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# Science in motion: what postcolonial science studies can offer[1]

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## Abstract
In the last 30 years, Science and Technology Studies (STS) have deconstructed the foundation of diffusion models of science, by showing that science and society are inextricably linked. Nevertheless, STS has rarely ventured into cross-cultural trans-national analyses of techno-scientific research. In recent times postcolonial science studies has attempted to shift the STS to trans-national domain, particularly in relation to impact of colonialism. This paper is in line with these efforts at analyzing techno-scientific research. I have analyzed Magnetic Resonance Imaging (MRI) research and development in the United States, India, and the United Kingdom in order to put into broad relief the hierarchical global topography of techno-science. I have shown how colonialism continues to undergird analyses of techno-scientific research. I have argued that in the first instance we need a 'decolonization of imagination' if we need to move beyond dualist categories of west/non-west, developed/developing, north/south, and so on, which are parasitic to some conception of 'lack' of the non-west.

## Keywords
science studies; postcolonial; magnetic resonance imaging; India; United States; United Kingdom

One of the greatest contributions of Science and Technology Studies (STS), in the last 30 years, has been to show that the contours of science/technology and society are inextricably interwoven (Bloor [1976] 1991, Latour & Woolgar [1979] 1986; Knorr Cetina 1981; Shapin & Schaffer 1985; Latour 1987; Haraway 1991). These studies have highlighted that techno-scientific developments are path-dependent; that is embedded within particular historical and socio-economic and technical enfoldings. They have consistently shown that the concrete environment in which techno-scientific research is conducted gets embodied in the design and working of technological systems. Nevertheless, until recently, STS, with the exception of Sharon Traweek's (1988) study, comparing the culture of high-energy physicists in Japan and the US, and Karin Knorr Cetina's (1999) comparative study of the European Laboratory for Particle Physics in Switzerland and a molecular biology laboratory in Germany, rarely ventured into cross-cultural or trans-national analysis of technoscientific practices.[2] To a significant extent this has been a result of micro-level focus of STS. However, it often has had an inadvertent consequence – when some STS scholars extended their work to trans-national or global context their work ended up exemplifying 'first in Europe and then elsewhere' Eurocentric temporality (Prakash 1999; Prasad 2006a).[3]

It would be too simplistic and also unfair to characterize these scholars or their work as inherently Eurocentric. Nevertheless, we need to explore whether (and along with it also why) STS needs a retooling of its analytics when it shifts its focus to trans-national or global arena. Such an investigation has to be done not with the aim of developing a different set of analytical tools for the study of technoscience in the trans-national or global domains, but rather for the purpose of self-reflexively interrogating and retooling the STS analytics that we have been using for studies of laboratory practices. That is, as Warwick Anderson argues, 'metropole and post-colony…[have to be] examined in the same 'analytic frame' (Anderson 2002: 643).

In this article I analyze Magnetic Resonance Imaging (MRI) research and development in the US, India, and the UK. My analysis is located within the recent STS efforts in explicating different aspects of postcolonial science studies that started with Sandra Harding's call for an integration of postcolonial, feminist, and laboratory studies of science and has resulted in a vigorous debate as is evident in the three special issues on postcolonial science studies in prominent journals (Anderson 1992; McNeil 2005; Abraham 2006). To clarify at the outset, I would not offer postcolonial science studies as an overarching framework for all analyses, but rather, to use a Donna Haraway's phrase, as another 'partial perspective' that has to be an intrinsic component of our mosaic 'vision' about technoscience.

I would argue that postcolonial science studies can offer useful analytical tools not only to move beyond west/non-west, developed/developing, or north/south techno-cultural divides; but also more broadly to put into broad relief the uneven terrains of technoscience networks and flows. Postcolonial science studies can highlight unevenness within networks and flows of knowledge, artifacts, and people by showing how even when scientific practice is contingent and emergent it can continue to be embedded within and operate through hierarchies of power which draw upon dualist colonial constructions of the 'self' and the 'other'. More broadly, it can provide us with a better picture of *science in motion*.[4] I use the phrase science in motion to signify not just movement across disciplines or geographical/spatial motion, but also to highlight 'motion' of science across material, discursive, and ideological domains as well as its motion across temporalities/histories. Motion signifies 'travel' across (and also within) these different domains as well as the roles of these domains as co-constitutive factors in impacting motion.

Science in motion is not a call for a radical departure from the focus of STS on science as practice (Pickering 1992), best exemplified in Bruno Latour's phrase 'science in action' (Latour 1987). In significant ways it can be argued that action can subsume motion and vice versa. Moreover, STS has emphasized science in motion: Geoffrey Bowker's book *Science on the Run* (Bowker 1994) and Latour's *Pasteurization of France* (Latour 1988) are two among many such examples. The work on 'boundary objects' (Star & Greismmer 1989; Bowker & Star 1999); 'standardized packages' (Fujimura 1992; 1996), or 'trading zones' (Galison 1996) are again just a few other examples within STS that have highlighted motion and translation of science across disciplines and institutions. Further, and perhaps most significantly, feminist studies of science have made hierarchical implications of science in motion the prime target of their investigation (Harding 1986; Haraway 1991; Clarke 1990; 1998).

However, we cannot deny that STS, in particular Actor Network Theory and its variants, with their focus on science in action has largely concentrated its energies to analyze/understand technoscience by considering laboratories as , to use another of Latour's phrases, 'obligatory passage points'. There seems to be an underlying and enduring belief that if we open up the 'black box' of scientific practices in the laboratory, the insights that are gained in the process can be directly translated to analysis of science in other domains and more broadly in the context of society and politics. A particular reason why some of the STS analyses have ended up exemplifying Eurocentrism when they were extended to trans-national scale is, as I will show in this article, because of such a 'laboratory' focused analysis of technoscience. Let me clarify, I am not calling for an abandoning of what I have called science in action approach. I see my own study as an extension of it. My argument is that if we wish to shift scales and analyze science in motion at different levels and domains we will be better served if we also shift to considering laboratory not as an obligatory but perhaps a 'strategic passage point'. A particular STS work may for example just use the insights gained through analyses of laboratory practices as a backdrop for say a discursive analysis of ideological construction of a developmental project or policy.

Science in motion is therefore not a call to throw away what we have gained in the last thirty years but rather a call for redistribution of emphasis and retooling of analytics. I am also not suggesting that STS is continuing to ignore analyses of science in motion across nations and societies. The joint conference of the Society for Social Studies of Science (4S) and the European Association for the Study of Science and Technology (EASST) at Rotterdam this year had several panels and workshops explicitly dedicated to travel of STS to the so-called global 'south'. It was however striking how often presenters and interjectors argued for the need of STS to travel to such regions without acknowledging that this may perhaps require a change in STS analytics. It seemed as though STSers, while consistently analyzing the problematic of how science travels as 'immutable mobiles', have started to believe that STS could (or should) do the same. My call is therefore to take stock of what we have been doing and change our 'thinking cap' at least somewhat. Otherwise we run the risk of making the same mistake that liberal feminists did in the early twentieth century: Their analysis/critique of the condition of women in the colonies often made them complicit with the colonial project (Nair 1992; Burton 1994).

## MRI Research in India: Reconfiguring the 'Periphery'

> [Gustave] Le Bon recognized the high level of craftsmanship that Indian artisans had attained in numerous fields, but he agreed with those who argued that such technology and scientific understanding as the Indians possessed had been borrowed long before from the Arabs and especially the Greeks...He concluded that an inherent incapacity for scientific inquiry and original invention had stranded them [Indians] at a far lower level of social development than that of the western Europeans,
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> Michael Adas, *Machine as a Measure of Man*, 1989:176-77.

> The relatively small geographical area covered by these nations [of western Europe] was the scene of the Scientific Revolution which firmly established the philosophical viewpoint, experimental activity, and social institutions we now identify as modern science.
>
> George Basalla, *Science*, 1967: 611.

In 1987, when the first imported MRI scanner was being installed at the Institute of Nuclear Medicine and Allied Sciences (INMAS) in Delhi, India, the US had nearly 900 MRI scanners deployed for clinical use (Rublee 1989). By this time, Raymond Damadian and Paul Lauterbur, two American scientists, were already in the midst of a bitter priority dispute over the invention of MRI and eventually in 2003 Lauterbur and Peter Mansfield, a British scientist, received the Nobel Prize for their contribution to the development of MRI. The MRI scanner installed at INMAS was manufactured by Siemens, a multinational company based in Germany. N. Lakshmipathy, the then director of INMAS, informed me that they not only had the scanner installed by Siemens' engineers but also made Siemens take care of the masonry required to house the scanner. Lakshmipathy also told me that he had come 'to know about this new imaging modality, which was not called MRI at that time, when a British scientist [had] showed the *in vivo* images of human anatomy produced by it during a talk at INMAS.' By 2001-02, the number of MRI machines installed in India increased to more than 200 – all of these have been manufactured by multinational companies based in Europe or the US, namely GE Medical Systems, Siemens, and Phillips (recently Toshiba has also become a supplier of MRI in India).[5] Almost all these machines are being used for clinical diagnosis rather than research. There has been little contribution of laboratories in India in the development of MRI.

In contrast, several groups of scientists in the UK and the US were involved in the development of MRI from the 1970s. In the UK, two groups of scientists (later three) at the University of Nottingham, one under Peter Mansfield and the other under Raymond Andrew, and another group at the University of Aberdeen under John Mallard, contributed significantly to the development of MRI (Mansfield & Morris 1982; Blume 1992; Christie & Tansey 1996; Mallard 2003). Moreover, Electrical and Musical Industry (EMI), a British company, was the first in the industry to engage in the development of MRI. In the US, apart from Lauterbur and Damadian, scientists at the University of California-San Francisco (UCSF) were important contributors in the development of MRI. Moreover, by the mid-1980s, General Electric (GE) Medical Systems based in the US was already a global frontrunner in the manufacture and supply of MRI scanners.

The trans-national history of MRI seems to be an ideal typical case in which technology is developed in Europe and the US and then deployed in India. If we slip into using categories of west/non-west or developed/developing societies, the case of MRI seems to reflect a techno-cultural divide between the west and the non-west or between developed and developing societies. The case of MRI research and development is not unique, in fact similar stories have been told about many other technologies. The story of MRI in India may also seem to be a classic exemplification of diffusion model of science. Simply put, these models postulate that modern science and technology have been and continue to be developed in the west and then they are deployed in non-western societies (Basalla 1967; Schott 1993). In spite of their differences, diffusion models of science and technology share a common problematic because they are undergirded by a particular understanding of history of science and technology – modern science, according to them, emerged in Europe at the time of scientific revolution and then it was and continues to be disseminated to the rest of the world (see for example the quote from George Basalla at the beginning of this section). Science in the diffusion model is not merely a particular discipline or practice, but a reflection of society's 'development' and also a way of being in the world.

The modernization and development theorists, for example, very often deployed this particular historiography to define stages of development of societies. Hence W. W. Rostow argued:

> "The pre-conditions of take-off [for transformation from a traditional to a modern society] were initially developed, in clearly marked way, in Western Europe in the late seventeenth and early eighteenth centuries as the insights of modern science began to be translated into new production functions..." (Rostow 1960: 6).

History of science therefore has never been just a history of science and history of a particular technology has rarely been told as history of just another technology. The moment the gaze is shifted to the non-west each particular instance becomes an exemplification of a broader west versus non-west divide. One simply has to read the newspapers even now to find how pervasive such a belief is.

One can argue following some of the proponents of alternative science and technologies that societies such as India do not need modern western technologies such as MRI (Alvares 1980; Reddy 1988; Sardar 1988; Upawansa 1988).[6] This can be a legitimate argument if its possibilities are not circumscribed by Eurocentric constructions of modern science and shown as applicable only in the context of the non-west, which is often the case as I have shown elsewhere (Prasad 2006b). Yet this argument cannot account for why scientists in India could not contribute much to the development of MRI or other technologies. Such a situation has prompted several social scientists and historians to analyze the reasons for 'backwardness' of Indian science. According to Susantha Goonatilake, scientific research in India (South Asia) constitutes 'only minor variations of the major view points' that are developed in the west (Goonatilake, 1984). Goonatilake argues that lack of creativity among Indian/South Asian scientists exists because there is a lag in diffusion of knowledge from the west to the non-west (ibid. 1984).

Economists and Scientometricians usually deploy statistics such as number of patents obtained, citation indices of journal articles, or even numbers of graduates in the sciences to show whether a particular nation/society has developed scientific culture or has innovative capacity. In a recent article in the *Public Understanding of Science* titled, 'What is scientific and technological culture and how is it measured' (Godin & Gingras 2000) the authors use percentage of university students enrolled in the sciences as an index of spread of scientific culture. Until very recently, this particular statistic presented a paradox for analysts of scientific research in India – there were a large number of Indians enrolled and graduating in science but the contribution of Indian science did not seem to be much affected by the presence of so many science graduates. Vandana Shiva and Jayant Bandopadhyay explained this paradox thus:

> [T]he existence of a scientific community which shares scientific criteria and values becomes essential for self-sustaining scientific activity in a particular society. While the *scientific profession in India is the third largest in the world, it does not constitute a scientific community sharing scientific values and commitments.* (Shiva & Bandyopadhyay, 1980: 593, emphasis added)

In a sense then Eurocentric construction of the episteme and values of modern science and the history of its development somehow keep becoming the 'holding studs' to explain the reasons for lack of contribution of Indian scientists in the development of science and technology.

Recent scholarship in history and sociology of science has questioned the proposed one way relationship between the center (west) and the periphery (non-west) by showing how science in the 'periphery', at some levels, had developed autonomously and how the periphery contributed in the making of the 'center' (Elzinga 1980; Chambers 1987; Macleod 1987; Krishna 1992; Raina 1996). Moreover, it has been shown, that science has been received and adapted differently in different societies (Habib & Raina 1989; Palladino & Worboys 1993). Hence it has been argued that instead of a center-periphery model we should for example see the development and diffusion of modern science as a 'moving metropolis' that is a function of the empire (Macleod 1987). Kapil Raj takes the thesis of exchange, as opposed to one-way flow of science, further by showing that significant amount of science emerged in the 'intercultural contact zones' of the colonial west and the colonized non-west such as South-Asia, hence the process should be seen as akin to 'circulation of science' (Raj 2006).

It is definitely true that MRI has been received and adapted differently in different nations. For example, in the US the concern with the term nuclear led to renaming of Nuclear Magnetic Resonance Scanner, as it was previously called, to MRI (Meaney 1984). Similar concern has been lesser in the UK and non-existent in India. INMAS as well as some other institutes such as All India Institute of Medical Sciences (AIIMS) continue to use NMR instead of MRI in the names of their research centers dedicated to MRI. The reason, as I was told by the scientists, is that they feel MRI is useful as a part of a 'panel of nuclear medicine' rather than just as a medical imaging technology. Moreover, scientists at INMAS have utilized MRI for particular diseases that are common in India such as thyroid. Similarly, Sanjay Gandhi Post Graduate Institute (SGPGI) in Lucknow has conducted several studies on viral diseases because these are more common ailments that they confront in their patients. Scientists at the AIIMS have also conducted an interesting study to investigate the impact of Gayatri Mantra, a Rig Vedic hymn, on the brain using MRI (Jayasundar & Rajsekhar 2000). Efforts have also been made to develop an indigenous MRI in collaboration with different scientific research institutions in India in the 1990s, but the project was later disbanded. I was informed by the Director of the Central Sophisticated Instrument Organization, Chandigarh (the nodal agency for the indigenous MRI project) that the reason they disbanded the project was because they were unsure of indigenously developing high strength homogeneous magnets that are required for MRI. When I told him that they could have easily imported the magnet as was done by almost all the scientists, including Lauterbur and Mansfield, who eventually received the Nobel Prize for their contribution, I was told they could not because they did not have funds for that.

There has also been no lag in diffusion of knowledge of MRI in India. Paul Lauterbur, for example, first presented his ideas about nuclear magnetic resonance imaging outside the US, in India, way back in 1974. Moreover, what we also have to bear in mind is that Nuclear Magnetic Resonance (NMR) research from which MRI emerged was thriving in India around the same time it started in the US and some European countries. G. Suryan's studies on 'radiation damping' or study of flow of liquids using NMR were well-cited and utilized in the US and elsewhere (Suryan 1949; Suryan 1952; Prasad 2006a). Suryan's research was influenced as much by I. I. Rabi's work in the US as by C. V. Raman's research and guidance in India. That is to say, there has been a 'circulation of science'. Nevertheless, the question remains – why has there been so little contribution of scientists in India in the development of MRI technology? Lag and/or lack (of knowledge, expertise, resources, culture, etc.) have commonly been used to explain this discrepancy. As I mentioned earlier, there was no lag in diffusion of knowledge (I have discussed this in more detail elsewhere, see Prasad 2005; Prasad 2006a). It may seem that lack of resources could perhaps be the reason for the existence of the technology development paradox in India. In fact, I did hear from scientists many times that they often did not have enough resources even to obtain an international patent, let alone to buy equipment. For example, SGPGI, Lucknow MRI research group under Rakesh Gupta developed several MRI imaging techniques, which they were using locally in their laboratories. They did not have enough funds to obtain international patents (such as in the US, EU, or Japan). These may seem obvious exemplifications of lack of resources in conducting research. However, we have to be careful and analyze 'lack' within particular social-historical contexts. That is, what constitutes 'lack' has to be investigated too. I will come back to this issue. Before that I would like to discuss MRI research in the US and the UK.

## MRI Research in the US and the UK: Demystifying the 'Center(s)'

Going from 'science' to 'technology' is not going from paper world to a messy, greasy, concreted world. It is going from paperwork to still more paperwork, from center of calculation to another which gathers and handles more calculations of still more heterogeneous origins.

> The history of technoscience is in a large part the history of all the little inventions made along the networks to accelerate the mobility of traces, or enhance their faithfulness, combination and cohesion, so as to make action at a distance possible
>
> Bruno Latour, *Science in Action* (1987, 253 & 254).

The history of MRI research and development is a lot messier than I have described until now. A particular problem, as a result of bracketing of what happens in the laboratories (both in the 'centers' as well as in the 'peripheries'), is that even when the historiography of science is criticized and reformulated, the epistemology of science is rarely challenged. STS in the last thirty years has demystified the scientific knowledge production process by analyzing laboratory practices. They have consistently shown that scientific practice, episteme, and culture are multiple and emergent. MRI research was no different at the centers.

Paul Lauterbur, who is credited with the first proposal for magnetic resonance imaging, did not believe initially that NMR could usefully distinguish cancerous and non-cancerous tissues, that Raymond Damadian had proposed (Damadian 1971). When he saw Leon Saryan perform the experiment on a NMR spectrometer that was owned by NMR Specialties Corporation, the company whose President and Chairman he was at that time, he was 'struck by the consistent differences obtained among various normal and malignant tissues' (Lauterbur, 1996: 447). Lauterbur bootstrapped his knowledge and experience in physics and chemistry and proposed a method that could be used for imaging using NMR. At that time hardly anyone was convinced about the possibility of MRI. The editor of the *Nature*, where Lauterbur had sent his paper for publication, at first rejected the paper because the editor and the reviewers thought 'it was not of sufficiently wide significance for inclusion in *Nature*' (Hollis, 1987: 145). Later when they accepted his paper for publication Lauterbur's reputation as a NMR scientist played a crucial role. One of the reviewers wrote, 'if I were not aware of Professor Lauterbur's eminent reputation I would not recommend acceptance without such further evidence' (Hollis,1987: 148).

Lauterbur was also not able to obtain a patent because the patent lawyers associated with the State University of New York, where he had started working after his short stint with the NMR Specialties Corporation, decided in 1974 that his method could not compete with CT (Computed Tomography) scanners that were already in use. Hagiographic histories of science have explained such occurrences as a result of the 'idea' (and the proponent of the idea) being so ahead of time that most people are not able to comprehend its importance. In fact, most often, as has been the case with MRI, the 'idea' can remain just another idea if it is not worked upon and made into a reality. We have to be careful here, I am not arguing that the difficulty lies in the implementation of an idea. Rather, as I will show in the following, the process is open-ended and contingent upon circumstances. This process requires working upon the theoretical as well practical aspects. Discovery and inventions are therefore results of a much broader network of actors as well as scientific work conducted in different places and time, rather than particular activities/ideas of individual scientists.

Even though the possibility of imaging using nuclear magnetic resonance was first proposed in the early 1970s, the history of MRI development from this point to its acceptance as a certified clinical tool by Federal Drug Agency (FDA) in 1984 was far from a straightforward translation of an idea into a machine. In the second half of the 1970s and the early 1980s, scientific groups in collaboration with the industry worked upon and developed different types of magnets, imaging techniques, coils, different types of images, etc. in order to build a clinically useful NMR imaging technology.[7] Among the many concerns that had to be worked upon - practically as well as theoretically - was the issue of developing a high strength homogeneous magnet for imaging. For a long time Oxford Instruments, based in the UK, supplied magnets to almost all the groups engaged in the development of NMR imagers. Derek Shaw writes, '[w]orking, as I was in this period, for Oxford Instruments, was in some ways like being in an Alan Akybourne play. Diverse characters from all the medical imaging companies would come in to discuss their own 'secret ideas' and specify their own unique magnet requirements, resistive/super-conducting, 0.15T/ 1.5T, four coil/ six coil' (Shaw 1996: 623).

The choices for the magnet were made not only on technical grounds. The University of California-San Francisco (UCSF) group, which was working in collaboration with Pfizer from the second half of the 1970s, decided to choose a super-conducting magnet for their NMR imager, after Derek Shaw of Oxford Instruments contacted them with a design for a super-conducting magnet.[8] Scientists at UCSF were not sure whether it would work, but still decided to go for it. Their collaborating partners at Pfizer were, however, quite enthusiastic, '[o]ne thought it sounded so sexy that no doctor would pass it up' (Crooks 1996: 270).

A key concern for scientists was over what should be the optimal magnetic field strength for NMR imaging. Research had indicated that the magnet should be '10 MHz maximum (.23T) for body imaging' (Crooks 1996: 269-70).[9] However, the UCSF group transgressed this theoretical boundary and produced images at 15 MHz. '[T]he lack of rf [radio frequency] penetration problems and effective head and body coils at 15 MHz with superior S/N [signal to noise ratio] began a race to ever higher magnetic field strength' (Crooks 1996: 270). As this case illustrates, even though the development of different aspects of a NMR imager drew upon already available knowledge, they very often occurred through scientists' hands on approach in trying out new things, which resulted in changing the theory too.[10] If, for example, UCSF scientists had followed the proposed theoretical boundary for the magnetic field strength in NMR imaging, they would not have been able to show that good images could be produced beyond this limit.

The issue over optimal magnetic field strength was however not settled even after this achievement of the UCSF group. Even though UCSF scientists had shown that NMR images could be produced at 15 MHz (or 0.3 T), they themselves did not believe that the theoretical limit could be transgressed any further. However, even this limitation was transcended by a hands-on approach and through the intertwining of technical and business concerns.

P. A. Bottomley, who had written one of the most influential papers on the limiting effect of magnetic field strength on penetration of radio frequency pulses, joined GE's Corporate Research and Development Center in 1980. Before joining GE in the US, Bottomley was engaged in the development of NMR imaging at the University of Nottingham in the UK and at Johns Hopkins University. He was hired to conduct research and development in the field of NMR spectroscopy because 'GE had concluded that NMR imaging could never compete with X-ray computed tomography [CT scan] in terms of signal-to-noise ratio per unit time' (Bottomley 1996: 237). In September 1980, GE also hired Bill Edelstein, who was earlier a part of John Mallard's NMR imaging group at the University of Aberdeen. However, it was not until early 1982 that GE seriously thought of developing a NMR imager.

The turning point for them came after they saw the images produced by the machines of Siemens, Phillips, Picker International, Technicare, and some other NMR imager manufacturing companies at the annual meeting of Radiological Society of North American (RSNA) in December 1981. In the light of these exhibits, GE management decided to enter the field of NMR imaging (ibid. 1996). A significant reason behind their decision was their concern that diagnostic NMR imagers could affect GE's CT market. GE had become a major supplier of CT scanners all over the world after it bought Electrical and Musical Industry's (EMI) CT research and development division. The change in business strategy of GE had a direct impact on its technical choices.

GE decided to opt for a lower field strength magnet because in the early 1980s scientists believed that NMR imaging could not be conducted at very high magnetic fields. However, since GE had already placed an order for a high field (1.5-2 T) magnet, because of their earlier decision to focus on NMR spectroscopy, they had to tune their strategy to respond to the changed context. The plan, as Bottomley recounts, 'was to obtain a few spectra at high field when the magnet arrived, then turn it down to 0.15 T' (Bottomley 1996: 238). In the process however GE group ended up devising a head-imaging coil and found that images of the head could be produced even at 1.5 T with that coil.[11] Again, in this case too, performance of experiments and socio-technical tuning led to furthering of the boundaries of the already accepted theoretical boundaries.

I can describe many instances of how MRI development was a contingent process in which ideas, practices, and concerns were continually bootstrapped and translated across different domains. MRI development also involved translations between the outside and the inside of the laboratory (Latour 1983). For example, one of the major concerns for the scientists was the time taken to collect data for the images. It was realized that if patients were kept too long inside the machine there were greater chances of artifacts production because of their movement. Therefore several techniques were developed (and continue to be developed) for faster imaging. As these techniques and the MRI machine were stabilized in the laboratories, they also started to be utilized in different parts of the world.

I can similarly show how the developments of MRI in the UK laboratories were also contingent upon particular circumstances and how they were stabilized. However, in stead of analyzing how scientific practices remain contingent upon particular socio-technical circumstances and then acquire the form of 'immutable mobiles' by mobilizing networks of actors, I would like to provide a cross-national perspective. A part of the reason for doing it is because as Sheila Jasanoff suggests: 'Setting the experiences of one country against another offers salutary reminders of the degree to which even the homogeneous West in not univocal in its responses to science and technology' (Jasanoff 2005: 290).

The possibility of a medical imaging technology using NMR was pursued much more vigorously in the UK in the 1970s. By 1974-75, several groups in the UK started work on NMR imaging. As mentioned earlier, University of Nottingham became a major center, particularly because of the contributions of Raymond Andrew, Peter Mansfield, and Bill Moore. Apart the University of Nottingham, John Mallard's group at the University of Aberdeen made important contributions in the development of MRI. MRI research and development was also carried out at the Oxford University under George Radda and by another group that was based at the Hammersmith hospital in London and associated with EMI. Besides, in the 1970s and the early 1980s there were very few companies that could build magnets with the specifications required for NMR imaging. For a long time Oxford Instruments, which emerged as a result of the work of scientists at the Oxford University, UK, supplied magnets to most of the groups engaged in the development of NMR imagers.

The efforts of these groups in the UK did not go unnoticed by their American counterparts. In a NMR meeting at Winston-Salem in 1981, Bill Oldendorf surmised, 'the poor showing of the US groups relative to those in the UK was due to excessive numbers of US physicists working in defense to the detriment of medical research' (as quoted in Bydder 1996: 248). By 1983-4 (just two years after Oldendorf's comment) many scientists working for MRI development in the UK laboratories left for the US to work in the academia or the industry. Mallard's group at Aberdeen, who had developed the 'spin warp' method for MR imaging, that minimized the effects of artifacts because of physiological and physical motion of patients as well as because of magnetic field inhomogeneity, had already imaged more than 900 patients by early 1981 on their prototype machine that was installed at Aberdeen Royal Infirmary. But Mallard and his group found it very difficult to generate enough money to build their second generation MRI (Mallard 2003).

They initially received a grant of 283,000 pounds from Asahi, a Japanese company and eventually generated 1.5 million pounds and set up M & D Technology Ltd. in 1982. But then, as Mallard himself writes, 'major multinationals were well on their way with their prototypes' (ibid. 362). Slowly his group broke up and several scientists moved to the US. Not being able to compete technologically also had impact on their clinical research. Mallard writes, '[t]he inequity in distribution had at least one ironic consequence: by 1984 our team's clinical papers were being rejected by editors and referees because they were no longer "state of the art"' (ibid. 363). What this example alerts us to is that 'lack' or 'lag' may not be issues and concerns only in relation to the non-west.

UK would have also lost all its MRI manufacturing expertise to GE of the US to whom EMI had decided to sell their MRI research and development division. However, as a result of some last minute maneuvering by Lord Winestock of the General Electric Company (GEC) of Britain, who met the secretary of state of the UK and asked the project be transferred to GEC in consideration of national interest, eventually EMI's MRI division was sold to GEC (Christie & Tansey 1998). GEC later acquired Picker, a CT manufacturing company based in the US, and started Picker International, which maintained a small share in the MRI market until it was sold to Phillips.

In significant ways GE's entry into MRI development was a key moment in the transformation of trans-national scape of MRI research and development. The effort of GE group in producing MR images at high magnetic fields of 1.5 Tesla was not merely a technical achievement. It had significant impact on who could partake in the MRI development process. As Ron Schilling (who was the Vice President international marketing of GE in the 1970s and thereafter became President of Diasonics and then President of Toshiba's US division of MRI research and development) informed me: Even when GE was proposing that MR spectroscopy was the way to go, it was not only that GE did not believe in the possibility of MR imaging, the strategy was to make researchers move in a direction that GE wanted to pursue. That is to say, the strategy was to control the trajectory of MRI research and development so that it remained beneficial to GE. The issue of high versus low magnetic field not only exemplified concern with image quality, but also the cost – cutting magnetic field in half cut down the cost of MRI by almost half too. Hence if research and development of MRI remained in the domain of high magnetic field, it also meant that many actors could not partake in the process.

## Conclusion: Reconfiguring the Trans-National Scape of MRI Research

> [W]e do not have to oppose the local knowledge of Chinese to the universal knowledge of the European, but only two local knowledges, one of them having the shape of the network transporting back and forth immutable mobiles to act at a distance…who includes and who is included, who localizes and who is localized is not a cognitive or cultural difference, but the result of a constant fight; Laperouse was able to put Sakhalin on a map, but the South Pacific cannibals that stopped his travel put him on *their* map
>
> Bruno Latour, *Science in Action* (1987: 229).

Latour through his science in action approach very nicely brings to the fore the problem with arguments about west versus non-west divide that are based on cognitive and cultural differences. One of the examples that Latour draws on to extend his science in action to the global domain is John Law's study of Portuguese sea expeditions to India since 1498 (Law 1986). Law shows that the successful navigation of the Portuguese occurred by developing techniques of 'long distance control'. In the process the Portuguese not only 'disciplined' (so as to make them travel as 'immutable mobiles') the machines but also the sailors (ibid.). He goes on to argue:

> [T]he significance of Latour's analysis of the role of the printing press is that its invention may be seen as a revolutionary improvement in the textual means of long-distance control, one that goes a long some way to explaining both the hegemony of the west and the 'great divide' between primitive and modern' (ibid. 255-56).

Law, however, sees the textual/printing as an exemplification of a broader transformation in the west that can explain its dominance and hegemony. 'The question then', he asks, 'is whether the west has been able to exert particularly effective long-distance control via the people as a result of an innovation analogous to that of the printing press' (ibid. 256).

Postcolonial science studies has to start at the point where Latour and Law leave us – not (just) with the intention of extending it further but also to reformulate the analytics of science in action approach. I agree with Latour and Law that in order to understand the hegemony and dominance of the 'centers' we need to show how they operate as 'centers of calculation'. The movement of Portuguese carracks, as Latour following Law rightly argues, not only had impact on India and other parts of the world, but in Portugal itself:

> As soon as they [carracks] started to reversibly come and go, an ever-increasing space was traced around Lisbon. And so was a new time: nothing before could easily discriminate one year from another in this quiet little city, at the other end of Europe; 'nothing happened' in it, as if time was frozen there. But when the carracks started to come back with trophies, booty, gold and spices, indeed things 'happened' in Lisbon, transforming the little provincial city into the capital of an empire into the capital of an empire larger than Europe (Latour 1989: 230).

> I also in principle agree with Latour that, '[t]he only way to limit this construction of a new space-time would be to interrupt the movement of the carracks, that is, to build another network with a different orientation' (Latour 1987: 230).

The problem with Latour's and more generally science in action approach is only partially what Peter Redfield points out. According to Redfield, 'Latour's principle of symmetry undoes boundaries and oppositions…he short-circuits modernity [and along with it various modernist dualisms] altogether, suggesting that its very self-conception represents an illusion'. Hence, as Redfield adds, '[i]n a world of moving networks we are always somewhere and never quite anywhere at all' (Redfield 2002: 812). The concern that I have is that science in action approach seeks to speak for everywhere from somewhere (the laboratory). Highlighting of the illusory nature of dualisms through a focus on the 'laboratory' simultaneously erases and re-inscribes dualisms. Hence Latour argues that nature-culture dualism is the founding dualism of European modernity and other dualisms like that between the west and the non-west emerge from it. The result is, as Gyan Prakash points out, Latour reinserts a 'first in Europe and then elsewhere' Eurocentric temporality (dualism) (Prakash 1999). Let me further investigate this issue in relation to Portuguese voyages to India in 1498 and thereafter that Law analyzes, which Latour uses as one of the exemplars in his analysis of west versus non-west dualism.

Law concludes his discussion about the Portuguese voyage with the argument:

> I believe the theoretical claim – that the undistorted communication necessary for long-distance control depends upon the generation of a structure of heterogeneous elements containing envoys which are mobile, durable, forceful and able to return – to be well founded (Law 1986: 257).

It is evident that Law wants to show how the Portuguese voyages to India can be analyzed similar to the way technoscience operates in the laboratory. In Law's analysis the voyages seem to become one large laboratory for creating 'immutable mobiles'. Law clearly points out that this is the interpretation towards which he wants to direct us. He writes,

> Thus Latour, who argues that power is a function of the capacity to muster a large number of allies at one spot, suggests that inscription, and in particular printed reproduction, makes possible the concentration of a far wider range of allies than had previously been possible (ibid. 255).

This argument when generalized becomes a truism [winner will most likely have mobilized a larger network and made more allies], but when it is used more specifically it merely shifts the reasons for west versus non-west divide from cognitive and cultural factors to abilities to form networks and makes allies. To quote Latour, what differentiates western knowledges from non-western ones is that 'one of them' [western one] has 'the shape of the network transporting back and forth immutable mobiles to act at a distance' (Latour 1987: 229). As I stated at the outset, it would be improper and unfair to argue that Latour's and Law's writings are Eurocentric. In fact, both of them are trying to go beyond Eurocentric dualist constructions. However, as a result of their laboratory focused approach (whereby the outside becomes an exemplification of 'laboratory' practices) they end up re-inscribing west versus non-west duality.

A postcolonial approach has to begin with a self-reflexive questioning of how networks take particular forms and have particular impacts in dialectical relationship with hierarchical and dualist constructions ('ideological', discursive, as well as 'material') of Europe/west and the rest and more generally the colonial and the colonized. It has to be self-reflexive because the intention has to be decolonization of our 'imagination' rather than yet another effort for sympathetic taking care of the 'other', which as we have seen has been the hallmark of European colonialism. In the first instance this requires an awareness that our interpretations could be distorted or limited because of their dependence upon 'writings' or archives which have been constructed with the European colonial interests in mind. Analogically, an unquestioned reliance on such archives, even if our intention is to transcend the usual dualist formulations, would be like re-analyzing constructions of modern science by positivist philosophers and hagiographic historians based on the latter's writings. Portuguese voyages are a classic example though by no means an exception to such Eurocentric and colonialist constructions.

The European dominance was a far more gradual and complicated process than it is made out to be. Moreover, a European (or western) identity may be an artifact of this gradual process itself. The networks whose interests clashed when the Portuguese started voyages to India for spice trade were not European versus non-European to start with. '[T]he Venetians [Venice was a major spice trading center because of supplies from Egypt and Syria] saw an obvious coming together of their interests with those of the Mamluk Sultans of Egypt and with other political entities in the Indian Ocean to whom the Portuguese presence was a blow – such as Kilwa and Calicut' (Subrahmanyam 1993: 64). Moreover, perhaps the reason why for example the Mamluks lost to the Portuguese was not because the Mamluks did not have a 'disciplined' sea power, after all that is what they were dependent upon to fight their enemies in the Mediterranean and the Red Sea (ibid.). Further, the material agents of this network the ships were not that 'disciplined' after all; they continued to get wrecked in the Indian Ocean until much after the Portuguese control of the trade route to India (Tripati & Godfrey 2007).

I am pointing towards these aspects of the history of Portuguese voyages not because I want to show that there could be other reasons for west versus non-west techno-cultural divide. My argument is that we need to move beyond understanding of European/western dominance through categories of 'lack' of the 'other' (or corresponding 'strength' of the European/west). Amitav Ghosh in his book, *In an Antique Land*, for example, offers an interesting alternative explanation in relation to the Portuguese military exploits at the turn of the sixteenth century:

> Within the Western historiographical record the unarmed character of the Indian Ocean trade is often represented as a lack or failure…Yet it is worth allowing for the possibility that the peaceful traditions of the oceanic trade may been, in a quiet and inarticulate way, the product of a rare cultural choice (Ghosh 1992: 287).

My interest here, as stated earlier, is not to offer alternative explanations for the eventual Portuguese control of the sea route to India. That would require a much more careful reading of the archives and that too with an understanding that most of them would be colonial writings. However, we need to question how 'lack' is constituted and whether we need it as an explanatory category, because one way or the other it just takes us back to European/western exceptionalism. So if it is not science and technology which can explain European/western dominance, then it has to be lack of nationhood, or lack of modernity, or lack of market economy, or lack of democracy, or lack of 'disciplining', the list goes on. These are basically a chain of signifiers whose *point de caption* is European/western exceptionalism. The extensiveness of European writings, for example, need not be an exemplification of yet another reason for European/western dominance but a particular socio-technical choice, which has been effectively used to construct the dominance of the west.

Let me get back to the example of MRI research and development in the US, India, and the UK. As I described earlier, scientists in India could not develop indigenous MRI because they did not have the resources to import the magnet. Similarly, the scientists at SGPGI, Lucknow who developed MR imaging techniques did not have enough resources to even get an international patent. These examples may seem to clearly show that a lack of resources may be the key to understanding 'backwardness' of MRI research in India. But what does this lack constitute? When we say that scientists in India did not have enough resources to import, basically we mean that they did not have enough 'hard currency' (dollar, pound, euro, etc.). One may argue that the reason why the currencies of certain countries are 'hard' is because their economies are resilient and strong. Even if we do not go into the question of the role of European colonialism in making these economies strong, we cannot deny that such a situation differentially and hierarchically inhibits possibilities for people in certain nations. It also guides interactions within networks to certain nodes thereby reaffirming their centrality.

In relation to obtaining international patent the lack is constituted not just because of lack of 'hard currency' but also because of imposition of European institutional practices such as patenting as the norm, which the rest have to follow in order to protect or develop their knowledges and practices. It is quite interesting how practitioners of Chinese and Korean traditional medicine have started to use MRI to show the efficacy of their medical systems. Before we celebrate these as exemplars of 'hybridity' (and hence beyond dualist constructions), we should take a moment to realize that these practitioners have had to use machines that are acceptable to Europe/west to even show that their medicines/practices are scientific/effective. What I am arguing is that science is embedded within a whole host of domains and the categories that are used to analyze these domains or operate within these domains are themselves hierarchically constituted, which gives science an imperial character.

It is not that 'lack' in western countries have not been debated. There has been, for example, a long standing debate in the UK about its decline. A variety of cultural arguments such as ''Britain is good at inventing but bad at developing' the 'low status of engineers', the 'two cultures', the 'anti-industrial' and 'anti-scientific' spirit of elites, are trotted out' to explain the decline (Edgerton 1996: 1). One can analyze the shift in MRI research and development in the UK in the 1980s in the light of these arguments. David Edgerton, highlighting the problems with such explanations, argues that there has not been any absolute decline in the UK but rather a relative one (in relation to other countries such as the

US and Japan, which have grown faster) (ibid.). He also argues that economic growth and science and technology innovation may not be direct correlates of each other. He writes, ''[c]atching up' by inefficient economies seems to have a more important source of growth than innovation. The inter-economy diffusion of technique played a key role' (ibid. 58). It is interesting that Edgerton analyzes science, technology, and industrial policies and practices from 1870 to 1970 without investigating the role of colonialism during this period. There has been a lively debate about 'circulation of science' as well as the role of the colonies in the British economy. I do not wish to rehash those debates here.

Edgerton's analysis is however limiting at several levels. First, he analyzes nations as though they are independent entities, except with respect to diffusion of techniques. This certainly does not hold true. For example, MRI research and development in the UK in the 1970s occurred to quite an extent as a result of several scientists from Australia, New Zealand, and the US. There were fewer contributions by scientists from India, but that could most likely be a result of restrictions in their travel rather than because of their abilities. Besides, as I showed, funding for University of Aberdeen MRI project in the 1980s was to a significant extent provided by Asahi, a Japanese company. Second, economic growth can lead to innovation possibilities. Japan is an ideal example. Third, why could none of the colonized countries 'catch up' for so long and how does this situation relate to the dominance of the colonial countries? I ask these questions particularly in light of the shift in the last few years which has propelled several of the erstwhile colonized countries such as China, India, Brazil, and South Africa as key world economy players. I cannot investigate this shift in this paper but would like to highlight that this shift has allowed countries such as India to develop technologies that largely remained incomplete trails earlier.

Proponents of postcolonial science studies or postcolonial technoscience often portray post-colonial analysts of science (in terms of their origin) as well as analysts of postcolonial (or colonial) science as the flag-bearers of an alternative mode of analysis that highlights messiness and ambiguity while at the same time challenging the dominance of western knowledges. Such an approach not only ends up mischaracterizing the writings of these authors, it also creates confusion about what postcolonial science studies is and what could be its objectives. Postcolonial science studies have to focus on the multi-faceted role of colonialism in technoscientific practices. This in the first instance requires a 'decolonization of imagination', otherwise such efforts will continue to take the form of yet another sympathetic undertaking of those located in the 'west' to take care of (or give 'voice' to) the 'rest'.

Postcolonial science studies already have a head start. STS, by showing that the episteme, method, and practices of science, are multiple and contingent upon circumstances, has already 'provincialized' modern science. The problematic cannot be (just) to show that European knowledges are also local, because such a position begs the question how did the movement from multiplicity of sciences to some kind of identity, albeit local (of European, Chinese, Indian, etc.), occur? Warwick Anderson rightly suggests, '[e]ven the most local studies should imply a network, suggesting connections with other sites through traffic of persons, practices and objects' (Anderson 2002: 652). However, we need to undertake such projects not because the boundaries (of nation, west/non-west, science, etc.) are melting away but rather because they have been and continue to be reconstituted and reconfigured in differential and hierarchical ways by drawing upon dualist and colonial constructions of the 'self' and the 'other'.

## Notes

1. This study was funded by National Science Foundation (NSF) grants # 0724474 and #0135300. The findings and conclusions expressed in this paper are those of the author and do not necessarily reflect the views of the NSF.

2. Bruno Latour uses the term technoscience to 'describe all elements tied to the scientific contents no matter how dirty, unexpected and foreign they may seem, and the expression 'science and technology', in quotation marks, to designate *what is kept of technoscience* once all the trials of responsibility have been settled' (Latour 1987: 174).

3. Dipesh Chakrabarty argues that Eurocentrism is tied to historicism in the sense that it is articulated within the temporal order of 'first in Europe and then elsewhere', whereby the rest of the world are consigned to 'waiting rooms' of history (Chakrabarty 2000).

4. Anderson in his analysis of 'postcolonial technoscience' cites Stacy Leigh Pigg to argue, 'we now need to find out more about how science and technology travel' (Anderson 2002: 644; also see Pigg 2001).

5. Japan is often cited as an exception to the west/non-west techno-cultural divide. There have been significant efforts to understand and characterize Japan's role in techno-scientific research. In the story of MRI there is little evidence that scientists in Japan engaged in the development of MRI in the 1970s and the early 1980s, even though several Japanese companies, particularly Toshiba, became major manufacturers of MRI in the late 1980s. Toshiba became a major player in MRI manufacture after it bought Diasonics an American company engaged in the manufacture of MRI (Diasonic's MRI machine was the first to get FDA approval) and became a beneficiary of University of California-San Francisco's (UCSF) MRI research because of Diasonics' collaborative relationship with UCSF.

6. The debate over alternative or appropriate technologies is often articulated in relation to the dominance of the west and modern western science and its negative impact. However, it differs from the debates over alternative sciences because often the focus of the latter has been to search for 'epistemic' alternatives to modern

science (Nandy (ed.) 1990; Nandy 1995; Uberoi 2002; Visvanathan 1997).

7. Joseph Battocletti (Battocletti 1984) provides a detailed discussion on different techniques being used by the companies to build a NMR imaging technology in the 1970s and early 1980s.

8. Initially Shaw "was much more conservative about superconductive magnets since Oxford [Instruments] was working on four coil resistive air core designs" (Crooks 1996: 270). In these business transactions for the supply of magnets national interests were also at play. Lawrence Crooks informed me that Oxford Instruments supplied the magnet to the EMI group in the UK before supplying it to the UCSF.

9. Two important studies that indicated the limits of magnetic field strength that could be used in imaging were by D. I. Hoult and P.C. Lauterbur (Hoult and Lauterbur 1979) and P. A. Bottomley and E. R. Andrew (Bottomley and Andrew 1978). The argument by the former was that signal to noise ratio would decrease as the field strength is increased, while the latter argued that as the field strength increased the penetration of radio frequency pulses that are used to spatially measure relaxation times or proton density is ideal between 10-30 MHz.

10. Andrew Pickering (1995) shows how already available knowledge/practices are used as 'models' such that techno-scientific research continues to have emergent properties.

11. GE wanted to showcase these images for the annual RSNA meeting in 1982, and their research group was working hard towards producing better resolution images. But their machine broke down. Nonetheless, the high-field images produced on GE machines created a sensation during the 1982 RSNA meeting.

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