THE STRUCTURE OF SCIENTIFIC EXCHANGES IN THE AGE OF COLONIALISM

Patronage, Competition and Rivalry

Dhruv Raina *and* **S. Irfan Habib**
National Institute of Science Technology
And Development Studies, New Delhi (India)

Studies on the fine structure of scientific exchanges between the metropolises of modern science and the provinces, within the framework of the centre-periphery thesis, have committed themselves to three major themes:

- the transmission of scientific knowledge from the metropolis to the periphery,
- detailed studies of the growth of the sciences themselves, wherein the periphery served as a depopulated geographical space for the investigation of nature (1),
- the canonization of an exemplar: nineteenth century shifts in the so called centres of science: from Britain to France to Germany (2).

The present paper hopes to explore the nature of the centre-periphery interaction through the encounter between leading scientists at the centres of science and leading scientists from the periphery. The aim of this exploration is to assess the place of the normative values of science, if any, and the dynamics of their mediation. To begin with, we shall suggest through the examination of concrete exchanges between scientists at the periphery and the centre – Yesudas Ramchandra and Augustus De Morgan, P.C.Ray and Marcelin Berthelot, S.Ramanujan and G.H. Hardy, Meghnad Saha and Henry Norris Russel – that the normative values of the scientific community, within the context of centre-periphery interaction, are activated once entry has been granted to the scientists from the periphery into the invisible colleges of the centre. Until that occurs the centre-periphery interaction would have to be explored within the framework of the politics of knowledge. This is not to suggest that these distinct dynamics of interaction in the production of scientific knowledge work within watertight compartments, but that they work in complementary ways.

Distinct Levels of Centre-Periphery Interfacing.

What is being suggested is not a rejection of the previous frames for the study of centre-periphery interactions, but a modality for extending the study of centre-periphery

interactions to the micro-domain of scientific research programs, in an attempt to elucidate the nature of replication of these interactions within the specificity of a research program itself. It thus becomes imperative to recognize levels of centre-periphery interactions:

- LEVEL 1: of shifting centres and peripheries within Europe itself (3), and the subsequent structuring of scientific exchanges,
- LEVEL 2: of the centres within the Western world vis-a-vis the peripheries of science in the non-West,
- LEVEL 3: the microdynamics of this interaction as manifest in specific research programs.

The important point to note is that while structurally these levels appear similar, the dynamics operating at each level would be different. The theme of the present paper is the interplay between these levels.

The foregoing argument has been premised on two fundamental propositions current within contemporary political theory as well as within the social history of science. The first of these propositions is, in fact, a corollary of the centre-periphery thesis (and hence runs the risk of being tautological): the centre-periphery relationship produces a hierarchy in the production, distribution and organization of scientific knowledge. We shall assume the centre to have the following characteristics given by Gizycki, though not in the form rendered here:

- work produced at the centre commands more attention and acknowledgement than work produced elsewhere,
- the centre is a place from where influence radiates,
- centres are not all encompassing but discipline specific (4).

There are other aspects to the centre, but those we contend have to do with the dynamics of the evolution of the centre, and the above-mentioned are sufficient for the identification of one. There could as well be other definitions of the centre (5).

The second postulate essential to the configuration of these interactions is now accorded disciplinary status under the formation science and colonialism. To put it rather plainly, colonialism itself produces certain social forms and thus co-produces forms of organization that in turn configures the production of scientific knowledge. Without getting into the details of this formation, and acknowledging the hierarchies this produces, this is an essentially political field: and it is imperative to acquire interpretive latitude that is sensitive to the normative values of science and does not ignore its cognitive content (6).

This brings us to the second axis that would further our appreciation of the nature of the centre-periphery relationship. In specifying this axis that runs orthogonal to that ordained by the previous postulates, it is imperative to elaborate upon the nineteenth century perceptions of science current among the practitioners of science, both at the centre and the periphery. This axis could be defined in terms of an epistemological and an axiological principle. The epistemological principle would assert that scientific ideas and theories are contested in a pristine space, where the merit of ideas is judged in terms of the tenets of reason and evidence. In representational terms the evolution of

scientific knowledge could be visualized as a "theater of proof" (7). Secondly, the axiological principle, stated that the culture of science was an international one, and that this internationalism was an essential prerequisite for the development of scientific knowledge. Pasteur is a figure who canonically embodies the interplay between the national prestige residing in science in the nineteenth century, and the internationalism of science. Pasteurian internationalism of the 19th century could be summed up as follows:

- the scientist was a citizen of the "world", not of any one nation,
- the procedures and results of scientific research were valid irrespective of the nationality of those producing these results,
- while scientists were concerned about the contributions of their nations to the international stock of knowledge, this «did not infringe on the solidarity of the scientific community",

and finally, the *central tenet of the internationalism*, articulated as normative value:

- that whatever "the deficiencies in the actual conduct of scientists, they should behave in accordance with the universally valid, common substantive and procedural traditions of science" (8).

Earlier on in this essay we had specified when the internal norms of science were activated, but here a qualification is in order. While internationalism provided the backdrop for scientific activity at the centre and periphery alike, the sacred norms of science regulated the performance of scientific activity, but the entire play was staged in the theater of politics. Putting it another way, the epistemological was a stable referent for the interlocutors, but the rules determining the evolution of scientific research programs are to be uncovered by the historian of science.

The Microdynamics of this Interfacing

The frame for interpretation having been defined, a frame that acquires plasticity in being dialectical, we shall now look at specific exchanges as instances of our hypotheses concerning the relationship between centre and periphery. But before doing so, we should refer to the typology of scientists at the periphery, in this case India, and their role in culturally redefining science within the Indian environment. Between 1840 and 1920, we witness a gradual metamophorsis of the autodidact into a professional scientist. A very tentative chronology has been suggested as the phases in this metamorphosis:

- the first phase involves the autodidact, and the mathematician and popularizer of science Ramchandra may be considered its embodiment,
- the second phase is commanded by the presence of scientist-Renaissance men, cultural analogues of the late nineteenth century German Kulturäger: chemists like Ray, and Sircar belong to this genre,
- the third phase sees the appearance of the professional scientist, and for the sake of convenience 1914 is a landmark date in this transformation (9).

Science and Colonialism as a Frame for Centre-Periphery Interaction

This section consists of a discussion of the exchange between Ramchandra and De Morgan on the one hand, and Ramanujan and Hardy on the other within the framework of science and colonialism. This will be followed by a discussion on Ray and Berthelot within the framework of science and nationalism that may as well be looked upon as the other side of the debate on science and colonialism, wherein agency is granted to the colonized at a particular stage of historical consciousness. However, we shall not begin here with an overview of the inauguration of the project of modern science in India, since that has been dealt with extensively in the literature (10). Suffice it to indicate that Ramchandra was a man of letters, a leading Urdu journalist and one of the progenitors of popular science writing in Urdu (11). He spent the decade prior to the 1850s in attempting to introduce calculus to Indian students, and in the process confronted serious pedagogic problems, current as much in Europe as in India – a deliberation that in today's parlance is summed up under the rubric of ethnomathematics, a European term that could among a diversity of connotations refer to the acculturation of the modern mathematical project in non-European contexts.

Ramchandra, schooled in the algorithmic tradition of the schools of mathematics of India was equally at home with the mathematics pedantically inscribed in British school curricula. His *Treatise on the Problems of Maxima and Minima* could in the light of contemporary mathematical knowledge be seen as a non-topological attempt to introduce elementary calculus within a civilization where geometry as encountered in the works of Euclid was not at a premium (12). This pedagogical uneasiness was to find mathematical expression first in a work in Urdu for school children called the *Sari-ul Fahm* (13). The Treatise is written in English, and is in the tradition of the textbooks of nineteenth century mathematics. However, the Treatise contained a new method and as Augustus De Morgan, then Professor at University of London, was to note, Ramchandra could have published a paper or two in a research journal, but his purpose was primarily pedagogic. De Morgan was the first arrived mathematician to see a copy of Ramchandra' work – and till he received it, the Treatise was to be a subject of much criticism rather than discussion on its novelty.

Here we already begin to see the pattern of recognition of the scientist at the periphery: an exile at the periphery, astride two worlds, the butt of ridicule of his countrymen, an exotic specimen for colonial administrators (for the colonial subject is both unlike them and simultaneously a creation of the *mission civilisatrice*), can only be recognized by a practised eye. The comparison in the narrative structure of the legends of Ramchandra and Ramanujan are surprising, though their mathematics is certainly not. More importantly, the scientist at the periphery is ignorant of the rules of this community that is undergoing rapid professionalization at the centre; and with the passage of time has to be tutored into the rules and etiquette of the science. This is the rationale for De Morgan's elaborate preface to the English edition of the Treatise. How does one explain to an audience tutored in a particular manner of addressing mathematics that mathematics could be done another way? (14) Consequently, the "quaint" mathematics from the periphery has to be decoded through an arrived inter-

locutor from the centre, in order that it be drawn into the realm of relevance of contemporary scientific practice. But that would be only one side of the story.

De Morgan's own interest in the work of Ramchandra arose from the fact that he was closely associated with the formulation of curricula for mathematics teaching in Britain; which involved devising methods for instructing British school students in elementary notions of complex algebra and the new discipline of calculus. Ramchandra's teaching could, he felt, prove useful in the latter project in Britain. The book, De Morgan felt, could be introduced in British schools, though it was written for very different purposes, viz. that of instructing students in India brought up on the theory of equations as encountered in the Bija-Ganita of Bhaskaracharya, into a relatively new branch of mathematics.

The Ramanujan-Hardy relationship, on the other hand, investigated from a diversity of perspectives throws up its own questions. Shils (15) for one raises two issues that have been conjectured ever since Ramanujan scholarship acquired its enigmatic station within both the history and the sociology of science. The first asks whether Ramanujan could have continued to produce the seminal mathematics, if he was not tutored by Hardy and Littlewood into the idiom of the contemporary mathematics/number theory? Secondly, had he not gone to Cambridge, would he have founded a school of mathematics in India? And lastly, why did Ramanujan turn to Cambridge, and not to France or Germany? Given our contemporary proble of reconstructing the dynamics of centre-periphery relations, some of these questions have an air of premeditation about them, in that all paths lead to the centre, for the centre lingers around like the invisible hand, guiding the trajectory of the scientists at the periphery to the fount of wisdom.

The scientist/mathematician trained at the periphery is at a disadvantage as to his knowledge of current concerns in terms of research problems, the range of theoretical and empirical possibilities, and the language of consensus seeking that is being forged. In any case, by 1910, Cambridge was no longer the mathematical centre in Europe, the centre having moved from Britain to France to Germany. In any case, within the English speaking world, the only world Ramanujan had access to, Cambridge was the centre. Thus Ramanujan's move to this centre, on the wane, was to provide fresh blood from the periphery that could decelerate the waning of the centre. As Hardy was to write in his autobiography: "The real crises of my career came ten or twelve years later, in 1911, when I began my long collaboration with Littlewood, and in 1913, when I discovered Ramanujan. All my best work since then has been bound up with theirs, and it is obvious that my association with them was the decisive event of my life" (16). In more specific terms it was to give new impetus to, to align with and fortify, Hardy's mathematical research programme on the formalization of number theory. The centre-periphery relationship thus becomes a symbiotic one, from the point of view of the centre, parasitic from the point of view of the periphery. But more importantly, the wild untutored mind at no point posed a challenge to the authority of his peers, no matter how enlightened his peers may have been. In fact, the relationship between Ramanujan, Hardy, and Littlewood, in the high tide of colonialism stands out as an exemplar for those swearing by the axiological autonomy of science, that at the cognitive and epistemological level, as well as in terms of the values of science, science functioned independently of colonial

prejudice. In terms of the institutions of science, the normative values of science sustained the existence of the centre.

And finally, if there is one lesson to be drawn from the enigma of Ramanujan's mathematics itself, a lesson that Shils recognizes, but interprets differently, for he wishes to save science as a privileged way of knowing, is how does a mathematician at the periphery invent a mathematics that surpasses the insights of the mathematics at the centre. While we do not intend to venture a hypothesis concerning this, what must be recognized is, while the centre is a source from which influence radiates, a dynamic centre is one that picks up the voices that "surpass it". Which possibly also means that it is absolutely essential to deconstruct one of the premises of the definition of the centre, viz. the one that imputes to the centre the privilege of generating influence; but the pattern of emulation set up by the centre could, nevertheless, trigger off efforts at the periphery that surpass some of the efforts at the centre.

The centre's construction of the periphery is such that it sees the latter as a source merely of data gathering and survey related research and not charged with the task of theoretical synthesis, of proposing new theoretical configurations or even epistemic departures. This definition of the centre makes it difficult to explain the contribution of an S.N.Bose (later 1920s) at Calcutta to the emergence of quantum statistics, M.N.Saha's ionization formula that set the foundations of theoretical astro-physics (17); of a C.V.Raman (early 1930s) to the study of the phenomenon of scattering; of Hiedekei Yukawa (early 1930s) to the meson theory or a Tommonaga to quantum electrodynamics (late 1940s).

Science and Nationalism

With this we come to the third exchange, that between the founder of the school of synthetic organic chemistry in modern India, P.C.Ray, and the pope of synthetic organic chemistry in nineteenth century Europe, Marcelin Berthelot. There is nevertheless a major difference between Ramchandra, Ramanujan and Ray. The former two were mathematicians, a discipline that functions very differently from the experimental sciences. Secondly, Ray was himself tutored at one of the centres; and having obtained his doctoral degree at Edinburgh, under the supervision of Crum Brown, and familiar with the joint work of Crum Brown and Thomas Fraser at the conjunction of synthetic organic chemistry and pharmacology, returned to India to set up a research laboratory and a pharmaceutical industry. Ray basically set out to establish a centre at the periphery, but his exemplar was not the institute of his graduation, but the virtual centre of synthetic organic chemistry, Germany. In fact, Ray's teachers and peers in England had studied in Germany, for Germany (Göttingen in particular: Roscoe was a student of Bunsen at Heidelberg) (18) was the centre for England as well. As opposed to Ray's networking into the community of leading chemists of the time, both Ramchandra and Ramanujan were in the mode of the autodidacts. This implies that while the former two fell outside the space of competition, though their work could be reviewed/assessed in terms of the professed methodological tenets, and on those alone, Ray was functioning within the same space as his peers, and therefore vulnerable to the strain of contesting the claims of the science of the centre from the periphery. The point we wish to drive home is that there is in the centre-periphery exchange a sphere of patronage and a sphere of

contestation/ competition. The two cases we have discussed above, refract through two different angles the nature of patronage and the benefits accruing from patronage within a particular framework of exchange. We now address the question of contestation.

In discussing contest, we will still abide by the hypothesis that the rules the interlocutors must appeal to in the contest are those of reason and evidence on the one hand, and the benefit to science and humanity that is actualized from the professed internationalism of ideas. However, Ray's exchange with Berthelot is of an essentially different nature, for the field of interaction is not so much science, but of the history of science, which in turn implicated notions of development and progress that were then used as a measure of the advancement of nations.

In 1888, while Ray, on his return to India, was looking around for a research problem, he began examining Indian materia medica, and in addition chanced across histories of alchemy, in particular Berthelot's History of Greek Alchemy. The work of Berthelot inspired him into a study of the history of Hindu alchemy; and this was to become the source of a deep seated debate and correspondence between both of them. Ray refused to accept the Greek origins of Indian alchemy, though he would not preclude Chinese influences. But Berthelot was an iconic figure in the world of chemistry and was possibly also one of Ray's role models, particularly for a Ray bent on establishing a system of industrial chemistry in India, that in turn required an industrial research system. Nevertheless, Ray's scholarship on the matter of the origins of Hindu Chemistry was to run into Berthelot's authoritative figure (19).

The important feature to note is the tropology of argumentation characterizing the exchange. Berthelot urges Ray to work on the history of alchemy in India. Ray's researches indicate that the use of mercury compounds and the preparation of caustic compounds in Indian medical practice, predates Arab influence. Berthelot's work proceeds along the lines that the transmission of this knowledge from Greece to Egypt, and then via the Syrian Nestorians ends up in India and China. In reviews of Ray's book, Berthelot accuses his understudy of nationalist prejudice, whereas all other reviewers at the time, the world over commend the book for its lack of nationalist prejudice. What the encounter itself reveals is that within the transnational community of scientists at the time, the term nationalist was deployed as a pejorative, while contributions to science also pandered to the sense of nationalist pride. But then Ray was placing his work before the court of appeal. And Berthelot had therefore to concede to the former's argument without surrendering his authority. The point of contention is elbowed into the background: the point being of the origins of Indian alchemy, and Berthelot concludes his second review of Ray's book in 1903, in the *Journal des Savants* with the lines, of which we shall offer a rather bald translation: «an important chapter had been added to the history of sciences and the human spirit, a chapter that was particularly useful in its understanding of reciprocal intellectual relations that existed between the Oriental and Occidental civilizations" (20). The norms of internationalism, and the regulative epistemological standards provided structure to the nature of interaction once Ray was already networked into the community of practitioners at the centre.

Here is also seen the effort to perpetuate the existence of the centre as the centre. From the perspective of the advancement of knowledge, the completeness of Berthelot's

project required the incorporation of as many histories within his history of alchemy; a work that was particularly committed, in the light of Berthelot's own scientistic project, to illuminating the origins of chemistry, through its emancipation from the clutches of a discipline like alchemy. We shall not go into the historiographic divergence between their two approaches, but for Ray, Berthelot continued to be a role model; as at the periphery the exemplar is drawn from the centre.

Can the Politics of Knowledge be integrated into an Epistemological Framework?

To overcome the limitations of the centre-periphery model, that to a limited extent presupposes Basalla's colonial model, it would be necessary to speak in terms of scientists functioning within «a shared epistemological universe" (21), but where political contest is the subcutaneous norm of the unfolding of scientific knowledge and the social perspectives of science. This brings us to a central methodological problem of the field we are investigating. In conceding the "relative epistemological» autonomy within the micro-communities of science, we reckon with the fact, however problematic, of the relationship between power and knowledge. An unsettled issue, that the present essay poses but does not resolve, is the linkage between what Elzinga refers to as micro-level analysis with the macro- and institutional levels (22), for finally the practice of science (even at the micro-level) is to be situated within historical and political contexts.

As has been pointed out earlier, within either the framework of centre-periphery interaction, or the metropolis-province exchange, there is an emergent structure that needs to be reckoned with, a structure that certainly contains prefigurations of novelty. But does structure determine all? If it does then we return to the Orientalist construction of the "compliant native»; or the passive recipient medium. To overemphasize the actors creating structures not only rules out the historical contingency of the actors themselves but the place of institutions. A reasonable position (Cozzens and Gieryn refer to it as "the rational seat in the middle of the board") would be where actors are not the "dopes of structure nor the potentates of action" (23). In the current language of shaping, structure is both the medium and outcome of interaction.

What then were the terms on which scientists on the periphery were networked into the practice of modern science? While colonialism provided the frame within which the exchange of this knowledge occurred, the colonizers were themselves committed to a different order of scientific practice, through imperial institutions that brooked no place for the colonized. To find their place in the sunthe scientists from the periphery needed to have their scientific claims forwarded through authority figures in the world of science, who at times accorded credence to their sometimes quaint research programs – programs that appeared quaint, at least during the early phases. But once "entry" was allowed into the network the politics of knowledge acquired a different flavour, and "consensus" was officially obtained through epistemic negotiation (24).

In this essay we have discussed the case of two scientists who trained at the periphery and were later drawn to the centre under the patronage of scientists at the centre; and the case of the scientist at the periphery, who trained at the centre but did most of his science at the periphery, but was audacious enough to contest the authority of the claims of an important authority figure at the centre: in this case only scientific

controversies can be resolved, non-epistemic closure cannot be the case, since the scientist at the periphery sees the rules loaded against him, in which case he would force the debate on to the epistemic ground.

The latter is the case, when we take up the rather tragic biography of M.N. Saha. This would be a separate paper altogether, where the primary question would be what would happen if the scientist at the periphery competed for a position of authority in the metropolis of science? A little elaboration would be in line here, before we close. DeVorkin and Kenat in their remarkable study of the contributions of the astronomer Henry Norris Russel point out that two phases have been identified in the history of astronomical spectroscopy: the phase of qualitative chemical analysis and radial velocities; and the second phase dealing with quantitative chemical analysis and the structure of the solar atmosphere. The principal problems of concern in this transition from the qualitative to the quantitative era in the history of astronomical spectroscopy were:

- detection of series in line spectra and analyzing the atomic structure of elements,
- determination of chemical abundances in the solar and stellar atmosphere,
- analyzing the role of temperature and pressure in the stellar atmosphere in producing the differences observed in the spectra of stars (25).

The Hertzsprung-Russel diagram and the technique of "spectroscopic parallaxes" were to aid in interpreting stellar spectra in order to decipher the relationship between the stellar temperatures and luminosities. Saha himself came to the problem through the courses on thermodynamics and spectroscopy he taught at Calcutta University, while he "devoured all available issues of European and British scientific journals" (26). The principal scientific influences on his work were those of Bohr, Nernst, Sommerfeld and Eddington. His ionization formula revealed for the first time the interrelationship of the total pressure of a gas, the degree of ionization of an element in the gas, the ionization energy of the gas, and the temperature of the gas. His ionization theory was worked out through a series of papers written in the early 1920s, and was to provide a rational theory of spectra that was to reorient the efforts of two schools of astronomical spectroscopy, one at Cambridge, and the other of Russel at Harvard in the United States. Russel anticipated the applications of Saha's theory and as "a result of ionization theory, he altered significantly his own research agenda to exploit it" (27). Saha's "relative freedom in isolation" enabled him to explore various applications of "modern physics and chemistry", but "prevented him from fully exploiting his own discovery". So while the scientist at the periphery is not drawn into the circle of fashionable problems current at the centre, this very condition disables the stabilization of the possible network (s)he could establish at the periphery for the subsequent replication of her/his research program. The program was to be developed through the incomparable spectral resources available at Harvard, as well as through the industry of Russel, Milne, Fowler, Menzel and Payne. This inability to stabilize the network at the periphery throws up an interesting problem for investigation, for even though the scientist at the periphery is unrestrained by peers to explore a different epistemological regime, (s)he is at a disadvantage in capitalizing the range of possibilities that are nevertheless outcomes of this epistemological or programmatic departure.

While it may be argued that the freedom available at the periphery renders the possibility of idea-hybridisation greater, since the pressure to conform to exisiting intitutions and disciplines is lower (28) it is attractive when discussing the centre-periphery relationship at level 1. But the subject of discussion here is level 2, where the marginality of scientitific infrastructure in the early decades of the twentieth century restricts the possibility of fashioning a new community at the periphery. Thus the possibilities opened up by the distances achieved from the pressures of conformity are compensated by severe limitations in scientific infrasructure, institutions, and the absence of normative legitimacy. Programs germinating at level 2 acquire their "scientific" potential at the centre itself.

Further, an underlying assumption of the study is that it makes inferences about the dynamics of scientific exchanges based on case studies of four leading Indian scientists. The point to be reckoned with nevertheless is that the concrete exchanges of these four scientists is being situated against the backdrop of the emerging scientific community at the turn of the nineteenth century on the one hand, and the location of these scientists as colonial subjects on the other. This sets the frame within which the scientist from pre-independent India was drawn into the global community of scientists. The advantage gained in examining the interplay between the political frame and the normative frame is that it becomes possible to overcome the overdetermination of scientific practice by politics on the one hand, and to avoid the broadbrushing of the character of scientific exchanges by overlooking the relationship between knowledge and power.

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NOTES

1) Lewis Pyenson, *Civilizing Mission: Exact Sciences and French Overseas Expansion, 1830-1940*, Baltimore and London: Johns Hopkins University Press, 1993.

2) S. Nakayama, "The Shifting Centres of Science", in *Interdisciplinary Science Reviews*, Vol. 16, No. I, 1991; Edward Shils, "Reflections on Tradition, Centre and Periphery and the Universal Validity of Science: The Significance of the Life of S. Ramanujan", *Minerva*, XXIX, 4, 1991, pp. 391-419; R Von Gizycki, "Centre and Periphery in the International Scientific Community: Germany, France and Great Britain in the Nineteenth Century", *Minerva*, 1973, pp.474-494.

3) Joseph Ben-David, *The Scientist's Role in Society: A Comparative Study*, Chicago, 1984.

4) Gizycki, *op.cit.*, p. 474. Most studies coming out of India on the centre-periphery interaction have been grossly negligent of the last postulate.

5) For Roy MacLeod metropolitan science does not mean just the science of Edinburgh or London, or Paris or Berlin, but a way of doing science, based on learned societies, a small group of cultivators, certain conventions of discourse and certain theoretical priorities set in eighteenth century Western Europe. Metropolitan science was science. For further details see Roy MacLeod, "On Visiting a 'Moving Metropolis': Reflections on the Architecture of Imperial Science", in *Historical Records of Australian Science*, Vol. S, No. 3, 1982.

6) Xavier Polanco, "Science in the Developing Countries: An Epistemological Approach on the Theory of Science in Context", *Quipu*, Vol. 2, No. 2, May-August, 1985, pp. 303-318. Polanco, however, sees the absence of an epistemological approach to the history of science in the third world to be a consequence of the marginality of science in third world countries.

7) Arnold Thackray, "Natural Knowledge in Cultural Context: The Manchester Model", *American Historical Review*, Vol. 79, 1979; Ian Inkster, "Science and the Mechanics Institutes,1820-1850: The Case of Sheffield", *Annals of Science*, Vol. 32, n° 5, September,1975; and Claire Salomon Bayet, "The First Bio-Medical Revolution: The Slow Shaping of Microbiology, France", *Xth International Symposium of History of Medicine, East-West*, Tanguichi Foundation, Tokyo,1985.

8) Gizycki, *op. cit.*, p. 478.

9) Dhruv Raina, "Recreating Modern Chemistry in India: At the Intersection of Historical Consciousness and Problem Choice", paper presented at Conference of the European Association for Studies on Science and Technology on Science, *Technology and Change: New Theories, Realities and Institutions*, Budapest, August 28-31, 1994 p. 3-5.

10) D.M. Bose, S.N. Sen and B.V. Subbarayappa, *A Concise History of Science in India*, New Delhi; S. Irfan Habib and Dhruv Raina, "Copernicus, Columbus, Colonialism and the Role of Science in Nineteenth Century India", *Social Scientist*, 190-91, March, 1989.

11) S. Irfan Habib and Dhruv Raina, "The Introduction of Scientific Rationality into India: A Study of Master Ramchandra-Urdu Journalist and Mathematician and Educationalist", *Annals of Science*, Vol. 46, No. 6, November 1989.

12) We shall not embark here on the debate whether geometrical notions are extant within the Indian tradition, but go along with the prevalent but dominant mathematical styles characterizing the Indian tradition.

13) Dhruv Raina and S Irfan Habib, "Ramchandra's Treatise through 'The haze of the Golden Sunset': An Aborted Pedagogy", *Social Studies of Science*, Vol. 20, 1990, pp. 455-72; Dhruv Raina, "Mathematical Foundations of a Cultural Project or Ramchandra's Treatise 'Through the Unsentimentalised Light of Mathematics'", *Historia Mathematica*, 19, 1992, pp. 371-384.

14) Luke Hodgkin, "Mathematics as Ideology and Politics", in Lev Levidow (ed), *Radical Science Essays*, London, 1986, pp. 173-97.

15) Edward Shils 1991, *op. cit.*

16) G.H.Hardy, *A Mathematician's Apology*, Cambridge University Press, 1967, p. 148.

17) David H.DeVorkin, "Henry Norris Russel", *Scientific American*, May, 1989, pp. 93-99. Saha, following his intitial papers wrote to Hale pleading for support to continue his work. Hale, Russel et al. were pursuing their work on spectra at Mouny Wilson. While Hale wrote to Saha stating that they were following his agenda, DeVorkin writes, "Saha was not invited by his European and American colleagues to collaborate with them in refining and extending the theory". DeVorkin, *op. cit.*, p. 98.

18) PC Ray, *Life and Experiences of a Bengali Chemist*, Chukkerverty, Chatterjee and Co. Calcutta, 1932.

19) For a detailed discussion on this issue see Dhruv Raina, "Recreating Modern Chemistry in India", 1994

20) Dhruv Raina, *op. cit.* 1994, p. 21.

21) Sverker Sorlin, "The International Contexts of Swedish Science: A Network Approach to the Internationalization of Science", *Science Studies*, 2, 1992.

22) A. Elzinga, "Science as Continuation of Politics by Other Means", in Thomas Brante, Steve Fuller, Wilhem Lynch (Eds.), *Controversial science: from Context and Contention*, State University of New York Press, 1993, p. 128.

23) Rob Hagendijk, "Structuration Theory, Constructivism and Scientific Change", in Susan E. Cozzens, Thomas F. Gieryn, (Eds.) *Putting Science Back in Society*, Indiana University Press, 1990, pp. 1-14.

24) Ernan McMullin, "Scientific Controversy and its Termination", in H.Tristane Engelhardt Jr, Arthur L. Caplan (eds), *Scientific Controversies*, 1987.

25) David H. DeVorkin and Ralph Kenat, "Quantum Physics and the Stars (I): The Establishment of a Stellar Temperature Scale", *Journal for the History of Astronomy*, xiv, 1983, p. 103.

26) *Ibid.*, p. 111.

27) *Ibid.*, p. 126.

28) Michael Chayut, "The Hybridisation of Scientific Roles and Ideas in the Context of Centres and Peripheries", *Minerva*, XXXII, 3, 1994, 297-308, p. 298.

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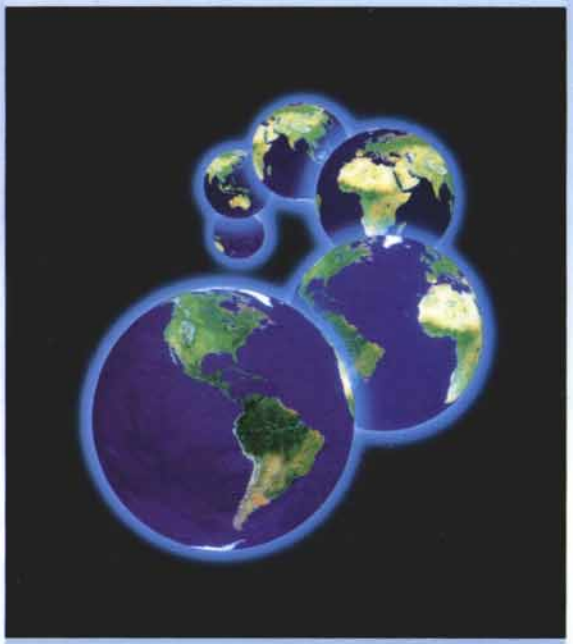
LES SCIENCES HORS D'OCCIDENT
AU XX^{E} SIÈCLE

SÉRIE SOUS LA DIRECTION
DE ROLAND WAAST



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