978) and their followers.

[2] I explore the thorny distinction between 'information', 'data' and 'knowledge' in Dasgupta (2016).

societies in terms of the *role* scientist play in such societies. Science had to be "needed" by a society; only then would the role of the scientist be recognized; only then would that society accept science as a worthwhile enterprise (p. 31). The emergence of such a need was determined by the prevalence of certain social conditions that "did not exist in any of the traditional societies" (p. 33). Ben-David's book explores and elucidates such social conditions that emerged, first in ancient Greece, then in Renaissance Italy, and subsequently, through Western Europe in the early modern period. His study is an out-and-out *sociological* history of science (as one reviewer recognized). For our purposes, what is most interesting here is Ben-David's identification of the scientist's role. By this, he meant the *intellectual uniqueness* of the scientist within the realm of humanity. In his words:

> He was a person studying nature rather than the ways of God and man, and using as his intellectual tools mathematics, measurements and experiment instead of relying on the interpretation of authoritative sources, speculation, or inspiration. He was a person viewing the state of knowledge in his time as something to be constantly improved on in the future rather than something to be brought up to the standards of a golden age in the past. This new scientific role was recognized and accepted as equal in dignity and superior in the scope of its applicability to that of the traditional philosopher, theologian, or literary man (p. 170).

The scientist, in this characterization, is a person of *pure* science, of *natural* science, of *science for the sake of science*. He or she has "the future in his [or her] bones", to quote C.P. Snow (1993, p. 10), not a material or economic future but one which we might call 'epistemic future'. And this intellectual role of the scientist had to be recognized by a society, culture, or country before it could secure a place in that domain. This is a far cry from Basalla's postcolonial critics' insistence that there is nothing universal in science; that scientific knowledge can be "local everywhere" (Raj, 2007, p. 8)—as natural knowledge as much held in the minds of "local savants, scribes, interpreters and artists, fisherman and forest-folk" (Arnold, 2000, p. 13) as in the minds of trained observers of the natural world; that, as Paolo Palladino and Michael Warboys (1993) put it, "western methods and knowledge were not accepted passively but were adopted and selectively absorbed in relation to existing traditions of natural knowledge and religion and other factors" (p. 99).

6 The case of the 'Asiatic Society scientists'

It is instructive to situate Basalla's model—particularly its phases 1 and 2—in relation to the early life and times of the Asiatic Society. This organization, inaugurated in 1784, was the brainchild of the remarkable polymath William Jones (1746–1794) and was designed to promote the study of "*Man* and *Nature*" as they related to Asia, in particular, India (Jones, 1784; italics his). Jones's project was to (re) discover Asia itself. And the history of the sciences of Asia was within the purview of this project.

However, when we examine the *Centenary Review of the Asiatic Society 1784–1884* (Roy Chaudhuri, 1986; originally published in 1885), we find not only accounts of the history of the sciences of Indian antiquity and the Middle Ages but also science as it was practiced in India in Jones's time and after. Thus, we may expect to find resonance of its contents with at least phase I of the Basalla model. We may expect to find evidence of Basalla's phase 1 science in action. In fact, we find a more complex situation.

The *Centenary Review* includes an 88–page, densely printed index listing the scientific papers that had appeared in the Society's annual *Asiatic Researches,* which then became the *Journal,* and its monthly *Proceedings* between 1788 and 1882. The papers were almost entirely composed by British authors. Their domains were primarily, but not exclusively, the field sciences—geology, zoology, botany, geography, ethnology (some aspects of the physical sciences—meteorology, hydrology, electrical phenomena—and economics and statistics were also studied). In the review chapters describing the natural sciences, written by the geologist Pramatha Nath Bose (on whom more later), the author ruefully admits that up to *circa* 1884, *apropos* chemistry, "there had been but few laboratories worth the name, and we have had few competent men with leisure to devote to lengthened chemical research" (p. 101).

This writer found just five titles with Indian authors. Two under 'mathematical and physical science', one under 'geology', and two under 'geography'. Of these, the only name familiar to this writer was Gourdas Bysak (or Basak) (1826–1899), an alumnus of Hindu College, Kolkata, and lifelong friend and confidante of the poet Michael Madhusudan Datta (Dasgupta, 2007, p. 91 *et seq),* who had published a geological paper. It would be interesting to know something about the other authors (P.C. Ghose and C.S. Chatterjee in mathematical/physical science and Mohun Lal in geography).

Arguably, one of the two most distinguished Indians cited in the *Centenary Review* was Nain Singh (1820–1882), who was despatched by a prominent British geographer to secretly explore, survey, and map parts of the trans-Himalayan region at a time (1865–1875) when few outsiders

had explored that region. Singh's local knowledge of Tibet, language, and customs were important for this venture. Such were his accomplishments that the Royal Geographical Society in London awarded him one of their royal medals for his work.

The other Indian mentioned in the review chapter on natural science and who merited the title of a *scientific researcher* was Radhanath Sikdar (1813–1870), also an alumnus of Hindu College, Kolkata (Dasgupta, 2007, pp. 83–89). Sikdar learned science and mathematics at Hindu College as a pupil of Robert Tytler, who introduced him to Newton's *Principia*. Employed as a 'mathematical computer' in the Great Trigonometrical Survey, Sikdar's field was geodesy, wherein his accomplishments were considerable, not least being the modification and improvement of some of the rules of projection George Everest (1790–1860) had formulated (Chakravarty, 1995). In the *Centenary Review*, however, Sikdar is mentioned as the compiler of meteorological observations, published in the Society's *Journal* and *Proceedings* from 1853 to 1864.

So, what are we to make of these reports in the *Centenary Review* in relation to the Basalla model? At first blush, it seems that the science conducted in India between 1788 and 1882 and published under the aegis of the Asiatic Society coheres closely with Basalla's phase 1 science. But a closer scrutiny suggests otherwise.

According to the Basalla model, phase 1 is characterized by Europeans *visiting* the colonies (or other unexplored lands), *collecting* its flora, fauna, fossils, and geological specimens, studying its physical features, and *returning* to their homelands with their scientific booty (Basalla, 1967, p. 611). Of course, there is evidence of this scenario not just in India but elsewhere. Perhaps the most celebrated instance is of Darwin on his *HMS Beagle* voyage of 1831–1836. He stopped at sundry points along the South American coastline and collected samples, eventually returning to England with his treasures. In India, the distinguished botanist, geologist, and paleontologist Hugh Falconer would return to England with a massive collection of plants, fossils, bones, and geological specimens. But unlike Darwin, who only visited places and moved on, Falconer lived, worked, and was employed in India for some 12 years before returning to London due to ill health.

Falconer's situation happens to represent the common scenario for a wealth of British investigators.[3] Here, I list just a few of the more prominent ones in birth-year order.[4]

(1) Reuben Burrow (1747–1792). Mathematician and surveyor.
(2) William Roxburgh, MD, FRSE, FLS (1751–1815). Botanist. Considered the *doyen* of Indian botany. Superintendent of the Royal (now Indian) Botanical Garden, Kolkata.
(3) William Lambton, FRS (*ca* 1753–1823). Geographer, geodesist. First superintendent of the Great Trigonometrical Survey.
(4) Nathaniel Wallich, MD, FRS, FRSE (1786–1854). Botanist. Superintendent of the Royal (now Indian) Botanical Garden, Kolkata, and first curator of the Indian Museum.
(5) George Everest, FRS, FRAS, FRGS (1790–1866). Geographer, geodesist. Second superintendent of the Great Trigonometrical Survey.
(6) Henry Wesley Voysey (1791–1824). Geologist and mineralogist and, by all accounts, the *doyen* of Indian geology.
(7) James Princep, FRS (1799–1840). A polymath with interests in metallurgy and meteorology, best known for his decipherment of the Brahmi script.
(8) Thomas John Newbold, FRS (1807–1850). Geologist, especially of southern India.
(9) Hugh Falconer, MD, FRS (1808–1865). Geologist, botanist, paleontologist. Superintendent of the Royal (now Indian) Botanical Garden, Kolkata, and early exponent of the evolutionary theory of punctuated equilibrium (made famous a century later by Niles Elridge and Stephen Jay Gould).
(10) William Brooke O'Shaughnessy, MD, FRS (1809–1899). Electrical research, chemistry, medicine. Established the telegraph system in India. Professor of chemistry, Calcutta Medical College.
(11) Thomas Oldham, FRS (1816–1878). Geologist and first superintendent of the Geological Survey of India.
(12) Henry Benedict Medlicott, FRS (1825–1905). Geologist, coiner of the term 'Gondwana', and recipient of the Wollaston Medal of the Geological Society of London.

Each of these scientists lived long enough in India to hold influential positions; each made significant contributions in their respective disciplines of science, more often in multiple disciplines, mainly in the field sciences; each published extensively in the journals of the Asiatic Society; yet each (save for Reuben Burrow) came to be sufficiently known in Britain to be elected fellows of one or more of the most prestigious British scientific societies and, in some cases, to be recognized with awards.

They (and all the others whose works are reported in the *Centenary Review*)—whom we may call 'Asiatic Society scientists'—arrived in India as soldiers, adventurers, medical

[3] Some returned to their homeland because of ill-health; some retired and returned; others died and were buried in India.

[4] The abbreviations stand for the following: FRS—Fellow of the Royal Society (of London); FRSE—Fellow of the Royal Society of Edinburgh; FLS—Fellow of the Linnaean Society; FRAS—Fellow of the Royal Astronomical Society; FRGS—Fellow of the Royal Geographical Society.

men, preachers, surveyors, and, of course, employees of the East India Company—in fact, as anything but scientists. They cohered in this regard to the profile of Basalla's phase 1 investigators. But in their pursuit of science they were, as Basalla put it, heirs to the Scientific Revolution and the culture of the sciences that had emanated therefrom. Their ultimate goal may have been to advance British imperial and economic interests, but their proximate goal was to generate new knowledge about the subcontinent. They were in the immediate business of practicing *modern* science.

Thus it makes little sense to refer to them as phase 1 natural historians, observers, and collectors *or* as phase 2 colonial scientists. *They were both*; or rather, they were quite simply operating as natural scientists whose domain happened to be India.

7 S.N. Sen's contention, once again

Consider Sen's (1966) contention that the British field sciences in India had nothing to do with the spread or introduction of modern science to India. As a blanket statement, this is obviously not true. As we have just noted, regarding the 'Asiatic Society scientists' there was a scientific *movement* in India from the late 18th through the 19th centuries. Presumably, Sen meant that this movement had nothing to do with Indians, that (except for the few Indians mentioned in the *Centenary Review*) the science generated by the British in India simply bypassed the Indian consciousness.

Sen may well have been right. This may have been because of the Asiatic Society's apparent dismissiveness concerning Indians as possible members of its community.[5] We are of course aware of the eighteenth and nineteenth century British—or perhaps more generally European—scepticism or contempt concerning the Indian's capability to conduct investigation along the lines of modern science (see, e.g. Michael Adas, 1989, pp 101–102). Even in the late nineteenth century, the young Pramatha Nath Bose (1855–1934)—who would write the section on the natural sciences in the *Centenary Review*—freshly returned from England armed with a B.Sc. degree from London University, a fellowship of the Geological Society of London, brief employment with the Royal School of Mines, and a publication in Britain's leading geological journal (Bose, 1880) was told by H.M. Mendicott, then superintendent of the Geological Society of India, that Indians were "utterly incapable of any original work in natural science"; that the "scientific chord" had not touched Indians (quoted in Arnold, 2000, p. 139).

So as a general trend, fed by the racist prejudice that was an abiding theme, not only in the realm of science but across intellectual disciplines (see Dasgupta, 2007, pp 34 *et seq*; pp 128 *et seq*), Sen's surmise, *vis-a-vis* Indians may well be correct. Yet science *did* arrive at Indian doors. To reiterate the Basalla question, how did this happen?

A strange, indeed ironic, aspect is that notwithstanding the relevance of the Basalla model to the Indian context (as evidenced by the foregoing discussion) *science in India merits scarcely a mention in Basalla's article.* Not a single scientific worker in India—British or Indian, colonial or independent—is mentioned by name; not a single instance of actual science in the subcontinent appears in the sections on phase 1, phase 2, or even in the extensive discussion on phase 3 science. It is odd that despite the obvious relevance of the scientific activity of the Asiatic Society authors to Basalla's phase 1, the society and its publications have no place in Basalla's discourse. In fact, the extensive bibliography does not cite a single publication on science in precolonial or colonial India.

8 The view from cognitive history

How did modern science find its place in India? As we have seen, Basalla himself and his Indian critics viewed this question from a social–historical/sociological perspective. Suppose we postulate an alternative view that posits that the proper study of science is the science itself. "Science generates knowledge", declared physicist and philosopher of science John Ziman (2000, p.5). And they do so by solving problems, as philosophers such as Karl Popper (1968), Larry Laudan (1977), and Thomas Kuhn (2012) (in their different ways) have insisted. *Knowledge-centrism* and *problem-centrism* are complementary views of how scientists have actually attempted to understand the natural world.

Accepting this leads us into the domain of human *cognition* since problem-solving and knowledge generation are cognitive issues. Thus, a very different (and perhaps more fruitful) approach to addressing the Basalla question would be to move from the realm of social history and sociology to that of *cognitive history*.

Cognitive history—so named by historian and psychologist of science Nancy Nersessian (1995)—is a relatively young inter-discipline or practice that came into being by way of the pioneering work of psychologist Howard Gruber (1981) on Darwin's invention of the theory of natural selection. In brief, it is a symbiosis of the empirical methods of historical inquiry and theoretical concepts drawn from cognitive science. It seeks to offer

[5] The first Indian to become a member of the Asiatic Society was Dwaraknath Tagore, Rabindranath's grandfather, in 1832, almost a half-century after the formation of the Society. Dwaraknath was, of course, not a scientist.

cognitive explanations for creative phenomena (Dasgupta, 2019). Here, we first present very briefly the cognitive model underlying cognitive history.[6]

An individual's world is a composite of an objective environment—the physical world, the social world of which the person is a denizen, and the cultural world of artifacts the person interacts with—and a subjective inner state, the contents of her brain/mind complex. The former constitutes the person's *ecological space,* and the latter her *cognitive space*. The beliefs and knowledge (including self-knowledge) a person holds at any given time, her goals, needs, desires, and feelings (affects) are constituents of her cognitive space. These constituents are constantly revised with additions, deletions, and alterations as a consequence of that person's interactions with her ecological space, which is constantly changing as the outcome of the actions of physical nature and other denizens of the social world. An individual's cognitive space is as much an 'open system' as is her ecological space.

Here, we must introduce the important concept of a *schema*. The representations of beliefs/knowledge held in a person's cognitive space are organized as schemas, a theoretical concept first advanced by the psychologist Sir Frederick Bartlett (1932) but much elaborated subsequently by cognitive psychologists, artificial intelligence researchers, and cognitive philosophers (Dasgupta, 2019, pp 60–80). In very brief:

> A schema is a hierarchical and interconnected structure of beliefs and knowledge that serve as a means of representing in one's cognitive space some prototypical or archetypal concept (or ensemble of concepts). In interpreting and responding to some external stimulus or experience the person will (consciously or unconsciously) attempt to match this experience against an existing schema in cognitive space. Thus schemas serve to guide a person in making sense of new experiences.

In so doing, adjustments may have to be made to a schema. It may have to be refined or expanded or sometimes significantly altered in order to assimilate the new experience without seriously affecting the overall structure of the schema. However, in the event the new experience or stimulus cannot be matched against any of the person's stock of schemas an entirely new schema may have to be invented (Dasgupta, 2019, p. 47).

9 Creative phenomena as cognitive events

I use the term *creative phenomenon* to refer to a cognitive event that enters the cultural domain and *makes history* in some significant sense. The creative phenomenon can manifest at several different levels of abstraction (Dasgupta, 2022, pp. 5–9). For our purposes here—to address the Basalla question—the most relevant creative phenomena are at the level, which I have called the production-consumption level elsewhere (Dasgupta, 2022). Here, there are two kinds of cognitive events: singular *acts of production,* whereby an agent generates *artifacts*—that is, anything that is human-made—and singular *acts of consumption*, whereby a consuming agent's cognitive space is altered in some significant sense by *exposure* to some artifact. An artifact may be a material object (a building, a machine, a painting, etc.) or—more relevant here—an *abstract* entity (such as a theory, a law, a philosophical doctrine, a concept, an idea, a design, a procedure). Artifacts may be utilitarian (such as a machine or a scientific instrument), affective (such as a poem, a musical composition, or a sculpture), or a hybrid, manifesting both utility and affect (such as an architectural design). Or an artifact may be *intellectual* in nature, appealing predominantly to reason and logic (such as a mathematical theorem or a scientific theory).[7]

Within this framework, an act of consumption entails enriching or modifying the contents of one or more schemas in one's cognitive space by way of inputs from one's ecological space, notably ideas, concepts, theories, plans, designs, procedures, algorithms, metaphysical beliefs, and so on—a body of abstract artifacts, in other words. But, such *internalizing* of abstract artifacts is rarely a passive process; it usually demands a certain kind of need or goal to activate it. *The act of consumption is a purposive cognitive process.* And the most compelling need/goal is *the need to know:* curiosity about some aspect of the world.

The most obvious manner in which the act of consumption is effected is formal education. But that is only one such means. Writers, poets, composers, inventors, engineers, artists, scholars—and scientists—usually go well beyond what formal education offers. Autodidacts establish for themselves certain values and goals that formal education alone can rarely induce in them. In response to these goals, they observe, read prolifically, take note of ideas and things as they experience in the physical and cultural worlds.

But note that the act of consumption itself is not a creative phenomenon. We are accustomed to thinking

[6] For a far more extensive discussion of the cognitive model presented here see Dasgupta (2019, esp. pp. 1–12, 60–80); and Dasgupta (2022, pp. 1–21).

[7] For more extensive discussions of what might be called the theory of artifacts, see Kubler (1962); the collection of articles in Bensaude-Vincent & Newman (2007), especially the introductory essay by the editors; and Dasgupta (2019, pp. 13–27).

of certain *acts of production* as history-making: The annals of science, technology and art are chock-full of them: James Watt's invention of the separate condenser principle in the eighteenth century, Darwin's invention of the principle of natural selection in the 19th, Picasso's and Braque's co-invention of Cubism in the 20th, to take just three examples—each produced artifacts (all, incidentally, abstract ones) that were [in cognitive scientist Margaret Boden's (1981) term] *historically original* (H-original) in relation to human history up to the time of their creation ('past history'); further, if they would go on to influence future history they are *consequentially original* (C-original) (Dasgupta, 2018; Dasgupta, 2019, pp. 28 *et seq*). Some acts of production, if not C-original, *anticipate* future events, such as Charles Babbage's invention of the principle of the programmable digital digital computer. Elsewhere, I termed this sort of history-making *anticipatory originality* (A-originality) (Dasgupta, 2018).

In contrast, a person engaged in an act of consumption is not producing anything of historical or, consequential, or anticipatory originality; he is not making history in relation to the world at large. The act of consumption is itself not a creative phenomenon. But he *is* in the business of altering his personal cognitive space.

And sometimes this alteration of one's personal cognitive space will *prepare* the cognitive agent for possible acts of production which *might* make history. Acts of consumption are themselves not exemplars of creative phenomena, but they may prepare cognitive agents such as artists, scientists, engineers, novelists, composers, etc., to become producers of artifacts that are H-original or even C-original; in some cases, they may be A-original, regrettably not recognized in their own times as history-making but only later as having anticipated some other creative phenomenon. *The act of consumption is the means whereby the prepared mind is created for future acts of production.*

In addressing Basalla's question, but now in a cognitive-historical setting, let us keep this assertion in mind.

10 A cluster of creative phenomena

So, did the considerable and impressive body of field science achieved by the British 'Asiatic Society scientists' in India in the late-18th/nineteenth centuries bring about any significant creative phenomenon *on the part of Indians*? To address this question, we consider five Indian pioneers, our protagonists of the genesis of modern science in India.

10.1 Radhanath Sikdar's geodetic rules

The one person who certainly comes to mind in relation to what might be called *mathematical* field science is Radhanath Sikdar, mentioned earlier. As noted, Sikdar's mathematical abilities were recognized as a young student at Hindu College. His initial act of consumption—his making of a prepared mind—began by way of his formal mathematical education in Hindu College under the tutelage of Robert Tytler; it would continue 'on the job' after he joined the Great Trigonometric Survey of India in 1831 at age 18. As the story goes, his mathematical abilities were quickly appreciated by George Everest who became a kind of mentor. Sikdar was taken to Dehra Dun, where the computing office was situated. He would work there for some 15 years, during which first Everest and then his successor in 1843, Andrew Waugh (1810–1878) served as surveyors-general of the Survey.

Sikdar was both a consumer of Western science, mathematics in particular, and a producer in the field of computational mathematics and geodesy. As a geodesist, he modified some of the rules of projection Everest had produced and created tables (abstract intellectual artifacts) that facilitated such projections (Deb Roy, 1986; Kochhar, 1991). A testament to the state of his mathematical and computational mind is evident in Everest's appreciation, documented in the *Historical Records of the Survey of India*:

> Of the qualifications of Radhanath, I cannot speak too highly; in his mathematical attainments there are *few* in India—European or native—that can at all compete with him; and it is my impression that even in Europe these attainments would rank very high. As a [human] computer . . . there is no person in my department so thoroughly skillful in the application of the variants formula... (quoted in Subbarayappa, 2013, p. 404)

These "mathematical attainments" and procedural skills Everest refers to are, of course, precisely the constituents of schemas representing what cognitive scientists call declarative ('know-that') and procedural ('know-how') kinds of knowledge that were held in in Sikdar's cognitive space and that he must have deployed in his geodetic work.

However, contrary to the common lore propagated in popular accounts (through much of the twentieth century and into this), Sikdar did *not* compute the height of the mountain then called XV and later named (at Waugh's suggestion) Mount Everest; nor was he the first to assert this to be the highest mountain in the world. Indeed, he was apparently not even involved in computing the height of XV, a project executed in 1849–50. As historian of Indian astronomy Rajesh Kochhar (2021) would put it, "A perusal of published records [of the Great Trigonometrical Survey] clearly establishes that Sikdar had no role in the calculation of the heights of the Himalayan peaks." This was because, at this time, Sikdar was relocated in Kolkata,

while the heights of the Himalayan peaks were being determined from the Dehra Dun office.[8] In his survey of the history of the Great Trigonometrical Survey, Rama Deb Roy (1986), in fact, is careful to omit any mention of Sikdar being associated with the computation of any of the heights of the Himalayan peaks, including XV. Sikdar's acts of production, as a lead 'computer' in the Survey did *not* include these computations.[9]

10.2 Yesudas Ramchandra's algebraic paradigm

Sikdar, an applied mathematician, is a notable but rare case of the active influence of British 'Asiatic Society scientists' working in India on the making and shaping of Indians as modern scientists. In the realm of pure mathematics, born less than a decade after Sikdar, Yesudas Ramchandra (1821–1880), offers a remarkable contrast in the manifestation of creative phenomena.

In Ramchandra, we enter the domain of the *autodidact*. As in Sikdar's case, his mathematical ability was apparently recognized early but therein, the similarity ends. There was no Robert Tytlers to mentor him nor the support of George Everest in his workaday life. And though admitted to an 'English' school at age 10,

> "It does not appear that any particular attention was paid to mathematics in this school; but shortly before leaving it a taste for that science developed itself in Ramchandra who studied at home with such books as he could procure" (Ramchandra's autobiographical notes, paraphrased by Augustus De Morgan; quoted in O'Connor & Robertson, 2016).

Here, we find the entwinement of *interest*, *curiosity,* and the *need to know* (Dasgupta, 2019, pp 87–89). Curiosity and interest are both what philosophers of mind call 'intentional states'—that is, mental states that are *about* or *refer to* something in the world (Chalmers, 1996, p. 191). One is curious *about* mathematics; one is interested *in* mathematics. Interest and curiosity are thus components of one's cognitive space. More to the point, they are intimately entwined: a person pursues at length and thereby acquires deep knowledge about a subject because she is both curious about it and it deeply interests her. And all such persons who sometimes obsessively pursue a topic are driven by the need to know.

Ramchandra's act of consumption thus entailed a process of self-learning advanced mathematics driven by the need to know, which in turn was the outcome of an intentional state of interest/curiosity.

In 1844, he was appointed a teacher in mathematics at Delhi College (his alma mater). Thus began an important part of his working life, for it was during this time that his continuing act of consumption as a mathematical learner evolved into an act of production as a mathematical creator.[10] A particular problem would engage his attention. In his own words:

> For the last four or five years, I was desirous of solving all problems of Maxima and Minima by the principles of algebra, and not by those of Differential Calculus. (Ramchandra's preface to the 1850 edition of *Treatise*; reprinted by O'Connor & Robertson, 2016).

The sources of these "problems of maxima and minima"—there were about 130 such problems—were all texts written by British authors: seven named textbooks on differential calculus, one on geometry, and an article from the *Encyclopedia Britannica*, along with other unnamed works on "mixed" (that is, applied) mathematics (Ramchandra, 1850, Preface). The outcome of this act of production to which he devoted some "four or five years" was a textual artifact, *A Treatise on Problems of Maxima and Minima, solved by Algebra* (1850). This was a textbook but one that Ramchandra himself believed was at least in part original (in Boden's term, H-original):

> All the problems solved in the second chapter bring out cubic equations, the solutions of which on the conditions of Maxima or Minima required a new method which I could not find ... I then resolved to find out a method and in intervals of leisure during three years, I continually thought on the subject and at last found it ... *I call it new for I did not find it in any book I looked into* (Ramchandra, 1850, Preface; italics added).

The publication of the book (at Ramchandra's own expense, apparently) had a consequence of some significance. By a curious circumstance, copies of the textbook would find their way across the oceans to the distinguished British mathematician and logician Augustus De Morgan (1806–1871), who arranged for its publication in England in 1859. The publication included a comprehensive preface by De Morgan in which he tells his reader that:

[8] Kochhar has explored further Sikdar's non-involvement in the computation of the Himalayan heights.

[9] It is interesting to note that in his 450-page history of the Trigonometrical Survey of India, Matthew Edney (1997) dismisses Sikdar's as "a Calcutta Brahmin" who had been incorrectly credited with the computation of XV's height (p. 262).

[10] It should be noted that in his adult working life Ramchandra was as much a man of letters in Urdu as he was a mathematician. (See Raina & Habib, 1990; Minault, 2003).

> Those who consult Colebrook's translation of the *Vija Ganita* will see that I have not exaggerated the point most connected with this preface (De Morgan's preface in Ramchandra, 1859; reproduced by O'Connor & Robertson, 2016).

He was referring here to the treatise on algebra written by the medieval Indian mathematician Bhaskaracharya (1145–1185). De Morgan's point was that, in general, the ancient (or medieval) archetypal Hindu mathematician "was ... an algebraist as decidedly as the Greek was a geometer"; "The greatness of Hindu invention is in algebra; the greatness of Greek invention is in geometry". He was suggesting that Ramchandra's particular problem was to determine the maxima and minima of functions "under the condition that none but purely algebraic process shall be used". De Morgan noted that such an approach could not have originated from Ramchandra's "European guides", who were all committed to the use of differential calculus, but rather, he "found in the demands of his own spirit and impulse [that was] ... of a character more fitted to the state of his own community than the imported science of his teachers" (De Morgan's preface, quoted in O'Connor & Robertson, 2016).

For De Morgan, the problem Ramchandra investigated was indeed, virtually uniquely, "Ramchandra's problem"—for:

> I cannot find that it was even current as an exercise of ingenuity in Europe ... to find the value of a variable which will make an algebraic function a maximum or a minimum (De Morgan's preface).

In other words, Ramchandra *had invented a problem that had not been recognized in the European mathematics of his time or before.* As suggested elsewhere this is one of the ways in which a *problem of interest* is generated in some domain (Dasgupta, 2022, p. 230). This problem-finding is as much a manifestation of H-originality as a new solution to a previously identified problem.

If indeed De Morgan's contention was true—Dhruv Raina and S. Irfan Habib extend this thesis further, by stating that Ramchandra's *Treatise* in this sense must be "construed both culturally and politically" (Raina & Habib, 1990, p. 460)—then this is a case of Ramchandra's cognitive space containing a schema representing the 'algebra paradigm' linked perhaps to a representation of *Bījgaṇita* gleaned from his ecological space. Ramchandra's problem of interest—and thus his goal—to solve problems of maxima and minima using algebra would then entail the interaction of these representations in his internal cognitive with external ecological spaces. The British colonial scientific presence in India through the nineteenth century was irrelevant to both his acts of consumption and production, and to (what we might call) *algebraism* as the abstract artifact embodied in his *Treatise* as a creative phenomenon.

However, it must be noted that Ramchandra's preface to his 1850 edition of *Treatise* gives no hint whatsoever of the validity of De Morgen's contention. This still leaves the question of how or why Ramchandra had invented his particular problem.

Did Ramchandra leave an imprint in the annals of mathematics? Was the H-original part of his *Treatise* his algebraic approach—of any consequence ? Could it be judged to have been C-original? It does not appear so. However, we find a short essay bearing the title "De Morgan's Ramanujan" by Charles Musès—the reference being to the 'discovery' of Ramchandra by De Morgan as somewhat analogous to the 'discovery' of Ramanujan in the early twentieth century by the English mathematician G.H. Hardy—in which Musès mentions coming across a work by Ivan Niven titled *Maxima and Minima without Calculus* (1981), published by the Mathematical Association of America. He wonders why, though Niven addresses ideas and problems Ramchandra had addressed a century-and-a-quarter before, there was no reference to Ramchandra, in the text itself, nor in the bibliography nor in the index (Musès, 1998). At least in this one respect, Ramchandra's algebraism may be said to be A-original: it anticipated a much later work, though it itself would be ignored by the relevant mathematical culture.

10.3 Mahendra Lal Sircar's experimental temper

Mahendra Lal Sircar (1853–1904) was born just over a decade after Yesudas Ramchandra, and thus a much younger contemporary of both Sikdar and Ramchandra. Like them, he never left his native land, even temporarily. Like them, his science was imbibed entirely in India. Yet he affords a striking contrast to both of them, in his capacity as both consumer and producer of science.

Sircar's act of consumption seems to have lain mostly in formal education, first briefly in Hindu College, then more substantially as a medical student in Calcutta Medical College, founded in 1835 and India's first medical school along Western lines (Gorman, 1988). Its first professors included William Brooke O'Shaugnessy (1809–1899) as professor of chemistry; Nathaniel Wallich (1786–1854) as professor of botany; and Henry Goodeve (1807–1884), the College's first professor of anatomy. O'Shaugnessy and Wallich, incidentally, are amongst the prominent 'Asiatic Society scientists' listed earlier in this paper.

Sircar obtained a licentiate in medicine and surgery (LMS) in 1860, awarded by the University of Calcutta (established in 1857), and then, in 1863, the much superior degree of doctor of medicine (MD)—a very rare

achievement at the time for an Indian to attain (Palit, 1991). Initially a critic of homeopathy, Sircar underwent a *volte face* in 1867 and abandoned conventional medicine (allopathy) for homeopathy, possibly (as suggested by Chittabrata Palit, 1991)) on nationalistic grounds since homeopathy was more inexpensive and affordable for patients than allopathic treatment.

Yet it is not as a medical practitioner that Sircar merits a place in this discussion. Witness, rather, an article bearing the title "On the desirability of a national institution for the cultivation of the physical sciences by the natives of India" published in 1869 in the *Calcutta Journal of Medicine* (which he himself had founded the year before) in which he called for the establishment of an institution of science wherein lectures would be delivered along with illustrative experiments; an institution *in* India, managed *by* Indians, and intended *for* the enlightenment of Indians.

The ideology of nationalism was clearly represented in his personal cognitive space. This ideology must have triggered the desire—or goal—to establish a house of scientific *practice* in which, as he expounded in a speech delivered in 1875 to a gathering of potential financial donors:

> Men must continually be at a subject, observing and experimenting, before he can acquire that knowledge [of science] which will enable him to feel his own deficiencies . . . before indeed he can engage with any hope of success in researches which will improve both himself and his science (Quoted, Anon, 1976, p. 6).

In conceiving his house of science, Sircar had been clearly influenced by the Royal Institution of Great Britain in London, established in 1799 by Benjamin Thomson, Count von Rumford, where Sir Humphry Davy (1778–1829) and then Michael Faraday (1791–1867) (amongst others) would achieve their most celebrated achievements in the physical sciences. Rumford's original intention was "a Public Institution for diffusing the knowledge and facilitating the general introduction of useful mechanical inventions and improvements, and for teaching by [way of] courses of philosophical lectures and experiments the application of science to the common purposes of life" (quoted by Martin, 1961, p. 9). Sircar's ambition, however, lay not in introducing "useful mechanical inventions and improvements" but in presenting "philosophical lectures and experiments".

Realization of this institution—given the name 'Indian Association for the Cultivation of Science'—would take several years to reach fruition; it opened its doors in January 1876. John Bosco Lourdasamy (2003) would call it, wryly, a "tortuous tryst with modern science". But what interests us here is not so much the social history of the institution (IACS in abbreviation) but its embodiment of Mahendra Lal Sircar's *conception of science.* "Men", he tells his patrons, "must continually be at a subject"—we are reminded of Ramchandra's remark on being continually at his problem—"observing and experimenting".

But it is only when we delve into his diary entries, edited and presented by Arun Kumar Biswas (2000), that we obtain a clearer, deeper sense of what he meant by this.

Unlike Sikdar and Ramchandra, our two protagonists thus far, Sircar was never an original researcher. He did not contribute to the annals of scientific laws, theories, scientific facts, scientific procedures, or experiments. Yet his diaries reveal that he did not merely preach how science should be cultivated; he practiced what he preached. Preparing for his lectures and demonstrations in physics—mostly on electricity and magnetism, then topics in full flow in Europe—he is constantly at his subject "observing and experimenting", performing experiments, not to seek new phenomena but to repeat and confirm those of prominent Western scientists such as Benjamin Franklin and Michael Faraday. We witness his frustration when experiments fail; his satisfaction when they succeed; his disappointment when his lectures and demonstrations draw a scanty audience (and fees). We find him seeking funds to purchase expensive apparatus which, when they arrive from Europe, may engage an entire day in unpacking. Instruments sometimes fall apart and he consigns them to sundry people for repair. Some days, the weather interferes with the performance of his electrical apparatus. By way of his instruments we sense the presence in his institution not only of Western physics but of their creators: "Faraday's great apparatus for diamagnetism; Foucault's apparatus for the action of magnetism upon revolving copper" (Diary entry, Aug. 21, 1879, Biswas, 2000, p. 74); "Ruhmkorff's coil, Geissler's tubes" (Sept. 18, 1879, Biswas, 2000, p. 76). Much before IACS is inaugurated, he purchases an astronomical telescope from a British associate and observes Jupiter and its four moons—following, as countless others had done before him, Galileo's footsteps three centuries earlier—and Saturn and its ring; he observes and records the time of the transit of Venus across the Sun's surface (Diary entries, March 9, 10; Oct 29; Dec 9, 1874; Biswas, 2000, pp. 19, 25, 26, 27). Even in his homeopathic practice, we find him conducting experiments as when, in 1874, he records carefully the effect of snake venom on a rooster as an animal subject, the time it took for the latter to die, and the manner of its death (Diary entry, Nov. 15, 1874; Biswas, 2000, p. 26).

Sircar's great partner in physics was Father Eugene Lafont (1837–1808), a Belgian Jesuit priest and rector of St. Xavier's School and College in Kolkata and, as an experimentalist, a scientific soulmate of Sircar who was inextricably entwined with the latter in the deliverance of lectures and experimental demonstrations in physics at IACS (see Namboodiry, 1995; Biswas, 2001 for more on Lafont).

So what did Sircar create in the realm of science? Quite obviously, there was IACS. But this, as Biswas's monograph and Sircar's diary indicate, proceeded in fits and starts and in his own lifetime, failed to realize his goal—though it would be fulfilled quite spectacularly in the course of the twentieth century accompanied by a Nobel prize in physics for C.V. Raman (Anon, 1976). Funded in Sircar's time by private sources, one can certainly claim IACS as an institution of the *independent* science tradition Basalla posited as characterizing his phase 3.

But at a more abstract level Sircar's greater act of production was of an *experimental temper*—and here Eugene Lafont's contribution must also be acknowledged—an intellectual artifact, the likes of which, amongst Indians, must be deemed not only H-original but also C-original, of profound consequence for the development of modern science in India by Indians.

By 'experimental temper' I mean a cognitive attribute but with cultural consequences. It emanated from Sircar's cognitive space and would diffuse over the surrounding shared ecological space for anyone else who wished to assimilate it.

In Sircar's cognitive space (and in Lafont's for that matter), it would reside as a schema. An essential aspect of this schema would have been a representation of *operationalism*. I am using this term in the broad sense the chemist-philosopher Michael Polanyi (1962) had meant. In more specific terms, *operational principles* are rules, procedures, operations, or frames of reference that facilitate *action* for creating, manipulating, and modifying artifacts (Dasgupta, 2022, p. 195). Alternative terms for operational principle are 'procedural knowledge' and 'know-how'.

Thus, one part of the experimental temper schema would constitute representations of the operational principle concept.[11] At the very least, to execute an experiment performed in physics, chemistry, or biology, one must *know what to do*: one must possess operational knowledge about the relevant apparatus.

The second part of the experimental temper schema would be a representation of the concept of *problem-specific knowledge*—that is knowledge of the principles, theories, and facts, pertaining to a particular problem domain in a particular science.

Finally, a third component of the experimental temper schema will be a representation of the fact that there is a *linkage* between operational knowledge and problem-specific knowledge, for it is only if such a link exists can a particular operational principle be applied to a particular piece of problem-specific knowledge.

Sircar's act of consumption, by way of his formal education as a medical student and also, no doubt, self-education, would have entailed the acquisition (consciously or unconsciously) of the experimental temper and its representation in his cognitive space as a schema. His claim to originality in the context of the history of modern science in India is not in the realm of scientific knowledge but rather in *meta-science*—for that is what the experimental temper is. He manifested this attribute by way of his experiments demonstrating various aspects of electrical and magnetic phenomena. It was as much this experimental temper as scientific principles that he desired the students attending his and his associates' lectures to learn.

Was this experimental temper shaped by the presence of Sircar's British professors at Calcutta Medical College? No doubt, to some extent, his formal medical education was an influence, as one might expect. But there must have been a hefty dose of self-education in the physics literature (in which Lafont perhaps had a place)—a dose in which his British teachers would have had little influence.

10.4 Jagadis Chandra Bose's technoscience

Much has been written over the years (including by this writer) on Jagadis Chandra Bose (1858–1937), physicist, biophysicist, and plant physiologist. Yet the Basalla question prompts a further inquiry. Bose is my first protagonist in this paper, whose activity as a scientist extended well into the twentieth century. Nevertheless, in addressing Basalla's question, we will confine our attention to what elsewhere I have called the 'early Bose' (Dasgupta, 1999)—Bose the physicist—spanning his physical research between 1895 and 1900, since this coheres in time both with Basalla's concept of colonial science and with the activities of Sikdar, Ramchandra and Sircar.

Bose is also the first in this discussion whose formal education was substantially performed abroad. He studied science in St. Xavier's College, Kolkata, taught by Eugene Lafont, then, following a B.A. degree from the University of Calcutta in 1880, he proceeded to England. There, after aborting a plan to study medicine in London, he moved to Christ College, Cambridge. In 1884, he obtained a B.A. in the natural science tripos (chemistry, physics, botany), having been taught by, among others, Lord Rayleigh, the Cavendish professor of physics and director of the Cavendish Laboratory. Rayleigh would play a substantial role in Bose's career as an experimental physicist from when he began serious research in 1894, a few years after his return to India and appointment as professor of physics at Presidency College, Kolkata.

[11] See Dasgupta (2022), p. 194 *et seq* for examples of operational principles and their representations in the context of Renaissance engineering.

Although Bose was closely associated with both Lafont and Sircar—he taught briefly at IACS—he was very much by himself as a physicist. Between 1895 and 1900, he published some nineteen singly-authored papers related to the phenomenon of what was then called 'electric waves' (radio waves), and barring his very first paper on the topic, published in the *Journal* of the Asiatic Society in 1895, all his other papers appeared in English journals and periodicals, most notably in the *Proceedings of the Royal Society*. Bose was the first Indian scientist to publish in this most prestigious scientific journal. In 1897, he delivered a lecture in the celebrated series of the Friday Evening Discourse (established by Faraday) at the Royal Institution of Great Britain—the institution that had served as Sircar's model for IACS—and may thus be said to have 'arrived' in the Western scientific scene.

The work for which Bose is best known is the production of sub-centimeter length radio waves and the study of their quasi-optical properties. This was the work for which he would eventually be elected a fellow of the Royal Society (FRS) in 1920.

Bose was arguably H-original in all those aspects of his chosen specialty in physics (Dasgupta, 2018). Yet what especially catches our attention (and admiration) is his invention of the apparatus—the material artifacts—for generating and detecting small-length radio waves. Here, we are in the realm of *technoscience.*

This term appears to have been coined by anthropologist and scientist Bruno Latour to mean "all the elements tied to the scientific contents no matter how dirty, unexpected or foreign they seem" (Latour, 1987, p. 174). This is rather too open-ended for our purpose here; I will use 'technoscience' to refer to the creative phenomenon wherein science and technology serve as two mutually interacting disciplines (Sismondo, 2010, p. 81).

Bose's physical researches were imbued with the kind of experimental temper Sircar dreamed of for his students in IACS. But there was an important difference. Bose invented, designed, and built all the material artifacts he needed for his radio waves studies. He created the necessary technology (under the most primitive conditions) to do his science. It is in this sense that I speak here of *Bose's technoscience*—again, like Sircar's experimental temper, an abstract intellectual artifact that was a symbiosis that drew upon declarative knowledge (the theories and facts of radio waves—'know-that' knowledge) and operational principles (or procedural—'know-how—knowledge) to achieve his goals. His physics fed into the devices he built, and, in turn, his devices enabled him to do his physics.

His first radio wave transmitter improved upon Sir Oliver Lodge's transmitter of 1894 (which had been an improved on transmitter built by Heinrich Hertz, the scientist who demonstrated the physical reality of radio waves). Bose's transmitter was thus an H-original artifact. And he invented at least three radio wave detectors—then known as 'coherers'; of these the first (in 1895) was an improvement of Lodge's coherer; the second (1899), a mercury-based 'self-recovering' coherer (which automatically restored its own detecting powers in the course of receiving radio waves in succession) was jointly H-original—he shared this credit with the Italian physicist Thomas Tomassina. The third (*ca* 1900), galena (lead sulphide)-based detector, was also H-original (Dasgupta, 2018, pp. 63–64).

In 1897, Bose demonstrated a complete 'compact' apparatus for generating and detecting sub-centimeter radio waves at the annual meeting of the British Association for the Advancement of Science. There is evidence that in building this apparatus, he also anticipated some inventions by other later engineers (Emerson, 1997; Ramsay, 1958). In this respect, he may be deemed to have been A-original.

The galena detector served a somewhat different role than had his earlier coherers. In the spring of 1900 Bose wrote to his close friend Rabindranath Tagore that he had developed a novel 'artificial eye' which could "see" invisible as well as visible light (Bose, 1994, p. 15). This device which he also called "artificial retina" consisted of a hollow sphere containing a galena receiver with an opening in front fitted with a glass lens. This device, he would declare in a lecture delivered in May 1901 as his second Friday Evening Discourse at the Royal Institution, provided "a complete model of an artificial eye" (Bose, 1901). When connected to a circuit and a galvanometer, one could observe its response to both the visible spectrum and radiation beyond the infrared and ultraviolet ends of the spectrum. The galena artifact served both as a self-recovering coherer and a photoconductor, a device for which Bose was awarded a U.S. patent a few years later.[12]

I think we can assert quite unequivocally that the presence of the 'Asiatic Society scientists' in the last quarter of the nineteenth century had no place in the making of Bose as an H-original technoscientist. Bose was not only the first modern Indian physicist but India's first technoscientist, as I understand this latter term.

10.5 Prafulla Chandra Ray's intertextuality

It is, of course, inevitable that one mentions Prafulla Chandra Ray (1861–1944) in the same breath when speaking of Bose. They were close contemporaries, Ray three years younger; they became colleagues in Presidency College, Kolkata—Bose in the physics department, Ray in chemistry—and worked in close proximity for a quarter of a century; they both published their first research papers

[12] U.S. Patent 755,840, March 19, 1904.

in the *Journal of the Asiatic Society* but all subsequent papers well into the twentieth century in British journals; they both obtained undergraduate and doctoral degrees from British universities, Bose from Cambridge (B.A.) and London (D.Sc.), Ray from Edinburgh (B.Sc. and D.Sc.); they both were hindered in gaining academic appointments by British officialdom and prejudice; they both ushered in laboratory-based research into India, Bose in physics and Ray in chemistry.

Yet it is with respect to this modernity that Ray differed sharply from Bose. Indeed, in this respect, there was greater resonance between Ray and Mahendra Lal Sircar.

We have identified the experimental temper as Sircar's singular contribution to the beginnings of Western science amongst Indians in India. But, performing science does not end in the laboratory. It must be made public. Scientific knowledge is *public* knowledge. A piece of scientific research does not end until it has been communicated beyond the confines of that scientist's laboratory: it may become public by way of lectures at meetings of scientific societies—as was already common in Europe in the nineteenth century, e.g., at the Academy des Sciences in Paris, the Royal Society in London or the Manchester Literary & Philosophical Society, or organized conferences such as the annual meetings of the British Association for the Advancement of Science. However, verbal communication does not suffice. The fruits of research must be *published*, usually in scientific journals. They must eventually be represented as *texts*: originally as letters sent between scientists as common in the seventeenth and eighteenth centuries[13]; then, with the advent of the scientific journal, as research papers or letters to the editor, as was the common mode of communication in the journal *Nature*.

Thus, the young geologist and paleontologist Pramatha Nath Bose (1855–1934), another contemporary of Jagadis Bose and Prafulla Chandra Ray, having obtained a B.Sc. degree from London University and acquired fellowship of the Geological Society, and worked briefly at the Royal School of Mines, returned to India armed with the credit of a paper published in the *Quarterly Journal of the Geological Society* in 1880 (Bose, 1880). Thus, Jagadis Bose published his first paper on radio waves in the *Journal of the Asiatic Society* in 1895. Thus, Prafulla Chandra Ray published *his* first paper on the nitrites of mercury in that same journal.

There is another dimension to scientific publishing that is integral to the production of scientific knowledge, certainly from the nineteenth century when publishing in journals and periodicals became commonplace. Following literary theory's influence, I have referred to this as *intertextuality* elsewhere (Dasgupta, 2023). The point is that scientific papers are not islands of their own. They build upon and refer to other scientific papers that preceded them and to which they are *epistemically linked*. Once a scientific paper is published, it may become referenced in turn by other future papers. In other words, a scientific publication resides in an *intertextual space*. Within that space, a link will exist from text *A* to text *B* in the event that the production of *B* is (in some manner) epistemically connected to *A:* the knowledge content of *A* is an input to the production of the knowledge content of *B.* A reader's understanding of a paper, her eliciting its meaning, and what the writer's goals were in undertaking the work reported will depend not only on the contents of that paper but also on its epistemic linkages to other papers it refers to. (See also Latour, 1987 on this topic.) What we call 'modern' science *is integrally characterized by such intertextual spaces.* It is also by way of understanding the place of a publication in intertextual space that issues having to do with *priority* can be ascertained.

It is well known that Jagadis Bose was notoriously 'economical' in referencing other scientists' work. For all intents and purposes, his papers on radio waves *were* islands of their own. In Ray's work, in contrast, we observe the presence of the intertextual space that is such a crucial aspect of modern science. For instance, Ray's very first paper on mercurous nitrite was epistemically entwined with several other texts Ray references in his text, including contributions by chemists published in such journals as *Journal of the Chemical Society* and *Proceedings of the Royal Society* (England) and *Comptes Rendu* (France). Another characteristic of intertextual space is its chronological dimension. The 'space' is strictly speaking 'space–time': intertextual spaces evolve diachronically—they spread over historical time. This diachronic feature enables epistemic relationships and priority credit to be established.

It is interesting to compare, for instance, Ray's first paper "On Mercurous Nitrite" (Ray, 1896) with Bose's (1895) first paper "On polarisation of electrical rays by doubly refracting crystals", both published in the Asiatic Society's *Journal*.[14] Ray's text includes references to a chemist's work reported in a chemical encyclopedia and another in a chemical dictionary; to papers by five other chemists in the *Journal of the Chemical Society* (London); and other papers in *Comptes Rendu* and the *Proceedings of the Royal Society.* Bose's paper does not have a single reference to any other publication. There is no intertextual space in which his paper is situated. That Henrich Hertz had demonstrated the physical existence of radio waves, that Oliver Lodge

[13] See Koyré (1968) for a magnificent body of evidence of correspondences between Newton and his contemporaries in the seventeenth century.

[14] Bose republished this paper in *Electrician*: see the relevant bibliographic entry below.

had published an influential article in the pages of *Nature* in 1894—which had stimulated Bose to begin his life as a research physicist on the topic of radio waves—are conspicuously absent in Bose's text.

Ray's contribution to the chemistry of nitrites is well known (see, e.g., Chakravorty, 2014, Dasgupta, 2023). But here, we focus on a very different aspect of the ethos characterizing modern science, the intertextuality of scientific literature. In this Ray was, of course, by no means original; he was drawing upon the established practice of 'doing' science as he had learned during his long tenure as an undergraduate and then a doctoral student in Edinburgh. Ray's contribution in this realm was to *import* the practice of intertextuality into the beginnings of modern Indian science, and we have no reason to suppose that the 'Asiatic Society scientists' had any influence in this regard.

11 Discussion

My main aim in this paper was to explore the Basalla question from the viewpoint of cognitive history. Cognition constitutes the means by which the brain makes sense of one's experience, but it does so by interacting with culture. Indeed, as psychologist Jerome Bruner (1990) has pointed out, culture is constitutive of cognition. In the anthropologists' sense, we use 'culture' to mean a body of public, distributed, and relatively stable symbolic representations of things in the world (Sperber, 1996, p. 57). Cognition and culture are inextricably entwined, and so cognitive history straddles the external and the internal.

The cognitive-historical approach entails scrutinizing particulars rather than generalities: the individual scientist who consumes and/or produces knowledge, how consumed knowledge shapes the production of particular items of knowledge, the nature and forming of particular creative phenomena—events that make history in the annals of science. In our particular case in this paper, the producers and consumers of interest were denizens of British India, and the creative phenomena of interest were confined to ones evinced therein. We also narrowed the time frame to the second half of the nineteenth century because this was the period in which modern science entered India. To address the Basalla question as viewed from cognitive history, we limited our scrutiny to five main scientists, Sikdar, Ramchandra, Sircar, Bose, and Ray, all of whom lived and worked contemporaneously in the second half of the nineteenth century and, each of whom were instrumental in creating modern science by Indians. (In the case of Bose and Ray, who continued to be enormously active and productive through the twentieth century until their respective deaths, we limited their work to that performed until 1900.)

Each of these protagonists were examined with respect to a particular kind of problem-solving and/or knowledge-producing activity. Radhanath Sikdar and Yesudas Ramchandra solved problems *in* their respective disciplines and produced knowledge that added to their disciplines—one in geodesy and computational mathematics, the other in pure mathematics. Mahendra Lal Sircar's production was at a meta-scientific level: he strove to introduce the experimental temper into the Indian consciousness of modern science. Jagadis Chandra Bose made original contributions to the physics of radio waves, but our concern here was his productions in technoscience: the fact that he conceived, designed, and fabricated the apparatus he needed for his physics. Prafulla Chandra Ray also made original contributions to the chemistry of the nitrites of mercury but here, our focus was on another meta-scientific matter—the presence and role of intertextual spaces in scientific practice. *Collectively, they constitute different aspects of science as a problem-centric/knowledge-centric practice*.

As for Basalla's 3-phase model, our explorations here suggest that one can, in fact, dispense with it altogether since it played virtually no role in our examination of the introduction of modern science amongst Indians in nineteenth century India. To summarize:

One. Our (albeit brief) scrutiny of the 'Asiatic Society scientists' suggests that Basalla's idea of phase 1 science does not entirely hold: it is true that the most prominent British scientists in nineteenth century India were engaged almost entirely in the field sciences; that India was their domain of investigation; and that Indian flora, fauna and the physical terrain were their objects of interest. But many of them were residents of India and reported their findings in the Asiatic Society journals rather than take the data back as 'loot' to their home country. The knowledge generated by these endeavors, before finding its way to the West, did find a place in India itself.

Two. We also noted that, except in the case of Sikdar, the work of the British scientists and their fieldwork appear to have no influence on that of the Indian pioneers portrayed here. The work of the 'Asiatic Society scientists' and those of the Indian protagonists (again, other than Sikdar) seemed to have run on parallel tracks. We could find no evidence of Basalla's phase 1 as shaping our protagonists' contributions.

Three. As for Basalla's distinction between phase 2 'colonial scientists' and phase 3 'independent scientists', one is hard-pressed to make that distinction in the case of the Indian pioneers. At best, Sikdar may be deemed a sharply delineated (phase 2) 'colonial scientist' in the sense that he served in the Great Trigonometrical Survey. But was Ramchandra one or the other? Was Sircar? Were Bose and Ray? On the one hand, each seemed to satisfy one of the characteristics of Basalla's notion of a 'colonial' scientist in that they were either formally trained or self-educated within

the ethos of Western science; On the other, each was an avowedly individual, independent scientist who contributed in their different ways to the problem-centric/knowledge-centric practice of science *by Indians* in India—that is, as phase 3 workers.

Four. In fact, from the viewpoint of cognitive history whose concern is to elucidate the nature of creative phenomena, Basalla's 3-phase model is, at the very least, a distraction but more likely irrelevant. I have endeavored to show here that the question of how modern science is introduced into a colonial space that did not hitherto manifest it can be fruitfully investigated by exploring actual empirical instances of individual scientific pioneers and their respective particular acts of consumption and production of scientific phenomena. The cognitive-historical approach suggests a very different model for illuminating the genesis of modern science in India.

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