On the Peripheries of Western Science: 
Indian Science from 1910 to 1930, 
A Cognitive-Philosophical Analysis

A DISSERTATION
SUBMITTED TO THE FACULTY OF THE GRADUATE SCHOOL
OF THE UNIVERSITY OF MINNESOTA
BY

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IN PARTIAL FULFILMENT OF THE REQUIREMENTS
FOR THE DEGREE OF
DOCTOR OF PHILOSOPHY

Ronald Giere

December 2010
"There needs to be greater recognition that what is called Western science drew on a
world heritage, on the basis of sharing ideas that make science what it is. The sharing
culture of science must be recognized as an important organisational tradition, which
continues to be significant today."

— Amartya Sen, New Scientist, No 2340, 27 April 2002 (italics mine)
ACKNOWLEDGMENTS

A journey like this requires mentors, friends, and several supporters along the way. Additionally, I completed this project while being half-a-world away from my home country. For the first time in my life, I was away from my family and from the culture in which I was used to think, write, and function. Thus, my journey in producing this dissertation resembles somewhat the journey of the scientists that I have written about. Like them, I too had taken up a new practice that originated in the contexts of a different culture; like them I too was seeking to adopt that practice in order to make it express something new, exporting it to an altogether different context.

My thanks go first to Ronald Giere, my advisor, who patiently stayed with me while I stumbled through many false turns and an equally numerous number of partial and incomplete drafts. Not only was he stuck with the job of reading and correcting all my drafts—the usual task of an advisor—he was additionally charged with the task of editing out my numerous non-standard usages of English, which made the process of advising certainly very interesting on both sides! I thank my wonderful committee members, whose feedback allowed me to arrange my views into some sort of a coherent framework, especially in the last few weeks when we were working towards the final draft of this dissertation. Naomi Scheman urged me to engage more deeply with the claims that I was making—which made me rewrite several paragraphs and at least one entire section. Alan Love wanted me to place more emphasis on certain important distinctions that I was making in my text, and finally, Paul Johnson wanted my project to be clearly demarcated from other similar projects. I benefitted from all of their suggestions. My thanks go to all of them for their insightful comments and their suggestions.

To my friend James McAllister, I owe a special ‘thank you’ for his company, conversation, and above all, for his steadfast love during all these years when I was writing this dissertation. He always believed my project to be worthwhile, even when both of us disagreed about the specifics of how this project must be accomplished. I owe also a special thank you to Prof. Joseph Margolis, who wrote letters in my support when I applied to the graduate program in Philosophy in Minnesota, and whose support letters actually got me admitted into the program. My thanks go to Subrata Dasgupta and to Susan Hawthorne for their continued conversation, exchange of views, and above all, for their emotional support. To Kathryn Plaisance, I owe heartfelt thanks for providing me with a crucial input about the organization of my dissertation which caused me to rewrite the entire first chapter (thank you, Katie!). To my friends Karen Treiber, Amel Khalfaoui, and Pamela Ludford—my main social supports during all these years in Minnesota—I say ‘thank you’. I appreciate your concern about my well-being, and will always remember the many wonderful evenings that I have spent in your company and in your house.

Finally, my thanks go to the Philosophy Department at the University of Minnesota, which has been my institutional home for the last eight years, and which provided me with a fellowship under the Graduate Research Partnership Program (GRPP) in 2008, and two other similar summer fellowships during the summers of 2004 and 2005. I thank the Center for Philosophy of Science at the University of Minnesota, which offered me a year-long Visiting Fellowship during 2002, which is how I first arrived at Minnesota. I thank the organizers of the Society for Philosophy of Science in Practice (SPSP) Conference in 2009, and more recently the Science, Technology and Society (STS) Conference, 2010 at the University of Texas, Austin, for
giving me the opportunity to articulate my views before a sympathetic audience. My thanks go also to the Department of Strategic Management (SMO) at the Carlson School of Management at the University of Minnesota, for providing me with continued opportunity to be part of their courses on business ethics, which allowed me to see how the practice of business often mirrors the practice of science. Lastly, I say ‘thank you’ to the wonderful staff of the Philosophy Department, to Judy Grandbois, Anita Wallace, and Pam Groscost for their continued support during the years of my graduate program. Finally, this dissertation is given with love to my parents, to Anil and Manisha Dasgupta, who always told me that there is no greater good in life than knowing things well, even when such efforts take you halfway across the world.
ABSTRACT

That newcomers often take up science remains a prominent feature of scientific practice. Thus, around the established centers of scientific knowledge there grows up a periphery, consisting of various types of newcomers: self-trained autodidacts, people from different disciplines as well as researchers from other cultures and other communities. As new communities join the previously-existing core group, the size of the network increases, setting up complex relationships of collaboration and competition among members of the community. Behind most of the scientific communities that today exist in the non-West, there lies this kind of a complicated history of origin. And yet, within the existing philosophical models of scientific practice that we have with us today, there seems to be no account that can tell us how such newcomers—who become scientists mainly through their own individual efforts—function in science. This seemed remarkable to me when I first started reading the literature of philosophy of science during my initial years in the graduate school.

Scientists from the non-West constitute one such prominent group of newcomers who often work from the peripheries of scientific knowledge. In his well-known model of the expansion of Western science into the locations of the non-West, George Basalla (1967) considered peripheral science, i.e., science practiced outside of Europe and North America, to be an instance of diffusion: thus stating, in effect, that those who accept science under such circumstances, accept it as a recipient. As is well-known, this model has been extensively criticized and it has also been suggested that science is perhaps a case of a moving metropolis, that the centers of established knowledge in science shift dynamically over time. However, precisely how the metropolis of science shifts from one place to another and how the newcomers who join the practice of science function within it, remained unclear. Thus, Basalla’s model might have been rejected, but nothing adequate so far has been put into its place.

This dissertation is an attempt to think about this long-neglected topic. It seeks to understand peripheral scientific communities and peripheral interactions, and the growth of scientific knowledge within those non-standard contexts. It is about those scientists whose research take place outside of the main community, and yet who often contribute quite significantly to the stock of scientific knowledge. It is written in the belief that there is more to peripheral science than passive acceptance and manifest individual difficulty, that it is intrinsically interesting, and that it tells us a story about science itself, especially about how science is socially organized, and the epistemic consequences of such organization.

Hopefully, here is a new topic that can be elucidated by further research—by myself, and by others, who I hope, will join me soon.
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INTRODUCTION

Obsession with big names and with the important events is prominent in philosophy of science. Most philosophical accounts of science concentrate on such important episodes, either by providing explanations or reconstructions of those important episodes. In contrast, this dissertation will be about the smaller names in science: peripheral scientists, first-generation researchers, and other such new beginners who generally work from outside of the main scientific community, but yet sometimes make important contributions in science, though remaining generally little noticed. But in such modest beginnings there could be big possibilities, and those possibilities require analysis. This is the thesis of the whole dissertation.

It will be noticed that this is a work which is devoted entirely to the contexts of non-Western science. This demands a little explanation. Why are such contexts important in explaining science? Speaking intuitively, we generally grant that the scientists reach solution of their problems by an exercise of reasoning. Science is, in essence, an activity devoted to problem-solving, which involves in turn an extensive use of reasoning and other such similar mental and embodied operations. A number of key texts within the standard literature in philosophy of science—from Karl Popper (1959) to current journals such as Perspectives on Science—assure us of this view.

Yet, very little literature exists in philosophy of science that talks about reasoning or problem-solving practices undertaken in the contexts of non-Western scientific communities. The vast literature on science in the non-West comes mainly from history and sociology of science, from studies of technology, and more recently, from the postmodern and the post-colonial critiques of science. But if one wants to know in any significant detail about how a non-Western scientific community engages in scientific practice, how it articulates and develops its reasoning practices on a particular research problem (and offer possible solutions to that problem) how it develops a new experimental set-up or addresses a new inquiry, one would be hard pressed to turn towards any particular philosophical literature. Indeed, such a literature is almost conspicuous by its absence, for most literature in philosophy of science invokes— almost always—the familiar contexts of the Euro-American science. Yet, it is a matter of historical record that, with the transmission of Western science into various locations of the non-West (e.g., during the late 19th and the early 20th century), a number of nascent scientific communities emerged soon within the various locations of the non-West, which, since then, have contributed significantly to what we now call modern science. Not only did the non-West supply raw data in the making of scientific discoveries, important scientific concepts—such as the concept of the indistinguishability of particles in quantum mechanics or the notion of a mock theta function in mathematics—originated first with a non-Western scientific community and were taken up from them by the main Euro-American science. Yet, very few non-Western scientists or scientific communities have been studied in any significant detail by the philosophers of science who take upon themselves the job of explaining science, especially how science produces new knowledge.
Why should our explanations of science focus only upon the contexts of Euro-American science, if by science we now mean a globalized community—and thus by implication, a globalized practice?

Recently, however, there have been some modest changes. A detailed study of the Japanese high energy physicists has been undertaken by Sharon Traweek (1992), to which task she has devoted a book and a number of articles. Even more recently, a study of peripheral science in Latin America has been undertaken by Alex Levine (Levine, 2010). Still, not too much is known, in general, about how peripheral scientific communities emerge, engage in science, and how they develop their respective forms of scientific practice. We also know little about how they begin their quest for scientific problems, and how they build a network of epistemic expertise with their peers from their difficult contextual settings. In short, we do not know how such scientists become reliable producers of scientific knowledge. This leaves a gap in our general understanding of science, especially if by science we mean a globalized community of knowledge, and not just a Western epistemic culture that started its career during the 17th century Europe.

This dissertation is an attempt to think about this issue and to develop a framework and a theoretical language for addressing such scientific situations. It contains a framework that looks at peripheral science (and thus by extension at peripheral scientists) from a philosophical-cognitive point of view, and uses this framework to analyze two episodes of peripheral science that took place during 1910-1930 in British colonial India. This was a time when a new culture in scientific practice emerged within the Indian subcontinent through the efforts of a few young researchers, who were mostly engaged in doing different sorts of theoretical physics. Working from their obscure location in the colonial city of Calcutta—then a center of the British rule in the Far East—this small group of young scientists contributed several important results to the stock of scientific knowledge that brought into existence a vibrant but a short-lived epistemic culture around the city of Calcutta. This culture subsequently became the predecessor of the scientific community that exists today in the subcontinent.

Those efforts took place against the backdrop of a sharp cultural encounter. The newly-born culture in science was part of another all-encompassing event, better known in history as the Indian or the Bengal Renaissance. Starting from the late 19th and extending up to the early 20th century, a remarkable phenomenon took shape all over the Indian subcontinent which was then undergoing an intense form of encounter with colonialism—being part of the once-widespread British Empire. Starting from the city of Calcutta in Bengal, but spreading soon to other urban centers of India, a new way of looking at the world was slowly taking a concrete shape. Much along the lines of the more well-known European renaissance, this non-Western awakening too brought major changes and major clashes of ideas in its wake—a result of two cultures and two societies coming face to face through the fortuitous circumstances of European colonialism. The results of this encounter ushered in a new mindset in the subcontinent. Whether in literature, political thinking, or in art, what came after the awakening was significantly different from what
went on before. The regeneration produced a peaking of the nationalistic spirit, giving rise to many new movements within literature, music, and arts but its most significant contribution was the formation of a new culture in the sciences, which today forms the cornerstone of Indian modernity.

Recently however, the very notion of such a Renaissance has been rejected. The above account represents an official view, supported by a vast historical and a sociological literature of its own. But with the rise of the different varieties of the post-colonial and the post-modern discourses — and that of subaltern historiography — the claims of the Bengal Renaissance to be called a genuine movement at all was sharply disputed, and soon it was denied to be a movement in any significant sense. The period that had until then been called as India’s stepping stone to modernity, was hereafter debunked as a phenomenon confined to a few colonial elites who depended upon the imperial administration (and its various policies) for their own comfortable living. As a movement that remained conspicuously confined within a small, Western-educated, colonized elite, the Bengal Renaissance’s claim to be called a genuine movement soon became a natural target for suspicion under the agenda of the post-modernist discourses. With it of course came under scrutiny the most quintessential Western product of this period — science. The sole legacy of this scientific culture, the post-modernists argued, was to drive a permanent wedge between the English-speaking intelligentsia of the country and its common people, who remained largely excluded from the possession of this privileged colonial language. The rising multiculturalism, emanating from the various American universities during the decade of 1980’s articulated a new question: can we formulate (and practice) an alternative form of science for the non-Western societies and set aside the agenda of modern Western science? In what sense is modern science an asset for such societies?

I take this debate about the aims of science in the non-West as the entry point of my dissertation, providing me with a context and a question to investigate, and motivating the need for this kind of work. In this dissertation, I undertake to examine a period of science in India during the colonial period of 1910-1930, which was initiated by a small group of young Indian physicists, who, back then, had just started their careers as peripheral scientists. By importing a method that has been used to explore the well-known conceptual changes in science, e.g., the construction of a new scientific concept or the production of a new experimental artifact, I propose to examine the epistemic goals and the practices of this small scientific community and through their lenses, the general nature of all peripheral scientific practice. I claim of course that the historical existence of such communities make them important contexts of understanding science. The goal is to ask in what sense such scientists can be called successful in creating a new tradition that can be considered as the starting point of an independent scientific community, and what were the cognitive processes operative behind such peripheral scientific creativity. In doing this work, I have used an expanded notion of scientific reasoning, borrowed from the literature of cognitive science, that tells us how the human mind conducts reasoning in general, how it sees and constructs a problem, and how it transforms those problem-situations into solutions. Such an
analysis of science has been used in the last twenty years in order to gain a deeper understanding of the sciences in its Western context, particularly during those periods when significant conceptual changes occur in science. In writing this dissertation, my goal was to export this analysis of science outside of its standard Western homeground—to illuminate the contexts and the creativity of a peripheral scientific community.

With this end in view, the plan of this dissertation is as follows. After an introductory chapter, where I argue that the peripheral scientists and peripheral scientific communities constitute an important context of scientific activity, my second chapter begins with a discussion of the debate on alternative science. In responding to this debate, I propose a methodological shift—a method called CPH (Cognitive-Philosophical-Historical) analysis—by means of which I propose to examine the contents of the science done in India by a small peripheral scientific community during the period of 1910-1930. Thereafter, in my next chapter, I sketch the outlines of this CPH analysis by putting together insights from a number of philosophers who derive their general inspiration from cognitive science and who show how this view of human knowers can be used as a resource for understanding scientific activity. Four people are taken to exemplify this approach: R.N. Giere who tells us that scientific reasoning is model-based reasoning, Nancy Nersessian and David Gooding, who show primarily through their case studies how metropolitan scientists like Faraday and Maxwell construct new concepts by using different forms of visual or simulative reasoning, and finally Michael Gorman, who tells us that an ongoing scientific practice can be analyzed as a trading zone that involves multiple, dynamic levels of cooperation among its agents. With these notions in hand, I seek to put together the portrait of a peripheral scientist and a peripheral scientific community—operating mostly within the contexts of the non-West—who enter the network of science as its newly-initiated members, and seek to begin a national tradition of science for their home community. The task then becomes to show how such a community develops an emerging practice from its peripheral vantage-point by establishing a zone of collaboration with its more privileged counterparts at the metropolis. What I seek to develop in the rest of this dissertation is a cross-section of such peripheral cognitive creativity.

Chapter four discusses how a peripheral scientific community forms its epistemic networks, which are necessary for its scientific practice. Here I sketch an intuitive, analytic schema to explore and analyze the networks of communication between a peripheral and a metropolitan community. Since it is impossible to analyze all peripheral science that had taken place in India during early 20th century, I select the academic discipline of physics as my main focus of study, and select two episodes, involving three Indian scientists, as my cognitive agents. Thus, in Part II of this dissertation, in Chapters 5 and 6, I develop two case studies from this group—S.N. Bose and his formulation of the Bose-Einstein statistics in 1924, and C.V. Raman and his discovery of a new type of radiation in 1928 with his student K.S. Krishnan, now called the Raman Effect. In unpacking these two episodes of Indian peripheral science my goal is to show the complex structure of Bose and Raman’s peripheral scientific practice and how as peripheral scientists they put together a research program by using different kinds of cognitive
strategies (which they developed under their marginal conditions), and what kind of intellectual authority could they eventually claim for their work. I argue that this science—although it suffered strongly from a problem of restricted agency—was nevertheless original and creative in a genuine sense. Finally, in conclusion, I argue that an account of science that take seriously the peripheral contexts of science allows us to develop a more nuanced view about such science, showing us to steer a middle course between the extremes of adoration and rejection of science. This, in turn, requires that we make some in our current social models of scientific practice, and by implication, this revises how we see peripheral science.

The goal of my dissertation was to construct a cognitive-philosophical-historical account of a generation of peripheral scientists in India who worked between the two World Wars in order to develop a new culture in science for their home country. With this end in view, I have taken up the question of the production of scientific knowledge from the perspective of a peripheral researcher, who always works at a considerable distance from his/her metropolitan peers, who remain crucially dependent upon their reception of his/her scientific work, and yet, who hopes to reach epistemic independence eventually by means of this complex trajectory. Thus, I have tried to take the perspective of how such a scientist would go about choosing his/her research problems; how he/she would use reasoning or an embodied activity to arrive at a conclusion, and in what way would he/she seek to gain intellectual authority in his/her chosen field of research.

My reflections began by asking the simple question of how a peripheral scientific community would begin its journey towards making new scientific knowledge, what goals it would seek during those encounters, and what problems it would face during such tasks. The usefulness of this exploration, as I see it, will be found in getting clear about how scientific knowledge disseminates and develops across different cultural and contextual settings—how it gives rise to a network of collaboration among many different communities, thus, in a sense, linking different cultures of the world. The allegedly universal character of science surely depends upon how well we understand this process, and what models we form of its operations during its different stages.
Part I: The Theoretical Framework

Chapter 1: Science and Peripherality: The Newcomers and the Outsiders of Science

“Science is a global activity with consequences for all our lives. It is also a human activity with ethical, social and political dimensions… The impact of science is not confined to scientists but affects all people everywhere.”

—Webpage of the ASE (Association for Science Education)

“When we speak today of natural science we mean a specific vision created within Western culture, at once of knowledge and the object of knowledge, a vision at once of natural science, and of nature.”


1.1. Does Peripherality Exist in Science?: The Notion of a Peripheral Scientific Community

What is science? Just like any other discipline that explains its own subject matter, philosophy of science seeks to give us an answer to this question. Attempts to answer this long-standing question have provided us with a set of views, giving us many of the standard positions in philosophy of science. A quick survey of some of these positions gives us the following sorts of thumbnail sketch about science. Science offers us knowledge—or at least a working theory—about our natural world. The habit of giving such explanations arose first in Europe during the 17th century, a direct continuation of such earlier habits among the Greeks, and from this Western location it was exported gradually to all other locations of the world.1 An extension of the scope of this knowledge to other parts of the world will eventually bring about a state where all prejudices and superstitions will be abolished permanently in favor of reason.2 Viewed like this,

1 For statements of the first type, see Herbert Feigl, 1988, “The Scientific Outlook” in Introductory Readings in the Philosophy of Science, p. 428. For statements of the second type, see A.C. Crombie (1996), p.12. “the history of science is the history of argument: an argument initiated in the West by ancient Greek philosophers…”

2 This is the view of science that was exported to other non-Western societies. For example, see Sir Alfred Gibbs Bourne, “On Scientific Research” in The Shaping of Indian Science, vol.1, p. 39.
the West becomes the natural home of science, and any attempt to contribute to this practice from outside of this homeground, especially from outside of Europe and the North America, becomes peripheral, i.e., less important in the scope of discussions. Consequently, peripheral scientific practices (and peripheral scientific communities) have rarely been featured as the main topic in any philosophical discussions about scientific knowledge.

Any unsettling of this accepted worldview will require that we give some sort of explanation about why peripheral contributions in science should be deemed as important³. Is there something in the property of peripherality that should draw our attention to such science? But claims like this will appear to be highly controversial. It could be argued that peripherality is not a significant property in science, and hence, it is fruitless to try to analyze episodes of science in its terms except perhaps in a limited, historical sort of way. Furthermore, it could also be argued that the location of a particular community of scientists in a certain part of the world—and the position of such scientists in the hierarchy of those who make (new) knowledge—are not at all relevant in seeking to understand the nature of the scientific contributions eventually produced by them. In science all communities share an egalitarian status—at least in principle—which more than adequately compensates for any possible peripheral effects.

So far, this view of peripherality has largely dominated our intellectual landscape. The interesting science—the received view claims—is mainstream science. What lies outside of this concentrated mainstream could be interesting to the historians or the sociologists of science, but this is of little importance to the philosophers of science who seek to understand the nature of epistemic engagements in science (and possibly reconstruct some of those interactions with their chosen analytic tools). What kind of epistemic interactions is represented in peripheral science? The traditional view has been that such science is either essentially similar with or is entirely derivative from mainstream science, and thus does not require any separate exploration.

However, ever since the work of Thomas Kuhn, a whole new way of viewing science (and that of the whole scientific enterprise), has emerged within the philosophy of science. Philosophical attention to science has been shifted, post-Kuhn, to the practices and the processes of science rather than to its products.⁴ Thus, philosophy of science today no longer consists

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³ The particular peripheral relationship that I shall explore below is that of a non-Western scientific community seeking to contribute to the Euro-American science and thereby building a (national) tradition of science at its own home location. As I shall discuss below, peripheral relationships in science can occur in a variety of ways.

⁴ The Kuhnian analysis of science did not consider of course if there could be peripheral members within a scientific community. However, in focusing our attention upon the practices of science in a community, Kuhn’s analysis becomes a helpful tool in grasping how a scientific community can acquire new members (from a periphery).
merely of analyzing the endproducts of science; increasingly such analysis focuses upon the daily
details of scientific practice: communication among scientific communities, their aims, their
cognitive make-up, in short, the entire social and the cognitive matrix by means of which a
research community accomplishes its everyday work. If we can show that peripherality is
frequently a part of those interactions—often infusing the endproducts and the practices of
science — then perhaps we could find some sort of room for the notion of peripherality and that
of peripheral scientists. Is it at all possible that we should be able to show this by an analysis? In
what follows, this shall be my main goal, and I shall argue that peripherality is a surprisingly
common feature of scientific interactions, especially when those interactions are embodied in
scientific communities who work together collaboratively and competitively but from their
varying levels of epistemic authority. Complexity of cognitive labor of this type entails that in
these cases some of these communities will enjoy a higher level of trust than their other
colleagues and others, conversely, will be trusted less. Thus, in peripheral science, i.e., science
practiced outside of the Western context mostly by the newcomers and the outsiders of science,
we see how trust and authority are differentially allocated and differentially negotiated in science,
especially when new research communities, who do not already possess an established track-
record, join its practice. Thus, in peripheral science, we get to see how new communities join in
science and make themselves participants in an already-existing practice, moving into a new
landscape from their differential vantage points.

In Philip Kitcher’s *Advancement of Science* (1993), we see a glimpse of this framework.
In Kitcher, we find the analysis of a scientific community that pursues science not merely out of
epistemically pure goals—such as truth—but is driven by many other non-epistemic goals besides
their overtly-proclaimed epistemic ones, such as the goal of being the first ones to make a
discovery. Such a driven community is called by Kitcher an epistemologically sullied one (1993,
chap 8, p. 310) but Kitcher goes on to argue that such a sullied community can nevertheless be
wonderfully productive from an instrumental point of view—both in terms of the results that
such a community achieves as well as the new tradition of research that gets subsequently
founded upon its work.

In the sections below, I shall embrace this insight, and argue that peripheral science is an
example of such an epistemologically sullied science. Thus, in spite of its manifest asymmetries

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5 Such scientists are described by Kitcher as ‘epistemologically sullied agents’. Kitcher’s sullied agents of course come
from within an established community (rather than from the outside or from a periphery) but it is easy to extend this
analysis to include the peripheral members of a community. The sense of the term, as Kitcher uses it, is non-classical
and complex, but not necessarily negative.
and shortcomings, it can still serve as the basis for many new beginning in science. This is precisely why philosophers should take an interest in peripheral science. Speaking for the moment from a peripheral point of view, such science can become the foundation for a new research tradition (in the sense of creating a new scientific community) based upon the work of a few lonely pioneering scientists who nevertheless are able to create a new community in a new social and cultural setting. As a prime example of how new communities emerge and begin their careers in science in the midst of an epistemologically sullied environment, peripheral science can be deemed as interesting in its own right. In such science we observe the differential quality of social interactions that frequently pervade engagements among research communities, often shaping their future epistemic outcomes.

The role of the social and the cognitive interactions among scientific communities (and its impact in the making of new knowledge) were recognized explicitly by Thomas Kuhn. The notion of a community’s agency, i.e., what groups of people do with their skills and their minds, today occupies a huge part of the literature of philosophy of science, showing how different cognitive and social factors play a crucial role in the making of reliable knowledge. Many volumes in philosophy of science have been written since then to shed light on how community interactions pave way to the making of public and reliable knowledge. An excellent example of this genre of analysis is Longino (1990; 2002); where in two volumes she addresses the problem of how science can be called objective in spite of its intensely social character. Similarly, in the works of other social empiricists, e.g., in Miriam Solomon (2001), we encounter discussions on other dimensions of the same issue. In all such works, philosophers of science seek to show us the different ways in which social interactions contribute to the making of scientific knowledge.

These admirable portraits of science no doubt capture important properties of scientific engagements, such as its social dimension and its prized epistemic character. Nevertheless, in this chapter, I shall argue that such social accounts—as well as the more traditional, analytic account on science—neglect, or at least considerably downplay, certain important features of scientific engagements that link it directly to the peripheral contexts of science. In most social accounts of scientific knowledge for instance, we find no mention of the location of scientific communities. Since scientific communities embody all the necessary expertise and the social properties necessary to make new knowledge, one might reasonably ask where such communities are

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6 Kuhn saw those processes as more psychological than cognitive but that was the dominant analysis in his time. For more on this issue, see Giere (1992), editor’s introduction, Minnesota Studies of Science, vol XV.
supposed to be located. And since equally clearly, there is more than one such knowledge-producing community, how do these communities interact among themselves? What is their form of internal organization, and how does such organization impinge upon the business of making new knowledge in science?

If one wonders about such questions while reading Kuhn’s well-known account of anomalies, revolutions, and of the normal periods of science, one is bound to come away a bit perplexed, for no specific answer to these questions is given in Kuhn. Neither can one be extracted from the subsequent literature of philosophy of science, which nevertheless reflects at length on the social properties of scientific knowledge. The talk of revolutions, paradigms, research programs and research traditions suggests however that the research communities—the active producers of new scientific knowledge—share (at least) some of the following basic properties. First, they share (as a group) most of their traditional stock of knowledge and expertise, in effect sharing a similar conceptual toolbox (i.e., a tradition of knowledge). But note that this can happen only when these communities either live in close proximity or communicate frequently with one another—in other words, when they share a long history of collaborative exchanges. This further presupposes that interactions among those communities are neither infrequent nor remote. In other words, the social image of science that our social epistemological models capture currently is the image of a highly concentrated center, possibly located in a contiguous space.

Social models of science that are built upon this (tacit) assumptions naturally take for granted that all scientific communities share a certain level ground with respect to their authoritativeness, thanks to their long history of collaborative exchanges. When we reflect on science using such models as our lenses of analysis, we are naturally led to think of science as a concentrated-center that contains little or no peripherality. The locus of this concentrated center is of course presumed to be in the West, from which, it is further assumed, that the sciences radiate to all other parts of the globe.

But a concentrated center of science is not the entire story of scientific practice. If there exist scientific communities, who participate in the task of making (new) knowledge in science from outside of the main community, then clearly, under our current social models of science, such communities and their epistemic efforts will find little representation. Scientific communities that are newcomers to science—whether this is due to their being outsiders or simply due to the geographical distance that separates them from the main community—are yet to become full-fledged topics in the philosophy of science. Consequently, we know very little about how such communities participate in science, how they produce new knowledge or build a
new practice, and how they form a network of communication with their established peers in the process of seeking a consensus or recognition for their work. It is of course true that, on the whole, our models of scientific interactions have become remarkably social. But ‘social’ in this sense still excludes a large variety of epistemic engagements that can and often do take place in science.⁷

In this dissertation, my goal will be to question the assumption of science-being-always-concentrated-at-a-center— which is held implicitly but quite constantly— in most social models of science. Such leanings in social epistemology have perhaps been taken over from its analytic predecessors. The result of this assumption has been that science is usually perceived to be a center of activity concentrated in the West. Contrary to this received view of scientific practice, in this dissertation I shall highlight the importance of peripheral practice, i.e., those interactions in science that take place outside of the main community and yet produce significant gains in knowledge. I shall contend that such interactions allow us to see how science evolves when new communities join the practice of science to form new (epistemic) partnerships with established communities from their differential vantage points (i.e., from new national and cultural settings), and the new ideas and contributions that they bring to such practice. This kind of lateral expansion of scientific communities has been one of the most striking features of science in the last one hundred years but it has so far received surprisingly scant philosophical attention. For example, in his attempt to create a database for the social existence of science during the 17th-century, Richard Westfall reported that he could put in 630 names within this category (Westfall, p.3, 1994).⁸ Today, any head count of scientists would surely exceed that small number, running perhaps into many thousands, and many of those names will surely include members from (several) non-Western scientific communities. By what process has this lateral expansion of scientific communities (and by implication, of science) has taken place, and how do we analyze the inclusion of these new members into the network of scientific practice? How do such members interact with their already-established peers and what is the nature of their subsequent scientific practice?

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⁷ Peripherality however is beginning to emerge as a topic in some recent discussions on science. See, for example, the website of a newly-founded international research group called STEPS (Science and Technology in the European Periphery) www.cc.uoa.gr.step/step1.htm, accessed on 10.7.2009.

⁸ Richard Westfall, “Charting the Scientific Community”, in Trends in the Historiography of Science. Westfall notes of course that he was concerned only with the Western scientific community, hence he did not include any Arab names within that timeframe. For more on this issue, see Bala (2008), who argues that the Arabs and the Chinese must be accepted as the predecessors of European science.
A little reflection of this type shows us that science has always been surrounded by newcomers of various types: peripheral scientific researchers, outsiders, people from new disciplines and other cultures. Outsiders like this frequently leave their mark on science and bring in important contributions. However, in order to understand how such people can and do contribute to science—i.e., how newcomers and outsiders participate in the work of making new knowledge from their unusual vantage-points—we need a new framework that no longer sees everything in the image of a center. Most of our current analysis of scientific knowledge however is built upon that assumption, thus allowing little room for any discussion about peripherality. The net effect of this presupposition has been that science is analyzed often as a Western epistemic culture, with little or no discussion of the role that the newcomers play within it. The consequent epistemic activity that such people bring to science—e.g., the relationship that they build with their main community, the research programs that they develop—in short, their entire contribution to the scientific enterprise has remained obscure. Making scientific knowledge from peripheral vantage points has remained a neglected area in philosophy of science, and even though we find some recent nods towards those directions, e.g., in Longino (see p. 229, 2002), such views appear only in the conclusions of their authors.

The notion of scientific communities that work at a distance from the main community, the ways in which such communities participate in scientific practice, and the knowledge-network that such communities build with their main community—in short, the entire topic of how scientific knowledge is produced outside of its Western centers is presently considered to be details of history. Philosophers usually leave such details in the hands of the historians and the sociologists of science, keeping their own analytic energies focused upon the episodes of science at the Euro-American centers. But to view science only through the examples of the central communities who share a long history of collaborative exchanges—and therefore are closely

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9 At this outset, I want to make a clear distinction between the peripheral members of a scientific community and the members who only appear to be peripheral (but are actually integral to its practice), e.g., the graduate students. Not only does graduate study last for a limited number of years (and once completed, those students join the practice as its newly-minted authorities), a community cannot reproduce itself without bringing in such apparently-peripheral members. In contrast, a metropolitan practice does not need to bring in or have contact with any of its peripheral contributors. My thanks to Alan Love and Doug Lewis who raised this important issue, and also helped me to come up with an answer.

10 Note that a discussion of peripheries also draws our attention to the properties of a center, or what epistemic features must be embodied in a central community.

11 Such ‘distance’ could be caused by geographical, social, institutional or even political factors. In general in this study I shall use the term ‘distance’ as a measure of difficulty that a community encounters while seeking to join the practice of another (resource-rich) community. I borrow this usage from that of economics and management, where a firm devises specific strategies in order to overcome its ‘distances’ before entering a new market.
linked and closely located—is to neglect important clues about how science evolves when divisions of cognitive labor among research communities induces both collaboration and competition among them, thereby inducing varying levels of epistemic trust and authority within the practice. In such situations, certain groups are perceived to be more trustworthy than the others, who, in turn, are trusted less—being perceived as either newcomers or outsiders. To neglect asymmetric interactions of this type among scientific communities, which are often observed at the peripheries of scientific knowledge, is to neglect important insights about how science grows outside of its Western contexts through the efforts of the newcomers in novel situations.

1. 2. Peripheral Science and the Non-West

If by peripheral science, we mean simply the science that has taken place outside of the geographical locations of the West, then this topic has received some attention from the historians. Joseph Needham’s magnum opus on the Chinese contributions in science is one obvious example, and more recently, Arun Bala has argued at length about the origins of modern science going back to its Chinese, Arabic and Indian roots (Bala, 2006). But Bala’s contributions were focused solely upon the origins of modern science in 17th-century Europe, adding little or nothing about how science subsequently took shape through the more modern times.

The trajectory of peripheral science (i.e., the science that took place outside of Europe and North America, or alternatively, science that is done by the newcomers and the outsiders) however remains equally important in the subsequent centuries as well. The history of science tells us that during the 19th and 20th century areas outside of Europe had been important centers of scientific activity. Many important contributions in science came from those researchers who worked from those sites as expatriates (a good example of this situation is Ronald Ross and his work on the life-cycle of the malaria parasite which took place in colonial Calcutta). In a series of recent case studies Kapil Raj has shown us that Europe’s advancement in science depended often on important non-Western contributions (Raj, 2007). Analyzing at length the case study of William Jones, the British High Court Judge who worked both in botany and in (Sanskrit) linguistics during his tenure in the 18th-century colonial Calcutta, Raj pointed out that not only did Jones’ collaborators bring him special contributions in expertise (such as highly skilled sketches of indigenous plants) they also contributed relevant theoretical classifications that Jones subsequently adopted in his framework. Thus, the contribution of the non-West did not remain
confined only to providing raw data for research (the most famous example of such raw data is perhaps Darwin’s observation of the finches at the Galapagos Islands) the *epistemic contribution* of the non-West had been important as well in the formation of new knowledge. The same pattern of knowledge-flow can be observed in the 20th-century modern science. In many cases a crucial concept in science arose first within a non-Western scientific community and was taken up from there by the main metropolitan communities. Two prominent examples of this type of reverse knowledge-flow from the peripheries to the center are the notion of the indistinguishability of particles in quantum mechanics, which started with a peripheral scientist named S.N. Bose in colonial Calcutta, and the technique of growing tetanus bacillus in a pure culture, which was done first by Kitasato Shibasaburo in Japan. Thus, non-Western scientists have contributed significantly to the growth of scientific enterprise during the 20th century. Furthermore, scientific communities of transnational types are becoming increasingly important, showing us how scientific practice has changed in our present day. Numerous publications in science bear testimony to the fact that their authors often span trans-national as well as trans-cultural boundaries. And yet, in our philosophical models of scientific knowledge we focus primarily on the science done in Euro-America, thus keeping all philosophical energies fixed solely upon those central communities.

Broadly speaking, this lack of representation of the scientific peripheries in the mainstream models of scientific knowledge produces two kinds of effects. On the one hand, it produces the psychological backdrop against which strident criticisms of science are often voiced in the non-West, questioning the very use and the beneficence of such sciences. Matching those criticisms on the other hand, we encounter an equally eloquent silence about such science in the mainstream models of scientific knowledge. But a discussion of remoteness and peripherality (and its subsequent impact upon scientific knowledge) could be useful for various reasons. Granted that it is easiest to see the property of peripherality in the track-record of a remote and a newcomer research group, yet, peripherality is not a property that can manifest solely among such distant or remote communities. Within the heart of a concentrated center peripherality may manifest itself in numerous shapes and forms and be felt as a distinct effect. It can reveal itself, for example, in the existence of deliberately oppositional groups— such as the feminist

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epistemological positions—or in the case of a scientist with a very divergent research approach, such as Barbara McClintock and her maize plants when her male colleagues focused exclusively upon bacteria (See Keller, 1983). It could also be seen in a scientist like Rachel Carson, who was the first to raise red flags about the use of chemical pesticides like DDT (Carson, 1963). Or, it can be seen in a scientist like Fred Hoyle who found himself gradually pushed out to the status of an outsider as a result of his unorthodox views about the origins of life (for more on this, see Jane Gregory, 2005). Once we grant that the scientific communities often exhibit strongly differential pattern in their interactions, *an inside as well as an outside*—which in turn shapes their epistemic interactions—the importance of the property of peripherality (and its impact on scientific knowledge) becomes manifest, and must be taken up as a topic for philosophical analysis.

Viewed like this, a study of peripherality becomes fundamentally a study of the *differential levels* of trust and epistemic authority which enter into the making of scientific knowledge—a task in which each community participates from its slightly different vantage-points of intellectual authority.\(^{13}\) This in turn leads to a differential access to the social networks that make new knowledge, thereby producing differential research outcomes. Thus, peripherality in science should be understood epistemically—*i.e., in terms of its impact on the making of new knowledge*—rather than simply socially, historically or psychologically. The importance of the peripheral contexts in science arise from the fact that they can draw our attention to the persisting roles of the insiders and the outsiders within scientific communities, and how these roles become important in the task of developing new research programs. Yet, because of our current overemphasis on the central communities in science, such situations in the history of science hardly receive any analysis. A silent assumption, held over perhaps from the days of the logical positivists, consider that all scientific communities are identical epistemic agents—holding more or less the same amount of intellectual authority—and thus nearly-equalized access to the networks of making knowledge. Yet, once we are prepared to set aside this assumption, history of science shows us cases of increasing complexity that clearly call for a new analysis.

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\(^{13}\) Communities that are less authoritative—and thus less trustworthy—could also exist within a central community. Thus, peripherality *is always* a matter of degree, and as I shall show below, can also be overcome in stages, *although never without difficulty.*
Why should one try to see science from the standpoint of its peripheral actors or agents? A prominent motivation for this kind of study is that while the older individualistic and rationalistic models of science might have been suitable for science as it was practiced one hundred years ago, such analysis no longer adequately represent science as it is done in our present day. Since science wields enormous intellectual and social prestige, newcomers and outsiders develop strong motivations for taking up science and organizing themselves into new epistemic communities in their search for greater epistemic power. This process has been observed in the history of 19\textsuperscript{th} century Japan and in 20\textsuperscript{th} century India, and perhaps increasingly, this is going to be true about the rest of the world. \textit{Indeed, many of the future engagements in science may substantially be of this type.} To develop models for such interaction in science—when an established, resource-rich community is joined by a group of newcomers who thereafter seek to become its collaborators—we must pay attention to those episodes of science where the main protagonists came from outside of the Euro-American science, and yet, participated quite successfully in the making of new scientific knowledge. How did scientific knowledge develop within such contexts, and what were the structures of the knowledge-networks that took shape between the two communities? How did such situations influence the subsequent development of research programs undertaken by either side (the central as well as the peripheral)? How did consensus emerge in such situations and how was the cognitive labor divided between two communities? Do we see any particular divisions of cognitive labor predominating in those arrangements? In the sections below, my objective will be to find some answers for these questions, arguing that such reflections link us directly to the well-known controversy about the rational/social dichotomy of science. The property of peripherality in science emerges from its manifestly social character, and from its attendant characteristic of the asymmetric division of cognitive labor. It is this feature of science that produces the outcome that while the veteran practitioners in science gradually assume the status of the established authorities, the newcomers are assigned a relatively peripheral role, thereby creating an important asymmetry between them during their subsequent attempts at collaboration. This asymmetry functions, in turn, as a general gatekeeping mechanism in science, which shapes the interactions among these communities. Peripheral science thus should be seen as an exchange between two groups where a newcomer and a resource-rich group enter in collaboration with one another, \textit{each group intent on realizing their respective goals in the making of new knowledge.}
1.3. How Peripherality Emerges Out of the Social: Science as Social Knowledge

The Kuhnian challenge to the rationality of science motivated philosophers to turn their attentions on the social properties of scientific knowledge. The rational/social divide in science caused the philosophers to focus on the consequences of the social nature of science, especially how the theoretical claims of science could be called reliable despite its prominently social character. For philosophers of classical persuasion—such as Lakatos or Laudan—being ‘social’ means coming close to a state of epistemic anarchy. In such a condition, either the claims of science will be constantly contested or the science produced would be designed to serve only the interests of particular social groups. Indeed, in the works of many of the social constructivists, such as in Bloor or in Latour (1979), it is this latter interpretation of the term ‘social’ that receives the most emphatic support.

This was clearly against the classical aspirations of science, at least as those aspirations were understood by philosophers like Herbert Feigl. Writing in 1950, directly after World War II, Feigl thought that science could provide us with an alternative methodology capable of replacing both metaphysics and religion—in short, a completely new way of life. Through science we could come into possession of a method that gives us an increased understanding of our natural world and the laws that are at work in our universe. Understanding such laws gives us intellectual satisfaction, which is the primary goal of science—any technological power that arises from such inquiry is merely a secondary consequence. Now, if scientific knowledge can no longer be shown to be guaranteed by logical implications, or by inductive relationships, which link evidence and hypothesis, then science can no longer claim to represent the true states of affairs. And thus, the question arises what kind of guarantees can we indeed offer for scientific knowledge. The social nature of science—once admitted—seems to lead directly to relativism.

Philosophers’ response to this problem has been to move the inquiry away onto a different track. To counteract the implications of the arguments of the social constructivists—and Kuhn’s somewhat more moderate view of science—philosophers responded by paying close attention to the nature of the inquiries that produce scientific knowledge (rather than the abstract outcome of those inquiries). If one cannot guarantee the products of science by analyzing their conceptual structures, perhaps some kind of guarantee can be extracted by examining the processes that give rise to those products.

Two such well-known process-based responses come from Helen Longino (1990) and Philip Kitcher (1993), both of whom seek to give us a detailed account of how science can be called objective knowledge despite its deeply social character—how knowledge made by many
hands can yet maintain its valued epistemic status. In the process of laying out those answers both Longino and Kitcher accept certain images of scientific communities—which serve to underpin their general accounts of scientific knowledge. In the sections below, I examine those accounts, seeking to show in what sense they differ from one another. I argue that an implicit reference to peripherality can be read into each of these accounts, and in fact, it can be read quite easily into Kitcher’s account of scientific practice (although it is not an explicit goal of his discussion). A similar reading, I claim, can be projected onto Longino’s account of the interaction among scientific communities, once we are ready to relax certain assumptions in that account. Thus, a recognition of the peripherality in science arises naturally as a consequence of recognizing the manifestly social nature of science. Contrary to the received view that sees peripherality only as an occasional aberration in scientific practice, peripheral occasions and peripheral interactions are quite common in the history of science, once we develop a framework for identifying such properties. Peripherality thus must be recognized as a widely-observed phenomenon in a scientific practice.

1.3.1. An Idealized Scientific Community

As a well-known and idealized model of the social interactions in science there is of course no mention of any peripherality in Longino’s account. Developed over a span of two books (1990; 2002), Longino’s views give us an excellent example of how social epistemology claims to restore the norms of objectivity in science. Viewed in this way, Longino’s position on science is as hopeful as Feigl’s, but it is more complicated, being more subtly nuanced. The science that Longino speaks of arises from the social fabric of the human interactions (and that of their skills)—and not from the unfettered workings of any universal methodology. The point of departure for such science is not nature in general, but only specific objects of inquiry, and this inquiry is shaped by what Longino calls contextual values—personal, social or cultural preferences about how things ought to be (Longino, 1990, p.4). The science that is born of those interactions is therefore not just Science, but science of a particular kind. Carrying the genetic imprint of the society in which it is conceived, such a science is naturally sensitive to the contextualized values that surround its existence. To that extent, Longino’s position bears close similarities with that of the social constructivists. Where she differs from them is in her further claim that while the origins of this science might come from its surroundings, it nevertheless enjoys an open future. Through a mechanism of social cooperation, these contextually-bound groups of social interactions known as science can still achieve much of the same results that
were once expected of Feigl’s universal science, even though the mechanism that now achieves it is a new one.

Longino thus emphatically accepts scientific knowledge to be social— contrasting ‘social’ with ‘individual’— the latter of which forms the conceptual foundation of the classical models of science. Just like any other kind of inquiry, science is a sum total of many individuals making many epistemic judgments. The standard processes of science— such as observation or experiment— lie outside of the purview of a single individual’s mind, being embedded in a wider community. Furthermore, such activities are guided by socially transparent norms. Clearly, interpreting the term ‘social’ in this sense does not mean at all that we will be driven to endorse the more extreme claims that social explanations will always preempt every other kind of explanation. A different brand of explanation— for example, an explanation of science in terms of its implicit cognitive mechanisms— would be just as legitimate. It is true that according to Longino science is surrounded by a number of values— and those values too require clarifications just like anything else. Explanations that shed light on such values serve as a deeper form of clarification for science, placing science within the larger context of other human purposive activities. Once those contextual values have singled out a certain class of parameters— thereby locating science within a specific range of human interests— the internal constitutive values of science, such as accuracy, simplicity, or breadth of inquiry come into play, thereby producing a set of results reliable and acceptable to a larger community. Yet, the epistemic system that consists of these two values— the contextual and the constitutive— can sometimes go wrong. Occasionally, contextual values may interfere with the internal constitutive values of science. Unlike Feigl’s science, which contains its regulative ideal within itself, Longino’s science is a much more realistic affair that could— at least in principle— always goes wrong.

But if science could be this crisis-prone, why call it ‘objective’? Longino responds to this question by sketching a step-by-step procedure—a transparent public machinery for knowledge— that she claims guides our social shaping of science. The constitutive values guide the initiation of new members in the scientific community by making it necessary for them to undergo an

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14 Feigl too describes science as ‘social’ in his 1950 essay. However, Feigl’s ‘social’ is quite different from the ‘social’ of Longino, or from the ‘social’ of the social constructivists. Being ‘social’ for Feigl simply means being intersubjective. Thus, a group of ideal inquirers could, in principle, replicate every inquiry to arrive at the same results. This is no longer possible in Longino’s account of science.

15 Indeed in chap. 6 and 7 of her 1990 book, Longino describes in detail how the contextual values can sometimes take over an inquiry, suggesting that in those cases the underlying machinery of science had gone deeply wrong.
extended period of training. During such a course of training, new members become part of a tradition that they will one day pass onto someone else. Science in this sense cannot be understood in terms of its theory alone, but must involve a number of manifestly social processes, such as designing, planning or conducting experiments (as well as interpreting their results). Even when the results of the experiments are in place, they do not automatically turn into knowledge. Every alleged piece of knowledge has to go through the gate-keeping mechanisms of peer review, peer acceptance (and eventually, peer use) before they could become part of the accepted knowledge-system. None of these extended processes—some of them lasting for several decades—could be completed by a single individual, or even by a single community. Extended interactions of this type require mainstream social processes, involving many individuals and many institutions. By developing a social arrangement that incorporates a shared network and a dialog among many people (and thus among their many values) science protects itself from erratic outside penetration. “The greater the number of views included in a given community, the more likely it is that scientific practice will be objective.” (p.80). The social processes in science may not be perfect, but such processes, according to Longino, function as our only control mechanisms of scientific knowledge.

Implicit into this account lies the thought that by casting our scientific nets as widely as possible, we will be able to reach a state of objectivity. To achieve that state of transformative criticism that for Longino is the equivalent of Feigl’s true methodology, scientific communities must embody four characteristics in their engagements: a) a group of shared standards, b) recognized avenues for criticism, c) response of the community to criticisms as a whole, and lastly, and perhaps most significantly for my purposes, d) an equality of intellectual authority. (Longino, 1990, p. 78). In her final 2002 version, Longino subsequently introduces the notion of a tempered equality, i.e., equality only of those people who hold the appropriate qualifications.

And yet, in both these versions, her ways of looking at scientific communities—and that of their internal epistemic interactions—stays the same. In Longino’s social epistemological account criticisms by (different) members of the community are viewed somehow as transformative. This transformative criticism is ideally employed to examine all background assumptions and the hypotheses that a scientific community uses in its ongoing projects. It is in the presence of such criticisms — and its constant enrichment by the addition of new members—that a community protects itself from stagnation or from the premature closure of a controversy.
Does the addition of such new points of view necessarily lead to an optimal outcome within the scientific communities? More importantly, does a dialogical process at all come into play when two communities—who are quite different from each other in their epistemic resources and in their track record—interact among themselves? Let us remember that the wider scientific community itself consists of a population of varying individuals, each with their differential levels of skill and intellectual authority. Thus, such a group might not be characterized in the first place by any community-wide shared standards or equality of intellectual authority. Instead, it might be dominated by a few prominent members and others who only occasionally participate in their deliberations. Furthermore, interactions among those groups (or individuals) might have begun long before any optimal dialogical arrangement had emerged among them. In other words, when under a certain historical circumstance two given communities begin their exchanges and their interactions with each other, they might not begin at all from a place characterized by the above four criteria.

Thus, the span of transformative criticisms over the practices of science may be actually less in scope than Longino envisages it to be. Indeed, there could be significant exceptions where such processes cannot be fully relied upon to keep the epistemic channels open and flowing. This draws our attention to the fact that the community that Longino describes is already a highly authoritative community—as well as a highly-idealized one. Not only does this community already have considerable resources, it also has a number of full-fledged social processes, e.g., peer review, and those social processes are fully institutionalized in its practice. Since the dialogical processes in science depend directly upon such resources (before they could be maximally effective), such processes might work best only the context of those communities where those social processes already have had a long history, having existed for considerable amount of time.

Outside of this context, however, when two unequal communities come face-to-face without being characterized by the above four factors, the critical exchange and the interaction that take place among them might be quite different in nature. The result of such interactions might not be dialogical at all—indeed, they might simply reinforce the view of the more prominent community in the interaction. In the peripheral context, which will be my main focus

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16 It could be argued, as I shall show in my next section, that differential processes among scientific communities lead, over time, to a state of progress in scientific knowledge. Such progress is not necessarily optimal for all individuals, but it is still an optimal state for the entire community.

17 Those could be the peripheral members who are located within the central community, i.e., someone from an unknown institution, or a community that has historically been excluded from such activities.
in this study, it is frequently observed that criticisms do take place—*but often without any transformative effect*. Instead, the criticisms that a peripheral community encounters are often brief, final, and dismissive. It is not hard to see why historically this is often the case. Not only it is difficult to make contact with a well-known community (or some member of such a community), it is also equally hard to sustain the attention of such a community for a long period of time—*unless the research programs of the two groups have already a considerable overlap*. Thus, a transformative dialog that identifies all the background assumptions (of both sides) and opens them to a community-wide debate take place most plausibly when the two concerned groups already share (some form of) nearly-equalized intellectual authority or at least share a consensus practice. In the absence of such overlaps, a true dialogical exchange between the two might never happen, and the result of an episode of criticism between two such groups might simply be the re-establishment of the point of view of the more established group (and a premature departure of the newcomer). Thus, the dialogical control over the background assumptions of science which is central to Longino’s account, works most effectively only in those situations where the communities in question already share a long history of collaborative exchanges.

Thus, the transformative role of criticism on the practices of science, which is the main insight that we gain out of the social epistemological accounts, depends on one important assumption—that the participants in the process share a certain level ground not affected by any important asymmetries which might bias those outcomes. In the contexts where this is already the case, the philosophical task lies simply in specifying those norms of social interactions that will lead to appropriate optimal results among those actors. In those contexts, where this is not the case, the dialogical process may be an insufficient guarantee against the premature closure of a controversy. Thus, while the social epistemological accounts certainly differ from their analytic predecessors in claiming that the real knower in science is the community, and not the individual,

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18 Longino’s account of course is explicitly designed to address the *internal* workings of an already-authoritative community, and not the interactions among a set of core members and their peripheral counterparts. But it is permissible, still, I think, to stretch the social models of science out of their homeground in order to see what kind of implications they yield in those contexts. All models are partial after all, and are not designed to cover every possible situation.

19 Here I use the word “bias” in the sense it is used in cognitive science, where bias simply indicates ‘a tendency of the system to go in one particular direction’. Thus, a right-handed person demonstrates a bias to respond with her right grip.
they still do not fully incorporate the fact of differential participation among communities, and how surprisingly common such differentiality is in the context of an actual practice of science, especially once we step out of the familiar contexts of the resource-rich, established communities.20

When a critical exchange takes place between two long-standing members of the same community, the result is often a community-wide debate—e.g., the debate between Niels Bohr and Albert Einstein over the Copenhagen interpretation of quantum mechanics, which clearly examined and laid out the background assumptions of either side. This is the mechanism that Longino describes at length in her account. Yet, this is rarely the case between a newly-emerging community (or an individual member of such a community) and an established authority. Such a group—intent on winning the recognition of their more well-known peers—rarely have the benefit of this sort of community-wide reflection; at best their views are known (and commented upon) by a few exemplary authorities.

Thus, the dialogical process that is supposed to inform and moderate the engagements and interactions in science goes differently, depending on the state of a particular scientific community. Historically, there exist many cases of scientific interchange which cannot be viewed as interactions between two such nearly-equalized groups. Instead, what we observe in such contexts is a differential interaction, based upon asymmetric ascriptions of authority among the two (as well as a strong division of cognitive labor between them). Such divisions of labor shape the nature of their subsequent exchanges and influence the outcomes of their interactions. The idealized process of community-wide reflection in Longino’s account, which subjects all background assumptions and the research protocols of science from time to time to a community-wide debate, are indeed rare occurrences in science. A more realistic account of scientific practice, especially in the contexts of a peripheral practice, takes into account the fact that when two very different groups engage with each other, the critical engagement of a new community with a metropolitan one is usually much shorter in length, reflecting their differential vantage points of resources and their different level of intellectual authority.

Thus, the dialogical processes that the social empiricists see as central to optimal outcomes in science, may not dispel all disparity and differentiality that exist within the practice

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20 Longino’s account of course include many normative recommendations about how a resource-rich community could gradually incorporate new viewpoints from the different sections of its community. My goal in making the argument above is simply to make the intuitive point that there are situations in history of science where social epistemological models cannot be applied straightforwardly—they require some revisions or extensions.
of science, even though they may be observed to work quite robustly around a cohesive center. It is from this common, cohesive platform of intellectual authority that the established scientific communities produce most of their research paradigms and their research protocols—and this, in the context of history of science, is usually called a metropolitan community. Yet, a uniform focus on a central community often downplays the different forms of asymmetric interactions that strikingly manifest in other sorts of community interactions, and how knowledge is produced in those contexts.

The centrality of the dialogical processes in science that is emphasized in the social empiricist accounts lead us to believe that the power of dialogical process extends to all scientific interactions—whether those interactions occur within an established community or outside of it; and no matter from whatever vantage-point one participates in science, the same optimal outcome remains open to all. From this, the conclusion naturally seems to follow that there is no community that cannot make knowledge on its own.

No doubt this is intended in the most generous spirit, but this highly idealized picture of scientific interactions (especially on the efficacy of the dialogical process) hides a crucial feature by means of which scientific communities together make new knowledge: their internal division of cognitive labor and the differential ascription of authority that divides this labor among different communities. Such division usually classifies communities into specialized subgroups, each group being ascribed a differential level of trust—each doing perhaps a task that is different from all the rest (Kitcher, p. 304). From this internally divided character of scientific interactions—which is surely part of any sufficiently complex scientific practice—arises the possibility that some of these members (or groups) might be peripheral, that they might contribute to science from their vantage-point of being outsiders. Outsiders like this cannot frequently make knowledge without involving another community, and they rarely count as its full-fledged members. Peripheral scientific practices thus essentially consists in making knowledge as outsiders—usually in collaboration with another established community—which complicates the nature of the epistemic engagements that the two communities subsequently develop between them. Scientists like this frequently hold a different level of intellectual authority and command less trust than their metropolitan colleagues. Thus, they inhabit, metaphorically speaking, a differently-shaped epistemic landscape, which contains an important asymmetry with respect to trust and intellectual authority. This qualifies their power of making new knowledge and that of developing new research programs. And yet, this is a landscape that still permits contributions in

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21 The notion of an epistemic landscape will be discussed in more detail below in 1.5.
scientific knowledge from their respective difficult vantage-points. *Because of this asymmetry, peripheral science works both as a constraint and as a challenge for the peripheral practitioners of science who work from within those contexts.* In order to see how this can happen and how scientific communities can consist of a population of varying individuals, and can still make knowledge that can eventually attains to some form of consensus, let us turn now to Kitcher’s account of the dynamics among scientific communities.

1.4. Internal Differentiation of Cognitive Labor Among Scientific Communities: The Basic Mechanism of Peripherality

1.4.1. A Community Not-so Idealized

In contrast to Longino, in Philip Kitcher’s account, we encounter an analysis of science where scientific communities are seen *as internally divided* from the very beginning. In such a community, we observe cognitive variations among the individuals as well as differential participation among the groups in their daily interactions during the process of research. In such an unequal world, scientists think of to whom to assign their trust from the very beginning. They also often defer to authority in search for greater epistemic gain or even for avoiding errors. Unlike Longino’s highly idealized world of scientific interactions which in fact models the internal interactions of a centrally located, cohesive community, Kitcher presents us with a broader— and considerably muddier— picture of the scientific enterprise, where we see how the development of a cohesive consensus practice emerges out of the multiple rounds of inegalitarian interactions that are carried on among many divergent individual practices. Thus, in Kitcher’s image of the scientific enterprise, individual communities *always* exhibit an internally divided cognitive character (thereby containing the *seed* for epistemic differentiality) and they retain this differentiality (in their community composition) until the very end of a cycle of inquiry. This, Kitcher claims, is one of the prominent features of modern scientific practice, which must be taken into account before any account can be developed about modern science as a progressive enterprise. Internal distribution of efforts among scientific communities, i.e., the organization of cognitive labor among different subcommunities during an episode of research (and the negotiations among those communities about outcomes) become a critical factor in what kind of knowledge could be produced by those communities. Furthermore, this activity shapes the
communication networks that these communities develop with one another, influencing how they endorse and take part in each others’ work.

To make these features intuitively obvious, Kitcher invokes the notion of a consensus practice (as distinct from individual practices). Individuals and communities, Kitcher claims, have practices. A consensus practice, according to Kitcher, includes various types of elements—a language, a set of questions that capture the most significant problems of the field, a set of accepted statements, (or, more broadly speaking, a set of representations), which is tied to a justificatory framework. Frameworks like this may involve further features—such as a set of explanatory schemata, a set of paradigms and authorities and appropriate ways for identifying authorities in new situations, a set of exemplary experiments and observations, as well as a set of methodological exemplars and principles. (Kitcher, 1993, pp. 87-88). Broadly speaking, this is close to what Kuhn calls a paradigm, but a consensus practice in Kitcher’s account is understood more concretely. Consensus practices are embodied in a number of ongoing research programs (in the execution of which a scientist displays his or her level of technical skill and theoretical expertise). Thus, the consensus practice emerges out of many individuals pursuing a number of different research programs. Furthermore, the entire consensus practice is embedded in the bedrock of a social network that has well-understood procedures for distributing trust and authority, thereby providing the necessary guarantees for epistemic outcomes. Naturally, such practices also include a training procedure in which training is imparted by the veterans to the new initiates of a community (Kitcher, p.59).

It is important to see that in Kitcher’s model scientists—despite their being equipped with the same inferential apparatus for making new knowledge—do not function as identical actors in science. Instead, they play out their roles of being epistemic agents very differently. While developing a particular area of research for example, scientific communities divide themselves into many subcommunities, each subcommunity holding perhaps a distinct function from all the rest. Each group is also ascribed a specific level of epistemic authority that may undergo changes during an inquiry. Trust and authority—the two key features that provide warrant for making new knowledge in this newly-diversified situation—are also distributed differentially across this network of communication among different communities. This is a

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22 On p. 85, 1993 Kitcher puts this as follows, “Recognition of our assignment of authority should serve as the beginning of epistemological inquiries.” A little earlier the same point is made on p. 58, “At the beginning of each period, there is a community of scientists, viewed by other scientists and members of the broader public as authoritative on a particular range of issues.”
picture that not only distributes the concrete task of research among different groups, it also distributes their trustworthiness in the form of differential packages.

The insight that communities are always divided into subcommunities during the work of making new knowledge plays a central role in Kitcher’s account, influencing his subsequent arguments about what it means to progress in science at the level of a community. Not only each subcommunity holds a differential assignment of credibility to begin with, at the end of each cycle of knowledge-making those assignments are updated and revised in the light of new information (Kitcher, p.59). This asymmetric allocation of trust and authority, which are implicit in these social arrangements, and the recognition that knowledge sometimes cannot be made without involving others (who command more epistemic authority and thus bring in higher levels of trust) reveals the basic mechanism of peripherality. This also shows us how progress in science can sometimes be possible under unequal and asymmetric conditions and how research communities consisting of such mixed set of actors can still produce optimal outcomes in science. Thus, in Kitcher’s account we see how new research groups, who start with limited opportunities available to them, can still participate in the task of making new knowledge from their differential position, and how from their peripheral vantage-point they can still do science sometimes quite successfully. “All members of the community share certain claims and commitments, but there are subgroups with richer sets of claims and commitments…for certain kinds of issues, particular subcommunities are considered authoritative” (Kitcher, p. 87, italics mine).

Thus, in contrast to the traditional accounts of science, where everyone is seen as being vested with a nearly-equalized levels of intellectual authority — but as I noted above which really describes the interactions of a tightly cohesive, homogeneous group— in Kitcher’s account we find a very different account of interaction among scientific communities. Intellectual authority in this case is vested unequally across the social network of science, and indeed, it is localized only in certain parts of that network. Authority is not available to the community in general, but only through certain selected agents. Since trust and authority are concentrated only in certain parts of the network, the ability to make new knowledge is also similarly locally concentrated, thus making partnerships with established communities essential for peripheral scientists in their attempts of making new scientific knowledge. Trust is primarily assigned to certain key members of the community who are located in specific parts of the network, and in a situation when a

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23 Since in this dissertation I only focus on the mechanisms of peripherality, I shall not discuss in detail what it means to progress in science under such circumstances, except to note in passing when a peripheral scientific community can be called a progressive one.
conflict of claims arises between two scientists, it is the concurrence of those “exemplary authorities” (p.85) that produce a consensus about the state of things. If the dissenting scientist happens to be a peripheral one, with limited access to those core members, his or her chances of being awarded a consensus for his work will be relatively slim.

Thus, in the process of developing a consensus practice from individual practices, we see how a consensus practice in science emerges from a vastly differentiated underside. Such an underside may be frequently populated by the newly-initiated peripheral members of the community. The task of evolving a consensus practice out of the practices of this widely-diversified community is organized by the following kinds of divisions of cognitive labor: i) first, the development of a core consensus24, ii) where the acknowledgement of authority is vested in certain parts of that network by other members, and finally, iii) the existence of a virtual consensus, i.e., willing acceptance of results from a recognized authority.25 Thus, distributions of research efforts are allocated to particular subcommunities (rather than to the community in general), who are thereafter recognized as responsible for and authoritative over particular types of issues, and it is they who alone are seen as entitled to give rise to that virtual consensus. Unlike the classical models of science—including the social epistemological models that I have considered above—the entire community is not seen as a uniform knower. In the micro-structure of scientific research that underpins and holds together this consensus practice, different parts of the scientific community are assigned differential functions, each embodying its own individual levels of trust and authority. When any new knowledge is made by this complex network—such as the development of a new research program—such knowledge often obeys the lines of this internal organization.

Thus, the basic mechanism of peripherality is captured clearly in Kitcher’s account—how a consensus practice grows out of the multiple, individual, and non-asymmetric interactions among scientific communities, and how a certain asymmetry already pervades those arrangements. Can we observe anything similar in Longino’s account of science? Indeed, it is easy to see how peripheral effects can also be observed in Longino’s (homogeneous) epistemic community, in spite its elaborate system of checks and balances. A complex social network such as Longino envisages science to be, can also lead over time to a state where epistemic authority become concentrated in the hands of a few, rather than remaining available to the community in

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24 In Kitcher’s sense core, consensus means those parts of individual practices that are common to all members of the wider community (Kitcher, p.88)

25 “Virtual consensus contains anything that a scientist could reach by following the judgments of a chain of authorities” (Kitcher, p.88).
general. An implicit assumption that Longino makes in her account is that once an epistemic network is assembled, it will keep its character over time, thereby embodying all the crucial four features in the social interactions of its members. Yet, an evolving epistemic network such as science is may do just the opposite. A social network of knowledge-makers, such as modern science now certainly is, may easily develop *differential functions* in its different components, especially when such a network reaches a sufficient level of complexity. Just as in societies and in the world of commerce, once something gets sufficiently complex, science too may start dividing itself among a number of subgroups, *thereby changing its nature from its former homogeneity*. Such localization of epistemic functions will cause levels of trust and authority to vary in the formerly-uniform core community, and indeed, one is inclined to argue that this has been true of modern science for a long time. Indeed, according to Michael Strevens (2006), differential levels of intellectual authority and epistemic trust serve an important function in science—*it helps people to make quick decisions about whose contributions in science are the most important*\(^{26}\), and thus, where to concentrate their own future epistemic efforts. Clearly, in such circumstances, some communities will hold more epistemic prestige (than others), and thereby will wield greater epistemic authority. Concentration of authority along certain nodes of this network will give rise, in time, to a division between the insiders and outsiders in the practice, thus introducing the condition known as peripherality. Thus, by merely increasing the size of the epistemic network as Longino envisages, one may not be able to reach that state of transformative criticism where everyone functions from a position of equality in science. Indeed, many individuals, once they are assembled in the form of a network, may simply assume differential functions—each performing only a specific kind of task in the making of new knowledge. Once this division of labor is combined with differential ascriptions of authority, this can lead, over time, to a core and a periphery formation within the practice of science. In such a scenario, a newly-emerging scientific group, especially a group that participates in the practice of science for the first time, will find itself occupying naturally a peripheral role, and will command little intellectual authority *unless it first decides to cooperate with another*. The hope of having such authority in the near future however may still inspire such communities, permitting them to enter into an epistemic partnership with more established ones *on an unequal footing*. Thus, a scientific community, deployed over an epistemic landscape, will eventually develop a highly differentiated internal organization, both a core and a periphery. Under these conditions, more recognized names will command more attention and will consequently receive more credit than

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\(^{26}\) These decisions of course are always done locally and are relative to the disciplines.
the relative newcomers, *even when both groups collaborate in the same projects of discovery*. Following Robert Merton, this situation in science are called the Matthew Effect.\(^{27}\) In brief, the Matthew Effect means that in science, just as in other walks of life, the rich progressively get richer and the poor poorer. The Matthew Effect is one of those prominent markers that tell us that idealized, egalitarian interactions among scientific communities is far from being the universal norm, and asymmetric interactions quite frequently dominate in the reward structure in science.

This is not to argue that we should get rid of all our idealized models of scientific interactions. Certainly, egalitarian models may very well represent interactions and exchanges within a collaborative core group, with their well-established history of scientific exchanges. But we need to develop a newer set of models to represent those interactions where egalitarian interactions cannot be said to be the norm of the day. This might require us to pay attention to a different set of scientific practitioners to be aware of those processes. Even when we accept that the social models of science seek to provide us with normative guiding ideals, and not with historical outlines, a normative model that *omits* some obvious and a prominent feature of science (such as the Matthew Effect in science), or represents those features only very partially, can be deemed to be in need of further models which represent those features in science more adequately.

### 1.5. The Asymmetric Landscape of Peripheral Science

In Kitcher— and later, in Michael Strevens— this complex asymmetry in science and in its social arrangements (among communities) has become the focus of much formal modeling. Using the constrained maximization approach, game theory, and the language of the Bayesian analysis, both Kitcher and Strevens show us how prominent social organizations in science— and their consequent divisions of cognitive labor— shape the outcomes of scientific practice. In those formal modeling efforts scientists are frequently represented as being motivated by less-than-worthy motives, such as a drive towards personal ambition, or towards intellectual priority, the drive to be the first in the game (rather than a disinterested love for truth). The interesting point that such analyses make is that such epistemologically sullied environments can nevertheless lead

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\(^{27}\) The term Matthew Effect was coined by sociologist Robert Merton (1968) and refers to *Matthew 25:29*. For a lucid discussion of what kind of role Matthew Effect plays in the reward structure of science, see Michael Strevens, “The Role of the Matthew Effect in Science” (2006).
to very productive science, thus allowing peripheral scientific members to make significant contributions to the stock of human knowledge.

In this modeling of research efforts that shows science under its epistemologically sullied conditions, we see how science can be done from its peripheral vantage-points, and how sometimes it can be done from there quite successfully. We also see how peripherality enters the practice of science via the complex interactions that characterize exchanges among scientific communities. The existence of the peripheral members in science arises from two factors that I have just mentioned in the above: first, a prominent division of cognitive labor (as the natural consequence of the cognitive complexity of science), and secondly, a differentiality of epistemic authority among the communities. A division of labor creates many subcommunities among the scientists who hold thereafter specialized functions in science. While it is true that these subcommunities are not necessarily peripheral, peripheral members in science often begin their career as a specialized kind of such subcommunities (e.g., from a particular place), who do not yet hold a sufficient amount of epistemic authority due to their absence of a track record within that practice.

More importantly, there is an asymmetrical distribution of trust and authority among those subcommunities—some are trusted more than the others— which becomes a marker for scientists to make quick judgments about whose contributions are to be deemed as the most important in science. Naturally, this distribution vests the most prominent and the most established veterans with most authority and the most epistemic resources. A new approach for visualizing this type of asymmetric distribution of cognitive resource and labor has been developed recently by Weisberg and Muldoon (2009), where Weisberg argues that scientists organize themselves—broadly speaking—into two kinds of groups: mavericks and followers, i.e., those who always lead, vs. those who mostly follow. Upon an epistemic landscape that represents all the significant problems in science, these two groups play out their games of making new knowledge with vastly different kinds of research strategies. Over time these two roles give rise to a differential pattern on the epistemic landscape (this is the sum total of all the significant problems in science). Mavericks, commanding more resources, set themselves to concentrate upon the riskier research programs, leaving the followers to work on those areas

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28 According to Weisberg’s formal representation of the epistemic situation in science, mavericks are the agents who do not follow others and always set off in a new direction. In contrast, the followers are those who always move in where somebody else is already at work. Thus, mavericks follow a high-risk but also high-gain strategy, and the followers typically opt for a safer strategy. Interestingly, since followers will usually track successful mavericks, it turns out that a mixed population of the two is the most optimal combination for new scientific research.
where a number of people are already previously engaged. Dividing their intellectual labors very differently, the two kinds of research groups (or two individual scientists as a limiting case) pursue their significant research problems with very distinct cognitive styles, developing essentially different sorts of research programs. In this approach, science is visualized as a broad landscape containing “hills” of significant truths on which different communities converge during their work on different problems. Thus, scientific communities work on the epistemic landscape of science in differential clusters, and a formal modeling of how those clusters evolve over time allows us to see how the social organization of scientific communities can itself become a factor in the making of knowledge. The epistemic landscape of science can thus become biased enough due to the existence of a microstructure of scientific research, which arises naturally as a function of the social interactions in science.

Using this simple intuitive image of scientific communities organizing themselves into unequal clusters upon an epistemic landscape, it is now easy to see how a peripheral scientific community—which, despite its constrained circumstances, decides to contribute in science—will find itself faced with an important internal organization in science that arises due to the existence of a microstructure in scientific research—the division among the insiders and the outsiders in science. This division arises from the fact that while the existing well-established members of the community command more epistemic resources—and thus more trust—the outsiders command less, at least initially, being relative newcomers. The result of this division of labor will be that each of these two groups will contribute differentially in science and will perhaps make use of different sets of research strategies. For the peripheral practitioners, this internal organization of scientific knowledge serves as their main platform for further research, setting up their cognitive environments and shaping their subsequent efforts at knowledge-making. Thus, the basic mechanism of peripherality is linked with the micro-structure of scientific research, shaping how a peripheral scientist enters the game of making new knowledge in science. Clearly, under the circumstances he or she will have to operate under a certain measure of intellectual asymmetry, at least at the beginning, and yet, there is a clear chance that such a scientist may yet be able to win a considerable degree of success. This combination of hope and risk creates a challenging cognitive environment for such peripheral scientists, creating an overlap between the two other strategies, discussed by Kitcher and Weisberg. Contrary to the general expectations, peripheral scientists often seek to act in the role of mavericks, hoping to be the first ones to make a new discovery (and thereby start a new beginning). Peripheral scientists are therefore not just followers: innovation often comes from the peripheral mavericks who have low authority and work from the fringes of scientific knowledge.
1.6. A Modified Account of Trust, Consensus and Conflict: How intellectual Authority is Assigned in Peripheral science

In the formal modeling strategies used by Kitcher, Strevens and Weisberg, different research groups divide their cognitive labors differently (as well as differentially), each selecting a research strategy appropriate to its own overall epistemic goals. Such strategic organizations of cognitive labor shows us how the micro-structure of scientific research shapes the knowledge made by this structure, once it is in place. Furthermore, this microstructure functions as a general gatekeeping mechanism in science, partially restricting the newcomers who enter science with their ambition of becoming established authorities, often giving rise to situations like the Matthew Effect.

In contrast to Kitcher and Weisberg who develop their views by using a purely formal approach, I want to call attention to data from the history of peripheral science, the context of various non-Western scientific communities, especially when such communities were near their time of their origin. If, as I have argued, a prominent division of labor in peripheral science exists in the form of a division of labor among the insiders and the outsiders of science, and if trust and authority are asymmetrically concentrated in the hands of the insiders, in what way does a peripheral scientist participate in the development of new research programs? How does a peripheral scientist gain a measure of intellectual authority and produce new knowledge in science from his or her vantage point as an outsider? From whence does s/he obtain the necessary inputs of trust and authority in order to reach a consensus in science that allow a peripheral scientist to establish his or her claims to the newly-produced knowledge?

The circumstance of being a peripheral scientist comes from being a researcher who works from outside of the main community, but who nonetheless contributes —sometimes quite significantly— to the central research programs of that community, hoping to make it someday his or her own. Thus, such a scientist necessarily strives to form partnerships with his/her peers within the main community, seeking to invite their epistemic support, collaboration, and input. A typical peripheral scientist, who works at a distance from his or her metropolitan center of scientific research, thus seeks to engage the attention of the scientific community at the center by first solving a problem for them. His/her extension of a problem derived from an existing
metropolitan program serves as his/her first occasion of research, and his/her solution to a current problem creates his/her *first interface* with a central community.  

How will trust and consensus be ascribed in those situations when two communities work together on a common research program from the vantage point of their *differential levels* of trust and intellectual authority, which remain unequal despite their ongoing epistemic partnership? Levels of intellectual authority are tied to questions of trust, which, in turn, are tied to the training of the scientists. In most cases, peripheral scientists are trained by one of the two main processes—either they have visited a metropolitan center during their student years or they have received training by means of an indigenously available education system that was derived in part from the education system of a metropolis. In most cases such exportation was explicitly undertaken with the goal of a ‘civilizing mission’. Of all the sources of authority mentioned in Kitcher (1993, chap 1)—sharing a common stock of past knowledge, being trained by the veterans of the same community, and peer endorsement, peripheral scientists are mostly excluded from the first two processes, thus being exclusively dependent upon the third mechanism.

Under the circumstances then, it follows that the contribution of a peripheral scientist will be welcomed the most when a metropolitan center of research has encountered an impasse—which might make them more sensitive to a peripheral contribution. This situation encourages, in turn, a peripheral scientist to attempt to make such a contribution. Indeed, it is on such occasions that a peripheral scientist gets to play the role of a maverick, giving him or her unique opportunity *to gain a first foothold* within the process of scientific research. However, gaining that first foothold does not imply that he or she enjoys a permanent place subsequently within that community or even shares the same level of epistemic authority as any of its other members for often, once his/her solutions are accepted, a different phase begins for such scientists. Naturally, a scientist who has once successfully played the role of a maverick, will want to expand his or her research efforts, moving on to the next and more ambitious phase of elaborating a research program. In this process, she or he gains for himself/herself (and perhaps for his/her research group) the beginning of an independent research tradition, and thus, a measure of intellectual authority. *An extension of such research efforts however calls for a*

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29 The reader will notice that from my earlier talk about peripheral scientific communities here I have switched over to the talk of an individual peripheral scientist. Such an individual peripheral scientist may be seen as a pioneer, and historically, indeed such pioneers have often marked the beginning of a new peripheral community.

30 The Japanese historical practice of hiring foreign experts for a temporary period of time might be seen as a third kind of mechanism.
substantially higher measure of intellectual authority. Often such extensions cannot be accomplished without introducing new styles of research or starting an altogether new paradigm. Therefore, it is precisely at this juncture that a peripheral scientist finds her/himself beleaguered by the asymmetric levels of trust that separates him or her from his other metropolitan colleagues. Thus, the epistemic partnerships that a peripheral scientist forms with his/her metropolitan peers at different centers can at best be described as an inegalitarian partnership.

It is this possibility— and the experience—of doing science under this form of asymmetric, inegalitarian arrangement that allows such newcomers to join science and leave a track record, and yet in the end, it also substantially constrains their effectiveness and (sometimes) their productivity. Do we observe this process anywhere in the history of science? Do we see examples of new knowledge in science being made by a peripheral newcomer, who was thereafter unable to take it to the next stage, and yet left an abiding contribution in science? In the case studies that I shall present below (in Part II), this will be the theme I shall explore at length, keeping in view the history of the peripheral science in India as my main focus of study. Here however, I want to make a brief mention of one of those studies, in order to highlight how a brief entry by a peripheral scientist can become an important occasion in science.

In 1924, S. N. Bose, provided the first non-classical, rigorous proof of Planck’s law of blackbody radiation. By producing a proof that was free from all classical assumptions about light as a wave, Bose provided a theoretical ground that established Einstein’s light-quantum hypothesis beyond all reasonable doubt. The heart of his proof lay in his modest 4-page paper, titled “Planck’s Law and the Light-quantum Hypothesis”, which provided this proof by means of a simple but remarkable move. In the course of his ‘simple’ proof of Planck’s law, Bose simply assumed that the light-quanta in a blackbody radiation field are indistinguishable particles. This implicit assumption (stated nowhere explicitly in his paper) led him to modify Boltzmann’s statistics in a novel manner. By mentally modeling the radiation field into a grid of phase spaces, Bose mentally transformed the field into a collection of phase space cells within which the light quanta may be accommodated. He then calculated the equilibrium of this (revised) system by simply calculating in how many ways those cells could be populated by indistinguishable particles, instead of counting how many individual particles could occupy a given number of cells (the old Boltzmannian statistics). This neat reversal introduced what is now called the indistinguishability hypothesis in quantum mechanics, thus laying the foundation of a new kind of statistics. With Einstein’s endorsement of Bose’s novel counting procedure and his (Einstein’s) quick application of this procedure to the theory of monatomic gases, predicting what came to be known as the Bose-Einstein condensation, a new quantum statistics was born, and a quantum
theory based on this indistinguishability hypothesis ushered in a revolution in physics. Yet, astonishingly so it seems, Bose did not take any part in this larger transformation of physics, in spite of producing one of its most fundamental early results. In fact, shortly after 1926, he moved altogether away from radiation theory, thereby raising the suspicion among his European colleagues that he had become a drop-out from science. Why did Bose get initially involved—and later withdrew—from a topic that once so intensely interested him?

Bose’s success in the first paper depended primarily upon the endorsement that he succeeded in gaining from Einstein, who in this case provided the necessary intellectual authority in the form of peer endorsement. But once Bose wrote his second paper (on the same topic), calling into question some of the basic presuppositions in Einstein’s framework, he was faced with a very different situation: Einstein’s withdrawal of the same authority. Faced with this impasse, Bose abandoned the project and moved altogether away from radiation theory, thereby losing his priority-claim over the ideas of new quantum theory. Thus, the brief collaboration between Bose and Einstein indeed exemplifies the collaboration between an aristocrat and a prole described in Kitcher (Kitcher, 1993, p.385). While from his prole point of view Bose was able to offer a creative solution to a long-standing metropolitan problem, as a prole, he barely commanded the required epistemic authority that he would have needed to carry out his expanded insight—an entirely new framework for explaining radiation. Note that even though in his first paper, Bose took up a new point of view, he was still elaborating a framework that had already inherited from an aristocrat. By practicing Western science from his peripheral location of colonial India, Bose did enter the scene of western science successfully as an outsider. But the limits of his science thereafter coincided with the limitations of the role that he could have played under those circumstances.

And yet, in hindsight, today we see quite clearly that Bose’s lonely epistemic efforts, including his final abandoned paper, did create something for the future. Within the metropolitan community, ideas similar to him came back within two years in the form of a new movement, now known as the new quantum theory. Within his own home community, following his footsteps in basic scientific research, came K.S. Krishnan and others, thus setting up a new trend.

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31 For a detailed account of Bose’s role in the formulation of Bose-Einstein statistics, see Chapter 5 below.

32 Kitcher uses these two terms as shorter versions of “aristocrats” and “proletariat”—thus clearly marking which side carries more epistemic authority. I have preferred to use the terms ‘metropolitan’ vs. ‘peripheral’ scientists to capture the same asymmetry.
in basic research in science in India, quite distinct from the field-based research practices developed by most colonial officials.

1.7. Conclusion: Peripherality in Scientific Practice

“Scientific inquiry is a collaborative human activity” (Longino, 1990, p.17). Yet, as we have seen in the above, those collaborative activities need not always be egalitarian in character. For a long time, scientific interactions had been thought of mainly on the model of egalitarian interactions, thus pushing all other kinds of scientific interactions out of view. While models of idealized interactions in science are perfectly acceptable in their own place, more realistic—and perhaps more inclusive—interactions among scientific communities should be included in those social models of science. Our goal therefore should be to develop models for the asymmetric interactions in science—quite common at the periphery—which might appear inequitable on the surface, but yet contain in them the potential to give rise to a productive and functional science, often by initiating new research traditions in a new national and cultural setting. From such models of epistemic interactions we can gain new insights about how scientific practice unfolds in a novel peripheral setting, especially when those settings involve a mixed community—consisting of both the newcomers as well as the established authorities in science. Analysis of such processes require of course that we pay attention to the key features of these interactions, e.g., how scientific communities reach a state of consensus, what kind of intellectual authority do they enjoy as makers of new knowledge, and how that authority could be enhanced or (conversely) reduced. In view of my arguments in the above, I submit that developing models for this kind of asymmetric interactions in science which notes the non-uniformity of epistemic interactions among scientific communities, and the possibility of a warped epistemic landscape—i.e., a landscape that recognizes a distinction between the centers and the peripheries—can be a worthwhile project for the philosophers of science. Thus, all collaboration between scientific communities is not, necessarily, stories of equal collaboration. This, furthermore, draws our attention to the fact that intellectual authority in a particular scientific enterprise is something that is gained over time—it is not a given resource that a community can start with.\[33\]

\[33\] While objectivity remains the central concern for established scientific communities, the most pressing issue for a peripheral community is progress or the achievement of a consensus—whether this is by means of publication, solving enough problems, or by being accepted by their more well-known metropolitan counterparts. Since communities at the center already enjoy a given measure of epistemic authority—they naturally focus on objectivity. The communities at the periphery however need to achieve (some form of) epistemic authority—hence their emphasis on progress (and consensus).
What I argued in the above shows us that knowledge can often be made under complex, inegalitarian circumstances, and that in this complex network of making new knowledge, various forms of peripherality—geographical, psychological, institutional—may often be at play. Looking at science in this way, with the model of an epistemic landscape as our tool of analysis, we see that during the phases of development of a new research group, unequal collaboration among communities may persist quite frequently, thereby giving rise to a strongly qualified and internally divided science. From the perspective of a newcomer in science therefore, this existing internal organization of scientific research implies that a line will be drawn between the insiders and the outsiders in science, allowing more credit— and thus more authority—\textit{to be concentrated in the hands of the well-established insiders}. This asymmetric arrangement during the process of making new knowledge further implies that the knowledge-networks will work more smoothly when they are initiated by a center (perhaps in collaboration with several peripheries), and less smoothly when a peripheral community attempts to make new knowledge relying on the collaboration of a more privileged center. \textit{It is from this asymmetry that the possibility and the experience of being a peripheral scientist arises}. Because of this existing asymmetry— which acts as the standing environment for such scientists—peripheral scientists and peripheral scientific communities often encounter a difficult and uphill battle of negotiation, especially during their formative, pioneering years when they require recognition and consensus for their work. Difficulties like this may result into several failed research programs, especially when those programs are initiated first by peripheral communities. Bose’s new ideas on blackbody radiation for example, received only qualified acceptance after a very successful first beginning due to Einstein’s strongly qualified reception of Bose’s later, more ambitious, ideas.

Should doing science in a peripheral context then be thought of as a hopeless enterprise? In spite of my explicit formulation of the difficulties that a peripheral scientist encounter along his or her way, I do not draw this conclusion. Indeed, in the rest of this dissertation, I shall argue that, despite its difficulties, peripheral science is \textit{essentially a project with a future}. Despite the strong asymmetry that often pervades such scientific encounters—especially during its pioneering stages—I consider peripheral science to be a practice that is pregnant with possibilities.
First, such science allows newcomers (and new communities) to gain their first foothold in scientific research and introduce their ideas into the main practice of science, thereby enriching the practice in general. Hence, the fringe areas of research that are populated by the peripheral members of science should be viewed as a region of possibilities and not as a region of passivity. It is in the existence of such fringe areas that we see how science gains contributions from the new members, thereby ultimately avoiding the stagnation and the rigid conservatism of a core group.

Why use the history of non-Western science in order to illustrate the processes of peripherality? Here I shall give the reply that Plato long ago gave in the Republic: it is easiest to observe a process where it is particularly prominent. In other words, since scientists from the non-West have often been one of the most prominent examples of the peripheral members in science— embodying both psychological as well as institutional peripherality in their persons—it is easiest to study the properties of peripherality in those non-Western contexts.

In conclusion then, what are my justifications for choosing to reflect on peripheral science? As it turns out, the study of such science can help us in answering some complex questions. For example, in recent decades, a large body of work in science studies (and in the history of science) has been devoted to the goal of showing how practices of science have often given rise to exploitative, derivative and imitative ventures of research within the non-Western contexts, devoted mostly to the commercial and the practical ends of various colonial empires (e.g., see Kavita Philip, 2004). Such analysis of scientific practice sets up the background of an important controversy that has taken place in the non-West in the last twenty years about the aims of science. Stated very briefly, this debate asked the question whether societies in the non-West should switch over to some form of alternative science, thus recovering the original projects of their societies. Exactly what can the projects of modern science contribute for such non-Western contexts? Various scholars in Indian science studies, e.g., Ashish Nandy and Shiv Visvanathan, have invoked powerful critiques of modern science, showing us in great detail how science has adversely affected the culture and life in non-Western societies (See Nandy, 1980; Visvanathan, 1997). In their analysis, science has often been indicted as a quintessential form of Western product as well as a hegemonic body of knowledge that imposes itself upon other bodies of (indigenous) knowledge, driving them out of existence. Thus, such debate urges us to draw the conclusion that since peripheral science can only pick up the minor themes of metropolitan science—historically, it has largely been a derivative activity—it cannot possibly fulfill the goals of enlightenment that science is supposed to bring to the non-West. Under the
circumstances then, the best policy for such societies would be abandon and defy the hegemony of scientific knowledge (Ubéroi, 1984).

What I want to conclude from my discussion above is that a proper analysis of peripheral science can show us a way out of this impasse, by showing peripheral science in a new light. It can show us how those scientific episodes that look constrained and peripheral on the surface, may nevertheless constitute an important beginning for a newly-emerging research community, who, by doing this work, lays the foundations of an independent, self-sustaining tradition in the midst of much epistemic asymmetry. A proper account of peripheral science should thus lead to a *middle view* about such science—steering clear of both positions that see science as enlightenment or, conversely, that see it as a submission to an alien body of knowledge. The problem however is that currently we do not possess any theoretical language with which to articulate such an account. Therefore, in the rest of this dissertation, I shall try to put together a theoretical language for such science (and to demonstrate it with some case studies) within whose purview such science can be further explored and evaluated. However, such exercises will require that we shift our attention away from the current dominant models of science that see science mostly as a Western enterprise. It will also require that we examine those contexts of science where the protagonists of science came from the non-Western peripheries, especially from the colonies of the former European powers. An analysis of such science from the standpoint of those (peripheral) protagonists will show us how the cognitive agency of these researchers create the beginnings of a new scientific tradition, and how they solve the problem of their immediate constraints during an ongoing program of research. A discussion of those constraints—and of their achievements in the face of those constraints—allow us to form a new kind of account of peripheral science. Like all human efforts, peripheral science is the response of a society that encounters a new epistemic product and engages with it in order to make this product its own. The story of peripheral science is thus the story of an exchange between two groups, who seek to initiate a flow of knowledge, skills and ideas in the midst of many inegalitarian conditions.

The lack of a philosophical model for this kind of science makes us fall back on the default model, labeling such science as cases of diffusion. This is what underpins recent controversies such as the controversy about alternative science. Since the goal of this dissertation is to reject that default model—*and* to provide a reasonable substitute in its place—I shall begin my next chapter by briefly summing up why science is often rejected in the contexts of the non-

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34 See J. Lourdusamy, 2004. “Science and National Consciousness in Bengal” for a number of such case studies in the Indian context.
West and what sort of arguments are used to provide the philosophical background for this kind of rejection. Having drawn a sketch of this debate, I shall next propose a cognitive-theoretical framework for exploring peripheral science where I shall argue that such science can be seen as a trading zone between two unequal communities. Thus, peripheral science is a significant epistemic activity despite of its profound asymmetries because it helps to set up a new network of knowledge between two communities who formerly had few epistemic transactions with one another. Thus, despite its inegalitarian beginnings, peripheral science contains the potential to bring about a more egalitarian future, if only sufficiently pursued. Thus, peripheral science is more than a minor epistemic activity, but in order to see this, we will need a new account of peripheral science, to which task I now turn.
Chapter 2: A Middle View of Peripheral Science: Proposing a Cognitive-Philosophical-Historical (CPH) Analysis of Science at a Periphery

2.1. An Alternative to Universal Western Science?

Is science an absolute good for the non-West? In other words, should a non-Western society seek to cultivate science or devote its resources to building an indigenous scientific community? It turns out that an answer to this question could be an exceptionally difficult one.

This difficulty was articulated explicitly in a debate that took place during the 1980’s in a public forum in India. An Indian left-wing weekly called Mainstream published a short five-page article that dealt with a nebulous issue called ‘scientific temper’. The article generated, surprisingly, a flood of responses and counter-responses that soon launched an Indian science war, pre-dating the science war in the West by almost a decade. The objective in this war was to reflect on the aims of science, its potential, and its applicability for a non-Western society like India. The issue around which this debate converged could be summarized in a single statement: Can science, which Kuhn had claimed to be a paradigm-bound practice, achieve these above goals for a non-Western society like India? Or, is it a Trojan horse handed over by the colonizers to which the post-colonial state clings simply by mistake? After more than thirty years of independence since 1947, the issues about the aims of science – and its underlying assumptions of a universal rationality and (potential) enlightenment for the non-West—had became controversial enough so to start an acrimonious exchange on the very notion of science.

The background of this debate went back of course to the long history of European expansion and to the resulting colonialism that had introduced a new knowledge-system and a system of education into the sub-continent which remained a colony of the British Empire until 1947. A new epistemology called modern Western science had gained ground in the sub-continent during this time as a part of the general “civilizing mission”. Riding on the backs of the different political and commercial objectives of the Empire, this newly-imported epistemology quickly superseded all the other local knowledge-systems, causing those older epistemic structures to die away within a short period of time.35 While the ‘backwardness’ of those local systems could be evaluated by using the standards of “modern” science, science itself claimed

35 This phenomenon has been dubbed recently as the ‘sudden death of Sanskrit Knowledge’. See Sudipta Kaviraj on this issue, ‘The sudden death of Sanskrit Knowledge.’ Journal of Indian Philosophy, 33. 2005. pp. 119-142.
to be a perennially progressive form of inquiry that might have been anticipated by other human societies, but never before quite realized in history—except in the West.

The West thus became the unique point of origin where a new epistemology called modern science first took its concrete shape. All other societies were thus naturally expected to emulate this Western ideal. The key mechanism that explained the movement of science from its Western home to its non-Western locations was called diffusion. Thus, scientific knowledge spreads from a number of Western metropolises to their different, respective colonies, which stand, figuratively speaking, under West’s umbrella of expert knowledge. In 1967, George Basalla provided a three-stage model of this process of transmission from the European scientific center(s) to their peripheries (Basalla, 1967). Basalla’s model, which soon received strong criticism because of its extreme Eurocentricity, spawned a literature in responses and replies but nevertheless articulated a sufficiently historically plausible framework within which, given enough time, all non-Western societies could be accommodated within a Western-style scientific framework. The de-colonization of Asia and Africa during the 1950’s and 1960’s did not end this basic project. Instead, it only changed the hands of those who would be directing this overall process of integration. The plans of this integration into a Western-style scientific rationality would now be carried out by the newly-formed post-colonial societies themselves (rather than by their former colonial masters).

The older theme of ‘civilizing mission’ thus merged smoothly within the modern goals of ‘development’. After its independence in 1947, the newly-formed Indian nation-state embraced a massive program of state-sponsored science, using the language of its colonial medium of transmission: the English language. The close partnership between science and the state received a new name—development. Writing in 1942, the first Prime Minister of India, Jawaharlal Nehru, celebrated his vision of a scientific rationality for India in the following kind of language:

“the adventurous and yet, critical temper of science, the search for new truth and knowledge, the refusal to accept anything without testing and trial, the capacity to change previous conclusions in the face of new evidence, the reliance on observed fact and not on preconceived theory, the hard discipline of the mind…”

36 See the critical response on this issue by Raina and Habib (2004) and Visvanathan (1997).

Needless to say that Nehru considered science to be an essential ingredient of a modern society that he was hoping India would soon develop into.

Yet, it is precisely this hope—and later, hope disappointed—that sparked off the debate about scientific temper in the pages of the *Mainstream*. Notwithstanding its optimistic name, the goals of development (and its attendant background of a scientific rationality) had failed—repeatedly—to bring about expected results. Guided apparently by the norms of a universal scientific rationality, the projects of development ran into numerous problems during its implementation years—whether they were in medicine, agriculture, or in the building of those large scale dams that promised abundant supply of electricity, but brought instead—as their inevitable consequence—massive displacement of the tribal populations by flooding their homelands. Even when such projects brought immediate success, such as the Green Revolution in the 1960’s which brought food security to India, the environmental price for such ‘successes’ was quite steep. The consequences of development, and the resulting alienation of the elites of the society from their own common people, made it increasingly difficult to believe that an imported scientific rationality, borrowed wholesale from the West, would be the best instrument for enlightenment (and progress) of a non-Western society.

This lackluster performance of the development ideals was reinforced by another element during the 1980’s—a changed image of science was slowly gaining ground within the Western academia. Disciplines such as the history of science or science studies, were ushering in a radical transformation of the image of science, challenging its widely-accepted, traditional properties. The long tradition of explaining science in the image of a wholly rational method was being questioned by the newly-emerging disciplines like history of science and sociology of science, which were laying bare the historical antecedents of many well-known episodes of modern science. Their aim was to show how modern science emerged gradually out of the older discipline called natural philosophy, and how key figures within it, such as Galileo, Kepler, Descartes and Newton, had worked to develop their solutions to chosen problems. These empirical investigations returned a number of results very different from what the philosophers had been teaching all along. Thomas Kuhn’s image of science as a succession of incommensurable paradigms suggested that the question of evaluation of evidence in science—a process that Nehru had thought was quite identical with “the capacity to change previous conclusions in the face of new evidence”—frequently depended upon many other factors that had little to do with evidence. 38 Even though Kuhn himself nowhere suggested that science, thus conceived, would be

38 Though not as famous as Kuhn, other philosophers such as N.R. Hanson and Ludwik Fleck, also highlighted similar views.
a non-rational process, many sociologists and anthropologists of science were quick to see this implication into his account. The rising tide of post-modernism, coupled with results of the historical and sociological investigations into scientific knowledge, made it increasingly difficult to believe in a scientific rationality and its attendant notion of a cumulative scientific progress. 

*But if scientific knowledge cannot be shown to be either rational or progressive, what sense could there be in converting to it en masse?*

Thus, faced with the impasse of the development ideals in ecology, medicine and human resources, coupled with a radically new perception of science emerging out of the various departments of history and sociology of science in different Western universities, scholars within Indian science studies began to look for an *alternative ideal* that would replace the fading shine of scientific rationality. Forging ties of mutual interest with scholars in the West, such as Sandra Harding and Bruno Latour, they asked the question: could there be an alternative science that would reject—or, at least provincialize— the universal claims of the Western scientific method (Uberoi, 1984)?

### 2.2. Science as Western Hegemony or as A Source of Alienation

The notion of an alternative science was thus born under the twin impacts of post-modernism and a perception of science as a march of incommensurable paradigms. Entire families of philosophical notions such as ‘progress’ or ‘universal scientific method’ were brought under scrutiny and was subjected to historical investigations. No longer could a view like Nehru’s pass without criticism. If there exists other, alternative, conceptions of science (i.e., if the Western scientific method were not truly universal), then perhaps it would be possible to articulate different conceptions of development, thus freeing non-Western societies from the general burden of emulating the West. The resulting philosophical battle over the notion of an alternative science was thus fought in a series of several key texts during the next twenty years by a number of scholars on either side. My examination of peripheral science will begin by (briefly) sketching this controversy— considering both its recommendations and its conceptual underpinnings. At the end of my sketch, I shall suggest that perhaps a new analysis of peripheral scientific practice would be better suited to address the potentiality of peripheral science in the non-West—one that takes a middle view between the two extremes endorsed by this debate.

Let me now sketch briefly the two major positions each side of the controversy. The scholars within the Indian science studies community— many of whom work from various American and European universities—find their natural allies in Sandra Harding and Bruno
In her 1998 book, Harding had argued for a multicultural ontology of sciences that will replace the one true science produced by the West (Harding, 1993; 1998). Harding’s proposal thus mirrored what many Indian scholars consider an essential move in the above game—to defy the European monopoly on a scientific method (Uberoi, 1984). While formulating his critique of scientific rationality in The Other Mind of Europe, Uberoi suggested that an essential move for all post-colonial societies would be to begin with a defiance of Western scientific method and to supplement this defiance by a thorough search of all other principles that have been used to organize indigenous knowledge, whether they exist inside or outside of the state-sponsored university systems.

Two critical voices that reject entirely the enlightenment potential of modern Western science come from well-known post-colonial scholars in India, such as Shiv Visvanathan and Ashish Nandy. In his article in Science (3rd April, 1998), Visvanathan had called India the great compost heap of many defeated ideas—foremost among them, modern science and the English language. Visvanathan’s earlier book, The Carnival of Science (1997) analyzed the operations of what he called a laboratory state: a tight state-science partnership that gradually compels all traditional lifestyles either to submit or to disappear, treating populations and environments as if they were but laboratory specimens. The incriminating evidence of such state-science nexus in India was presented by Visvanathan from the three major sites of development: the large-scale building of dams, scientific agriculture, and modern medicine. Since in all three areas, science, according to Visvanathan, uses routine forms of domination and violence, secular scientific rationality or modernity becomes, for Visvanathan, an essentially vivisectionist project. Similarly, Ashish Nandy, a psychoanalyst-turned-sociologist of science—and only slightly less radical than Visvanathan—rejects modern scientific rationality on the ground of the right of the survival of different cultures (Nandy, 1998). The survival of other cultures with their distinctive projects requires that the secular grand narrative of modernity be rejected, or at least be given no more space than one single voice among the multitude. Much like Gandhi, what Nandy really rejects is the setting up of the West as the principal psychological category, to which then everything else is forced to measure up, only to be declared as the non-West.

Nandy’s rejection of scientific rationality is founded upon two main arguments. Nandy finds two fundamental features within the projects of scientific rationality that he considers as its essential defining features: isolation, i.e., the ability to “pursue an idea without being burdened by
feelings” (Nandy, 1998, p. 278), and secondly, the alienating and the self-defeating nature of the colonial relationship that served as the main vehicle for introducing scientific knowledge into the various contexts of the non-west. Following Sigmund Freud, Nandy holds ‘isolation’ to be an important property of modernity, for the ability to ‘isolate’ or ‘observe something from a distance’, without allowing oneself to be burdened by any emotions about it, enables us to pursue a consistent logical trajectory about things without being disturbed by the fate of its targets (especially if such a policy holds high levels of future success). This interpretation of scientific rationality clearly licenses various forms of normalization that can easily mask a strong degree of paternalism or even violence in the name of progress. Modern science, according to Nandy, is just such a highly-organized isolation system, and hence it is dangerous for any non-Western societies that import it from the outside in the hope of achieving quick, immediate ‘progress’.

What Nandy thus rejects is the Popperian ideal of science as a consistently progressive enterprise where scientific rationality connotes the ultimate stages of an evolutionary enterprise, in spite of its historically piecemeal character. It is thus presupposed that such rationality can bring about— although always in a piecemeal way—a more enlightened state of the world. Rejecting entirely this utopian dream, Nandy holds that the goal of scientific knowledge can only be the cultivation of many paradigms or multiple discourses that must necessarily exist side by side. According to Kuhn, this would have been labeled as a state of immaturity in the sciences, but like Feyerabend— his more well-known Western counterpart— Nandy considers multiplicity of perspectives to be good news for societies since it increases our options and provides protection against the severe, coercive experiences of normalization that are often the fate of non-Western societies speedily converting to the ideals of the West (Nandy, 1998, p. 336). In his somewhat more well-known critiques of science, Feyerabend too had taught that the hegemony of science must be limited by the decisions of a democratic society. Thus, according to Nandy, the violence of modernity arises from its secular pursuit of rational interests, whether those interests are pursued at the individual or at the collective level. Scientific rationality thus reduces everything to an impersonal, negotiable, oppression system (Nandy, 1998, p. 285). On that same page, Nandy summarizes his point forcefully as follows:

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40 Colonial empires, historically, were the primary vehicles through which scientific knowledge was introduced in different countries of Asia and Africa. Only a few societies like Japan, were an exception to this rule.

41 Or, at least the hope of such success.
“…main civilizational problem is not with irrationality but with ways of thinking associated with the modern concept of rationality; that modern science has already built a structure of near isolation where human beings themselves…have been objectified as things and processes, to be vivisected, manipulated and corrected.”

Nandy’s arguments thus boil down to the simple but powerful schema:

\[
\text{No organized science} \rightarrow \text{No isolation} \rightarrow \text{no vivisection (of societies)}
\]

2.3. Science as Enlightenment

If the arguments of the post-modernists focus on the lack of potential of scientific rationality to bring about a state of enlightenment for the non-West, the other side on the debate focus exclusively upon the dangers that lie inherent in the abandonment of a universal rationality and in embracing local notions such as various forms of ethnosciences (among which there could be no possible communication). A very articulate response of this type comes from Meera Nanda, a former biologist trained at the Rensselaer Polytechnic Institute, who picks up the task of defending modern scientific rationality (from the ethnoscientific encroachments) quite vigorously (Nanda, 2004). Nanda’s responses indeed swing to the most opposite extreme, placing all values of modernity within the secular discourse of modern Western science. The indigenous traditions of the sub-continent are entirely stripped of their ability to produce any enlightenment, appearing, in her account, only as sources of oppression. It is science, and its background of a secular, Western-style rationality that could bring about, according to Nanda, a much needed enlightenment for societies like India.

Nanda’s main attack is directed towards the very notion of ethnosciences, a term made popular by Sandra Harding and her post-colonial Indian allies. Knowledge-claims of this type, Nanda says, will abolish all notions of scientific expertise and human freedom, and ushering in an era of darkness, such as an epoch of Hindu (and Muslim) fundamentalism. The acceptance of the right of all cultures and traditions to decide their own domestic norms of evidence will seriously dilute the concept of special, professional expertise that now forms the cornerstone of all modern science. Furthermore, the few examples that have been produced to date as instances of ethnoscience by the post-modern thinkers, e.g., the midwives’, the peasants’, or the herbalists’
knowledge, can barely stand up to empirical tests with controls which presently form the cornerstone of modern scientific practice.\textsuperscript{42}

Rejecting completely the position of the social constructivists that I have sketched briefly in the above, Nanda offers two main arguments as her rebuttal of the social constructivist position. First, she argues that the post-modern/post-colonial approach, if embraced by non-Western societies, will bring about a complete halt to the process of secularization, which she considers as essential for the formation of an independent scientific tradition. If secularization is impeded, then those limited and non-secular worldviews will forge strong partnerships with the rising tide of fundamentalism. Thus, according to Nanda, the costs of embracing the notion of alternative science will be to remain wedded to a frozen, fundamentalist society for life.

Furthermore, Nanda discusses special Hindu worldviews such as Vedic sciences and their claims to being a separate ethnoscience. Those enchanted magical, and organic worldviews, promoted by the classical Hindu metaphysicians, cannot be granted, according to Nanda, the status of another paradigm on par with scientific rationality, no matter how enchanting they might look like to the Western scholars on the surface. Historically, such worldviews have always been used to oppress the marginal sections of society, such as the women or the ‘lower’ castes, keeping them in a state of perennial oppression. Being naturally oppressive conceptual systems, ‘indigenous paradigms’ such as the Hindu metaphysics desperately need modern science in order to liberate their own people who still remain tied to those oppressive traditions. Thus, the notion of a Feyerabendian ‘free’ society where all traditions/paradigms claim equal access to power (Nanda, 2004, p. 140) will completely abort the process of an Indian enlightenment,\textsuperscript{43} giving rise to a fundamentalist society frozen in time. Thus, the post-modern ‘charity’, routinely extended to the third world, must be firmly rejected as an inherently dangerous gift, no matter how charming such ‘gifts’ might look like on the surface.

Summing up her arguments into a schematic form so as to compare them better with Nandy’s, we obtain the following schema:

\textbf{No science $\rightarrow$ No secularization $\rightarrow$ No Enlightenment}

\textsuperscript{42} The question can of course be raised whether such examples of ‘alternative science’ at all need to conform to the standards of modern scientific practice. The point Nanda makes is perhaps about \textit{effectiveness}, i.e., since such knowledge does not pass the efficacy tests of modern practices, they cannot be deemed as effective as modern science.

\textsuperscript{43} And according to Nanda, this process has already been so aborted in India.
A quick assessment of the arguments of either side reveals some serious flaws in the logic of both, and a tendency to look upon science only in the abstract. The fact that the cultivation of a scientific worldview has sometimes helped to fight oppressive practices within (some) non-Western societies does not preclude the opposite possibility that those very worldviews might also be co-opted (at some other point of time) to produce other—and perhaps more oppressive—sets of circumstances. To use scientific rationality to weed out all other forms of knowledge and popular traditions that currently survive in the form of local and oral traditions, might abolish all checks and balances upon scientific rationality, thus giving rise to a monolithic society with a single source of knowledge. In Feyerabend’s posthumous work (1999), *The Conquest of Abundance*, this scenario is indeed sketched with great anxiety. If such a thing comes to pass, then there is not a hint in Nanda’s writings on how such situation ought to be addressed. In her account, the power of the sciences to save the world has been taken for granted. It is also by no means self-evident that resistance to a scientific worldview is necessarily synonymous with a ‘backward and a medieval’ mindset. After all, the oppressive consequences of modern scientific rationality have been experienced most acutely within the framework of post-colonial societies where science has always formed a tight nexus with different state-sponsored projects, and have often been pushed down the throats of the people in the span of a few decades. Reluctance to convert to such ‘modern’ views, may after all be simply signs of wise caution. While similar problems might surface within the industrialized societies as well, it might manifest there in a less severe form, given the fact that the West had lived historically a longer time with the institution of modern science, and was able to modernize at its own pace.

On the other hand, Nandy’s wholesale rejection of scientific rationality as an independent isolation system crucially hinges upon a particular way of viewing science. Nandy insists on seeing science as a unified, monolithic enterprise of totally Western origin. Thus, he ignores the possibility that scientific knowledge might after all be multi-faceted—both in its origin and in its practice—and be focused on other kinds of research programs rather than simply on violence or domination. His analysis of science as a rigidly-structured isolation system is firmly wedded to this monolithic view of science, and therefore stands or falls with it. It is of course hard to deny that science contains a great deal of potential for destruction, but if science is considered to be a multi-faceted institution and practice, it may be found to contain—in addition to those coercive experiences—experiences of empowerment, such as conquering infectious diseases or controlling the forces behind global warming. Experiences like the above can certainly bring about feelings of empowerment, especially in those societies which have historically been deprived of such experiences by their own indigenous worldviews. The account of science as a
domain-general rationality that first took shape in the West and thereafter spread to all other cultures by the cultivation of a secular temper, may be oversimplified. But to claim that it should therefore be eliminated altogether from how we take stock of our world and how we intervene in it may be another excessive form of oversimplification. The formation of the practice of science in the non-West must be understood by using a more fine-grained model than what has been provided here so far by Nandy.

Thus the debate about alternative science fails to address adequately how modern science evolves within the contexts of the non-West, and how, in course of time, its newly-emerging scientific communities come to participate in the practice of such science. Despite its one-sided, polemical character however, the debate on alternative science does expose an important issue about science: a society will have to live with certain consequences if indeed it chooses to invest in the epistemic resource called modern science. Thus, from the standpoint of a society that is about to make such an investment—or has already so invested—it becomes quite reasonable to ask how science works in such contexts, especially, how it has worked in the past in the location of the non-West where science has frequently been imported in the form of a peripheral practice. Exactly how have such practices helped or hindered the well-being of those societies? What kind of knowledge outcomes did they produce? And what is the potential and the future of such transplanted science?

One way in which this inquiry can be addressed is by unpacking the concrete trajectories of peripheral practice, i.e., by exploring the contributions of those protagonists who developed a practice of science from such peripheral contexts. Thus, in order to understand how a newly-emerging scientific community in a peripheral context creates an independent trajectory of its own, we require the historical details of a peripheral practice—and how it accomplished the difficult task of re-interpreting and re-defining science in a new context. Thus, our inquiry about such science must begin by unpacking the practices of those scientists—with a new set of methods—learning how they manipulated their representations and how they established communication with their peers at a metropolis. An inquiry of this kind will show us the conceptual and the cognitive underpinnings of such a practice, thereby avoiding the previous polemical attitude of thinking about such science only in the abstract.

What an inquiry of this kind reveals is the creativity embedded in these peripheral situations—how peripheral scientists develop and create new conceptual representations and how they begin a new scientific tradition. In reconstructing the trajectories of science from such a context, we can see how a new scientific community emerges within a non-Western setting with
goals and research programs of its own. To show the cognitive and the social aspects of such a peripheral community, one needs to make use of historical materials as well as recent developments from cognitive science that show us new ways of reconstructing and exploring scientific reasoning. Analysis of this type has already been used to show how new concepts or practices emerge in science, and how older theories take new shape to receive new contents (Nersessian, 1992). But such analysis has so far been confined only to analyzing science in its Euro-American contexts. In this dissertation, my goal is to extend this analysis to the locations of the non-West, by capturing the dynamics of scientific practice outside of its standard Euro-American homeground.

2.4. Science in the Non-West: Basalla’s Model

The most important theoretical portrait of science in the non-West was produced by a former physicist-turned-historian-of-science named George Basalla, who in 1967, provided a three-stage model to explain the process of dissemination of science outside of its Western home ground. Basalla’s model postulated three distinct stages in science during this process of transmission. In its first stage, a non-Western society serves as a peripheral area of interest and investigation (i.e., as a source of data) to a Western scientific community. New information is gathered from this peripheral area and is brought to a center. The next stage is characterized by a period of dependent or colonial science, during which time the peripheral society receives a chunk of scientific tradition from a developed center by means of a diffusion of expertise and training. In the third stage however, those traditions and institutions of science become sufficiently well-grafted onto the non-Western society so as to give rise to a newly-emerging, indigenous scientific community, capable of functioning on its own.

At the outset, Basalla’s model claimed a strong historical plausibility that was intended to measure up against the actual accounts of science in the non-West, but soon fine-tuned historical analyses by post-colonial scholars (McLeod, 1999) identified a number of flaws in that model,

44 See Kapil Raj, 2000. Raj’s article shows the role of peripheries and its (large) contribution in the flow of scientific knowledge towards a center. In this dissertation, I focus on the converse process, i.e., how, within a periphery, scientific knowledge is assimilated and redefined in an attempt to create an independent center of such research and how this project is both shaped and constrained by the location of such scientists at a scientific periphery.

45 The term ‘colonial’ here signifies a relationship of epistemic dependency and not necessarily a political relationship. No political colonial relationships existed between 19th-century Russian science and that of the Western Europe, and yet, Russian science of this period can still be described as colonial science because of its dependent nature on the European science.
underlining its heavy Eurocentricity. Put briefly, Basalla’s model had made the Western side the seat of all agency in this exchange of knowledge. The non-Western side simply waits there to receive what has been transmitted to it, contributing little—if anything—in the process. Thus, even after the sciences have been transmitted in that location by means of diffusion, it survives there only as a weak kind of presence, rooted insufficiently in the psyche of its (indigenous) researchers who seem prone to slide back to their pre-scientific ways at any moment. The newly-implanted research tradition is thus perceived as not well-integrated with the rest of its tradition. Nandy’s account, which saw in science only a source of social alienation for the non-west, draws out this implication in the transmission model of peripheral science and expands on it very thoroughly. But if the notion of transmission is replaced by other notions, such as ‘redefinition’ or ‘translation’, then those passive actors are immediately transformed into active agents, and the peripheries are seen as centers of busy activity (See Raina, 2003, p. 161 on this).

To be fair to Basalla however, he did not claim any historical or metaphysical necessity for his model. Instead, he expressed the hope that his crude beginnings would soon be replaced by other sophisticated appraisals of peripheral science, giving us a better accounts of how science evolves within those different cultural, social and institutional settings. Thus, Basalla’s model was actually an invitation to develop more context-specific models for peripheral science. The post-modernist and the post-colonial projects on science however, did not tread that path. Instead, they insisted on seeing peripheral science through the lenses of the old transmission model, except that under their reading, science no longer had any virtues left that it formerly possessed. Thus, according to their views, the transmission of scientific knowledge to a non-Western society would surely be detrimental for those societies, for the non-West has no clear agency in the formation of this knowledge.

2.4.1. Replies to Basalla: Raina and Others

Replies to Basalla spawned an entire literature of their own (see Prakash, 1999; Raina, 2003. Babbar), but here I shall confine myself to a specific aspect of that analysis without summing up the entire critical literature. The last two stages of Basalla’s model—the colonial stage followed by the stage of independent science—was presumed by him to be two successive stages of a single process. As a matter of fact however, they embody altogether two distinct

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46 Such is often the self-perception of the peripheral scientists themselves. For this kind of self-criticism, see “Science as Culture” in the Journal of Indian Physics, October, 1961.
processes and thus should show completely different form of dynamics upon analysis. While during the dependent (i.e., colonial) stage, the peripheries receive a number of inputs from a center (in the shape of new research programs or the rudiments of a scientific education), during the independent stage, it falls upon the indigenous researcher to design a preferred model of practice, and construct research problems that thereafter becomes their basis for an emerging scientific practice. Thus, the social and the cognitive resources used in each of these stages must be vastly different. This point was made briefly in two articles by Dhruv Raina (2003), in which he used several brief sketches of early Indian scientists—during the early 20th and sometimes the late 19th century—highlighting how, in each case, it was the peripheral scientist who brought the more advanced—and thus the unexpected gift to the center. Facts like these show us a high level of cognitive creativity existing within the limited confines at the periphery, thereby refuting Basalla’s one-sided transmission thesis. At its best, peripheral science is well able to contribute to the science done at the center, and has in fact so contributed, which we can see from its history.

A more representative model of peripheral science should thus aim at showing us in greater detail this creative and the cognitive component of peripheral scientific activity, which can be seen clearly if we analyze its moments of engagements with a new tradition and with a new body of knowledge. If fully articulated, such accounts can show us the differences in dynamics that separates the dependent and independent stages of peripheral science. It can tell us for example, what different kinds of cognitive and social practices were operative during the independent phase of creating a new scientific practice. In view of the membership of the peripheral scientist in two very dissimilar worlds—his/her own as well as the metropolitan center (in whose centers of expertise his/her entry remains a difficult challenge but must be accomplished)—such accounts become a crucial part of our reconstruction of a peripheral practice. The important issue about science in the peripheral contexts thus becomes to show how this situation shapes the development of scientific research programs in those locations, and how new practices that are developed by individual scientists in such situations start newly-emerging research groups and a new tradition of expertise. What kind of agency did those newly-initiated peripheral researchers display during such exchanges of knowledge: could they generate a consensus for their research by means of their own agency, or were they confined to the role of

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47 See Raina, 2003. “Reconfiguring the Centre” and “From West to Non-West”.

48 Raina gives us two such examples in his 2003 essay: Ramanujan and G.H. Hardy in mathematics, and M.N. Saha, the astrophysicist, who collaborated with the American physicist Henry Norris Russell.
playing followers to metropolitan science? How much intellectual authority and trust did they command in matters of scientific knowledge, when knowledge was produced by them?

Clearly, an inquiry of this sort will produce a significantly different account of peripheral science—an attitude that sees in such science neither a straightforward path to enlightenment nor a surrender to an alien body of knowledge, but instead, the natural cognitive response of a society which, confronted with a new epistemic product, seeks to engage with it with the aim of forging a (new) epistemic instrument for its own use. Such instruments—if properly nurtured—can become a valuable resource for such societies, despite its initial stages of asymmetry and limitation. Thus, peripheral science is more a case of initiating a new cognitive beginning and less a case of diffusion. Contrary to the two polarized views sketched above, in this dissertation I defend a middle view about peripheral science, positioning myself into a third stance between those two alternatives.

2.5. A New Analysis of Science in the non-West?: From Alternative Science to a Cognitive-Philosophical-Historical (CPH) Model of Science

The questions raised in the previous section show us the need for a fresh inquiry about peripheral science. Note that the debate on alternative science only asked the question whether different non-Western societies should shift to their own forms of ethnosciences or embrace the “universal” norms of a Western scientific rationality. In putting the question in this abstract form, the debate on alternative science found it possible to ignore all the details of scientific practice at (different) peripheral locations. Thus, in such discussions, science assumes (routinely) the character of an omnibus abstract unit, merging all disciplines and all kinds of science under a single dominant rubric. This coarse-grained inquiry must now be set aside in favor of a finer-grained exploration of peripheral science that recovers for us the details of those emerging practices—highlighting the cognitive and the social engagements by means of which a community creates and sustains such practices. Such a revised form of inquiry is capable of giving us new insights about the development of specific research programs at different peripheries and the outcomes that they have led to in such contexts. Furthermore, an inquiry of this sort calls for a different set of materials than has been used so far in exploring and evaluating peripheral science. To show how such science contributes to the task of making new knowledge or in other words, how a peripheral scientific community (still in its nascent stages), engages with scientific knowledge in a non-Western setting—we require materials that are more detailed in
nature than an abstract analysis of science. What we require in fact is a cross-section of a scientific encounter, i.e., the representations, the cognitive practices, and the mental models of the peripheral community, and what use they make of those materials during their task of creating an emerging form of scientific practice. In exploring this material, we see both the difficulties and the possibilities of such practitioners. Until we are able to reconstruct the episodes of peripheral science more concretely at that level, the question of what such science can do for non-Western societies cannot receive a satisfactory answer.

Clearly, a shift like this demands a new kind of analysis. Thus, from abstract polemical discussions about alternative sciences, we shift to a new form of inquiry, known as the Cognitive-Philosophical-Historical (CPH) analysis of scientific knowledge (Nersessian, 1986). If science is indeed universal, then one way this universality manifests itself is by producing new professional communities in new social (and institutional) settings. Hence, in asking how such nascent communities develop a practice of their own in different contexts (and how they start a new network of scientific expertise), we ask what scientific rationality can achieve for (non-Western) societies (and also whether it can function there as an instrument of enlightenment or oppression). Thus, we must begin our inquiry by asking how a research tradition in science is produced within the specific contexts of a non-Western society by means of the activity of some of its early pioneering researchers. How did someone, living in a colony, and perhaps being a colonial subject, turned himself/herself into a scientist? What were the cognitive practices such scientists used in accomplishing their tasks, and what were the social resources to which they had access during this kind of endeavor? What were their aims in developing this science, and were these aims the same as (or different from) those of their metropolitan colleagues?

If an investigation of this form can be undertaken, it will give us a clearer picture of how science emerges within the contexts of a specific peripheral location of the non-West, and how a peripheral scientific community, seeking to enter this practice, engages in exchange and communication with its counterparts at a metropolis. Since the relationship between scientific practice, knowledge, and culture is contingent upon specific historical contexts, such a theoretical framework must be sensitive to those details of history. In this dissertation, I shall explore those peripheral processes by exploring the formation of the discipline of physics in India during 1910-1930 as my main site of inquiry. Since the first generation of scientific communities in India dated back to those colonial times, my goal will be to investigate some actual colonial scientific community and of their epistemic efforts. I shall ask how their encounter with Western science was structured and whether this engagement produced any independent, self-sustaining tradition.
Could a peripheral scientist make knowledge in the same way that were available to his European counterparts? Did his or her unusual location provide him/her with richer resources as well as with a richer set of problems? What exactly were the mechanisms by means of which his/her work was produced, cited, accepted (or perhaps rejected) by his metropolitan peer group? How was the research validated, and what produced a new consensus? Just who determined success and what counted as a failure?

An inquiry of this sort can shed light, I believe, on three interesting facets of such science:

1. What kind of scientific practices were formed within those peripheries?
2. How were those practices structured and created? How did a normal scientist strive to overcome his identity as a colonial subject to become part of an international community?
3. What goal did this acceptance achieve? What were such sciences for?

An inquiry of this kind will produce an investigation about science not at an abstract or polemical level, but at the level of an actual engagement of a peripheral community in the process of its work of making new knowledge. My task will be to explore such a community in the context of its development, looking upon its practitioners as cognitive agents, how they propose and (sometimes) develop solutions to problems, and what conceptual tools do they use during such encounters. When we keep in mind that the early Indian colonial scientists were working with an ontology as well as a methodology that were not formerly part of their tradition—but received by them in their identity as colonial subjects—but the provided them the only permissible format for their work, those complex underpinnings of peripheral science become interesting.

A Cognitive-Philosophical-Historical analysis of science that supports such an inquiry has been employed in recent decades to explore the problems of conceptual change and the creation of new scientific knowledge in the contexts of metropolitan science. It has become a preferred tool for analysis for some philosophers who seek to provide an epistemic account of scientific knowledge that is different in nature from a social analysis. Christened generally as the CPH or cognitive-historical method (Nersessian, 1992, 2002, 2008), such analysis usually focuses upon the representations, the knowledge base, and the communicative practices—as well as the embodied expertise of the scientists, such as their instrumentation or their laboratory practices—to explain science. It seeks to reconstruct how a scientist (or a community) manipulates those elements to produce a new cognitive outcome. Such reconstructed picture of scientific activity gives us an account of how new concepts and new practices are produced and how they change
— both at the social as well as at an individual level. In this model of science, conceptual structures, such as mental models or experimental skills, are manipulated by an individual in order to produce a new result, such as the postulation of a new concept, or the formulation of a new theory (Johnson-Laird, 1983; Giere, 1988). Applied to the history of the electrical field, for example, such a CPH analysis tells us how a scientist, in this case Michael Faraday, transformed his inherited representations by his experimental activities into a set of new outcomes. The very same form of analysis, I suggest, can be applied onto the episodes of peripheral science, showing us how newly-emerging scientific communities are born at the peripheries of the dominant Western ones, and how such peripheral practitioners engage in the task of making new scientific knowledge and developing their own research programs.

A recent work on the 19th century Indian Renaissance by Subrata Dasgupta (2007) provides us with some background assumptions that can help us specifically exploring the contexts of Indian peripheral science. In this work, Dasgupta claims that the 19th century Bengal Renaissance in India was characterized by two major cognitive themes: its assumption of a universal, cross-cultural mentality, and its deep desire to overcome the negative identity of a stagnant society (Dasgupta, 2007, Chap 1). These two represent the positive and the negative sides of a cognitive response that was responsible for giving birth to a professional science in the Indian context. Thus, the early Indian science was born of an interaction between these two elements, and was undertaken, often consciously, to overcome a negative identity and produce a science characterized by national spirit.

Dasgupta however, does not agree with the general approach of science studies, at least the usual forms that such studies take in the Indian contexts. In a recent privately circulated paper entitled “Science Studies sans Science,” he argues forcefully that most science studies contributions bypass the actual content of peripheral scientific activity, thus allowing their authors to derive too broad, sometimes even fanciful, conclusions about this kind of science. Thus, the conclusion of those critical attempts cannot be perceived to hold any real value in explaining science, especially science in its peripheral contexts. Even though the paper does not explicitly say so, it is implied that Dasgupta would actually approve of a study that takes into account the details of peripheral practice and evaluates whether such practices can be judged to be a progressive enterprise.

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49 Models thus could be invoked to generate an explanation at many levels. For instance, the human functional ability of memory has been modeled as a collection of boxes, such as a short-term, long term and semantic memory. The resulting analysis thus produces a picture of memory operations, which in turn is used to organize and guide further research.
In the remaining chapters of this dissertation therefore my goal will be to develop an account of peripheral science by analyzing the episodes of some peripheral encounters in colonial Indian science during its formative years of 1910-1930 as my main focus. This was a time when Indian peripheral science was coming out of the second stage of Basalla’s analysis, emerging into a small but sophisticated urban community. In what follows, therefore, I shall try to reconstruct the outlines of this emerging practice as follows. In Chapter 3, I shall provide a short review of the literature on the CPH analysis of science, which tells us how we can understand the nature of a scientific practice by using this analysis. In Chapter 4, I shall develop a framework for analyzing the interactions of a peripheral scientific community with its metropolitan counterparts, and the patterns that such interactions assume during the progress of a peripheral community. Having put this framework in place, my final task in the dissertation will be to unpack some episodes of early 20th-century Indian peripheral science in the light of this theoretical framework. The two episodes from the Indian context that I shall choose for analysis are the formulation of a new quantum statistics by S.N. Bose (1924), subsequently extended by Einstein to predict the phenomenon of Bose-Einstein condensation. My second episode will be the discovery of a new form of radiation by C.V. Raman and his student K.S. Krishnan, which is now known as the Raman Effect (1928), and currently forms an important area of modern spectroscopy. In exploring these two episodes of Indian peripheral science, my task will be to sketch the outlines of science which was created by these peripheral scientists in their quest for a professionalized science in India. We shall examine how this newly-emerging group, focusing mostly on the problems of basic science, brought into existence a culture of scientific research by their contributions and their agency, and if their contribution was adequate to fulfill those chosen goals. If at the end of this inquiry, we have an adequate reconstruction of these two episodes that provide us with a clear picture of the cognitive processes of those scientists, we shall be better equipped to return an answer to the controversy on the aims of science in its non-Western contexts.
Chapter 3: The Trading Zone of Peripheral Science: Combining the Cognitive and the Social in a CPH Analysis

3.1. Introduction

Peripheral science then, could be usefully explored by taking a cognitive point of view. But how do we see a peripheral scientific community in terms of the mental and the embodied activities of its practitioners? What kind of analysis will show us how in engaging in such activities a peripheral scientist engages with a new body of knowledge? To see this, one must first needs to see how a cognitive account illuminates the practices of science in general.

In the contexts of metropolitan science, a cognitive analysis of science has been used primarily to solve the old problems of conceptual change and the problem of incommensurability within the theoretical language of science. The other meta-project with which this analysis has been associated is the question of whether to reject an excessively intellectualist view of scientific knowledge, commonly referred as the ‘reasoning-in-the-skull’ view (cite literature). What I shall suggest in this chapter is that there could yet be a third project with which we can link this analysis—that of exploring the creativity of peripheral scientists.

The problem of communication in scientific knowledge started with the notions of paradigms and the revolutions introduced by Thomas Kuhn. Instead of thinking about science as a continuous, cumulative form of inquiry produced by means of deductive and inductive reasoning — the model of science that the logical empiricists had been espousing since 1926—Kuhn suggested that science should be viewed instead as a collection of paradigms, its each block being isolated from the rest as in politics or in fashion. Following Peter Galison, I shall call this approach the block periodization problem in science (Galison, 1997). Between two periods or blocks there exist no continuous gains in knowledge and no common core of meaning. Thus, there could not be any continuous progress in scientific knowledge, given the actual history of scientific thinking. Clearly, this analysis privileges the social elements in scientific thinking over the epistemic, and following this line of reasoning, in 1986, Latour and Woolgar issued a famous moratorium in which they claimed that after a ten-year period, there would not be any necessity of any further explanation of scientific knowledge (apart from social explanations).

But the primacy of epistemic factors in science did soon make a comeback in the form of a different kind of analysis. With the emergence of a loose collaboration among psychology, neuroscience, anthropology and philosophy of mind, a new trend in explaining scientific
knowledge—now collectively called cognitive science—emerged during the 1980’s.\textsuperscript{50} This new analysis focused upon the internal as well as the epistemic factors in scientific knowledge, noting the different ways in which scientists process and make use of information, e.g., how they use analogy, mental modeling, or different forms of thought experiments. The use of these processes, it was felt, contributes crucially to the making of new knowledge. Thus the contrast between this new approach—and the old sociological approach—lay in the former’s strong emphasis upon the operations of the human mind and its indispensable contribution to scientific practice. Put briefly, the new approach claimed that the relationships among concepts in science must be understood not by looking at the concepts themselves but by looking at the creative processes in the minds of the scientists by means of which such structures emerge into existence. This required, of course, that more factual and psychological details be admitted into the analysis of science than was formerly permitted by a purely logical reconstruction of scientific knowledge (which tended to bracket out such factors as mere contexts of discovery). On the other hand, claims that invoked mental processes had always been suspect from a sociological point of view, but research in cognitive psychology assisted philosophers in this explanatory trend, giving them more information about how human mind processes and formulates knowledge. Learning how our minds generally think, reason and solve problems helped further to see how minds can also do science, especially with such cognitive tools such as analogical reasoning or visual imagery. In this newly-emerging field known as the cognitive studies of science, philosophers and psychologists started working together forging new explanations of scientific knowledge. Turning their attention upon the cognitive elements of science—e.g., how human minds process information once it is given its inputs—they started claiming that the mental activity of the protagonists in science constitutes the crucial part of explanation of scientific knowledge. An in-depth analysis of such activities, and how they sustain and create various embodied practices in science, would tell us how older concepts get transformed into new ones. Such explanatory attempts were accordingly tried out in various formats, e.g., in historical studies of science, algorithms of discovery, and in studies of expert systems.

In this chapter, my goal will be to show that this approach, which is already used by a number of philosophers to explain the processes of science in its metropolitan contexts, could be usefully extended to explain the processes of peripheral science. Indeed, in both cases, such

\textsuperscript{50} Cognitive science broadly speaking, of course, is concerned about mind or intelligence in general, and how information is stored and processed by an animal/human brain (or even by a machine). The implications of such studies for science however is quite transparent, for scientists too are cognitive agents, and much of the practice of science requires the storing and processing of information.
analysis helps us in gaining the same kind of insight—showing us how science is done at the level of its actors or its protagonists. Thus, in this and in the subsequent chapters, my aim will be to put together an account by means of which peripheral scientists and peripheral science can be explored from a cognitive point of view. An analytic framework of this type will show us how a peripheral community and their practices can be understood in terms of the mental and embodied activities of its protagonists, and how such protagonists build a network of communication with their metropolitan peers by means of those activities. With such an account in hand, we can further see how in engaging in these cognitive activities, a peripheral scientist gradually turns himself/herself into a reliable maker of knowledge.

To develop this approach, I shall proceed below as follows. Firstly, I shall sketch the contributions of a few philosophers, who have given us an outline of how science in general can be seen from a cognitive point of view. To make this account intuitive and transparent, I shall next explain some exemplars of their cognitive analysis. Note that such analyses are not necessarily tied to the image of a single problem-solving mind and its internal processes. Indeed, cognitive explanations of science can comfortably accommodate science when it is done by a community, or when a number of such communities engage with one another in producing a complex chain of practice. Once I have sketched such a revised account of scientific practice in the contexts of metropolitan science, my next goal will be to extend that account to include the contexts of peripheral science.

Let us begin this task by considering the contributions of four different philosophers, who together present us with a cognitive account of science. I shall begin with the position developed by Ronald Giere, who gives us a semantic account of scientific knowledge, which in turn becomes a case-study based approach to science in the hands of Nancy Nersessian and David Gooding. Both Nersessian and Gooding seek to show us how by developing a CPH analysis we can put together a dynamic account of a scientific practice. Finally, in the work of Michael Gorman, we shall find an approach that I consider crucial in developing a revised account of peripheral science. Gorman introduces the notion of a knowledge-network, which he construes in terms of the cognitive transactions that its agents establish (and maintain) with one another. Such networks are both dynamic in nature and capable of evolution in the future. This is a crucial concept in analyzing peripheral science for it is by means of such dynamic networks that a peripheral scientific community forges its first ties with its metropolitan counterparts, beginning

51 Karl Popper, for example, thought of science by using the model of a single, critical scientist.
thereby its first serious quest for science. Peripheral science thus can be analyzed in terms of a
dynamic network, which is capable of further growth, especially a growth that can be either
positive or negative, depending on the circumstances—i.e., which can either take off or diminish.

Taken together, these four approaches show us how a scientific practice can be
analyzed in terms of the cognitive structures that grow out of its problem-solving practices. Once
looking at science in this way is grasped in the metropolitan contexts, my next task shall be to
extend this analysis to the peripheral contexts, with the aim of showing how peripheral scientists
create new representations in their course of work. Since my preferred periphery in this
dissertation will be the early 20th century colonial India, in the second part of this study, I shall
apply this analysis to the two episodes of Indian peripheral science. This analysis of a peripheral
scientific community in terms of its creative, cognitive processes and its networks of
communications with its metropolis will allow us to see how an epistemic culture in science is
born in a new national/cultural setting as the outcome of the activity of a few pioneering
practitioners. Such account can then be used to understand what kind of science can be produced
by such a newly-emerging, scientific community from its differential location. By evaluating a
scientific practice in this way—showing what such a practice consists of and what it can
achieve—we shall gain an insight into the potentials of science in the contexts of the non-West.

3.2: Science as Model-Based Reasoning: Ronald Giere

Let us begin this task by considering a framework that is different in kind from the
sentential framework used by the logical empiricists. In Ronald Giere’s work (1988; 2002; 2004),
we find a theory of science that is a conscious replacement of the older logico-empirical
framework of scientific knowledge. Instead of seeing science as a collection of theories expressed
in the form of linguistic statements (and related to one another by their logical relationships)
Giere suggests that scientific practice be seen in *terms of the activities that its agents undertake*,
more precisely, the activity of building and applying models. Models are theoretical structures
that stand halfway between the natural world and the human mind, and are often *embodied*, i.e.,
models can be physical, mental or mathematical. Thus, to put it briefly, scientific activity
involves manipulating groups of models from different problem perspectives in order to produce solutions for specific research problems.  

Clearly, on such a model-based approach science emerges as piecemeal and perspectival, but it has the pleasant consequence of providing a continuity in scientific enterprise, which we intuitively take to be a marker of scientific activity. Thus, Giere’s account offers us an attractive way out of the block periodization problem of Kuhn. Furthermore, switching models does not imply any loss of knowledge. Knowledge gained under one set of models remains quite viable (and accessible) when we begin working with a different set of models. Indeed, it is normal for practicing scientists to work with multiple sets of models — all of which can exist in their mental toolbox side by side — different sets of models being invoked for different purposes. This analysis of scientific activity in terms of the diverse operations of mental modeling provides us an explanation of what scientific activity consists in by focusing our attention upon the mental activity that is implicit in science, and its natural embodiment in concrete, experimental practices (such as technical skill or visual imagery).

Giere’s approach thus steers us firmly towards an agent-based approach — giving us, in effect, a cognitive and a pragmatic theory of scientific practice. Doing science consists in engaging in different forms of human agency, applying different models in the service of different goals. Learning how to do science means — under this account — learning how to manipulate, construct, and reason in terms of a group of models. Thus, the mental operations of the scientists, as well as their experimental set-ups (and the construction of those set-ups), become crucial for understanding particular episodes in science. However, since not all the operations of modeling take place within the privacy of a human mind, other supporting structures or scaffoldings of mind, such as instrumentation, social structure, support of a culture

52 Regarding the ontological status of such models, Giere claims that models are like maps — which are always partial, and are always used to serve specific purposes. Since models behave very much like the maps, they too must be partial, and can only be used from a specific point of view.

53 An excellent example of this kind of activity in the peripheral contexts can be seen in C.V. Raman’s attempt to use both the wave theory and the quantum theory of light — for different purposes. While Raman supported the quantum theory of light to construct his overall research program in optics, he still tried to interpret newly-discovered results in X-ray diffraction in terms of the traditional wave theory of light. See my case study on Raman in chap. 6 for more details.

54 Furthermore, this account provides us with a tentative explanation of why, in spite of having the same cognitive apparatus, different societies exhibit different levels of engagement with science, i.e., why the growths in science that are observed in some places are not observed elsewhere.
within which a scientific practice is embedded, *all become equally important ingredients of scientific activity*, making scientific knowledge in the end, a matter of distributed cognition.

### 3.3: The Method of Cognitive Case-Studies and the Networks of Expertise: Nancy Nersessian, David Gooding, and Michael Gorman

These general insights on scientific knowledge became concrete programs to analyze the history of science by a number of other philosophers, whose reconstructions of scientific knowledge in the shape of case studies became the hallmarks of a cognitive approach. Using the model-based, semantic approach that I have just now sketched in the above, a number of philosophers worked hard to develop concrete case-studies of scientific thinking, developing fine-grained investigations of scientific reasoning in the process. Two philosophers in particular, Nancy Nersessian (1984; 1992; 2002) and David Gooding (1992; 2006) exemplify this approach particularly well, showing in their work how by means of such an approach, we can develop cognitive-historical analyses of scientific reasoning, and recover the dynamics of a scientific practice (or that of an individual scientist). In order to do this we must show his or her pathways of constructing a new concept (or a new artifact). Thus, the format of analysis that they develop is to trace *a line of cognitive descent* in the midst of all procedural or conceptual changes that a scientist or a community undergoes during his or her practice. In her 1992 essay in the *Minnesota Studies in the Philosophy of Science*, Nancy Nersessian names this approach the Cognitive-Philosophical-Historical (CPH) analysis of science (Nersessian, 1992, p.5), which is the name that I shall adopt for the rest of my dissertation.

Put briefly, a CPH analysis joins the two endpoints of an episode of creativity, exploring the cognitive pathways that the scientist travels between his or her initially perceived problem to the finally proposed solution. This reveals the dynamics of scientific activity from the participating protagonists’ point of view. The philosophical task in this analysis is to *reconstruct* the trajectory of the cognitive agent with four kinds of resources: a) concrete data from the history of scientific practices, including the socio-cultural contexts of that practice, b) concepts, methods and explanations of scientific knowledge from cognitive psychology, c) literature that provides accounts of science, mainly from science studies, and finally, d) a grasp of the philosophical issues and the necessary conceptual analysis (Nersessian, 2008, p.21). In this task of putting together a cognitive account of scientific knowledge, research on the knowing subject serves as our most important resource, since it informs us about the human mind and its
knowledge-constructing practices, including how those practices are applied in concrete situations.

The overall attractiveness of this approach comes from its ability to offer solutions to the problems of conceptual change and conceptual incommensurability, raised long ago by Kuhn. In a CPH analysis, the different paradigms in science are linked together by tracing their cognitive descent from one another, showing us how, in that process of growth, an older concept becomes transformed into a new one. Articulating the cognitive mechanisms that lie behind such transformation—pushing the process from one level to the next—becomes the dynamic factor in this cognitive approach. Thus, much of Nersessian’s and Gooding’s work consists in showing the processes that were operative during a particular episode of conceptual change and about which we possess concrete historic evidence. As Nersessian herself puts it, “This method combines case studies of actual scientific practice with the analytical tools and theories of the cognitive sciences to create a new, comprehensive theory of how conceptual structures are constructed and changed in science.” (1992, p. 5).

Both Nersessian and Gooding therefore seek to provide us with exemplars of this kind of analysis—exemplars that are usually taken from the well-documented examples of metropolitan science, e.g., Maxwell’s formulation of the four equations that completely describe all electromagnetic phenomena, giving us a unified representation within which the velocity of light can be calculated. Similarly, Gooding provides us an account of Faraday’s invention of the world’s first electrical motor, which Faraday had developed using an extensive method of visual reasoning. The centerpiece of their analysis is to show how Faraday and Maxwell acted as cognitive agents, how they constructed a group of new scientific representations from the starting point of their initial problems and initial resources. Using such resources, as well as the generative processes of human mind, each of them created a trajectory by means of which they solved the problems and created either new representations or new artifacts. Once we know that trajectory and its detailed processes--and how those processes were threaded to their cognitive and social situations—we arrive at a good description of their science.

To get a flavor of this approach, let us now briefly consider how Nersessian explains how Maxwell created his four equations of electromagnetism (Nersessian 1992, 2008). Maxwell developed a set of equations that completely describes all electromagnetic phenomena, leading to a quantitative transition from Faraday’s earlier, and more qualitative, approach. Thus, between Faraday’s earlier work on electromagnetism and Maxwell’s fully developed later version, there comes a complete shift in the concepts, and therefore, a new change in meaning. Yet, this
conceptual change and the subsequent change in meaning become tractable, once we remember that between the two there also lies a clear line of intellectual descent. The crucial element that brought forth this transition from Faraday’s qualitative approach to Maxwell’s quantitative laws is, according to Nersessian, the operative mechanism of a ‘physical analogy’: a mental image of rotating wheels or vortices in terms of which Maxwell articulated his representation of the electrical field of forces—and which finally allowed him to calculate the flux of the electromagnetic field. This physical analogy of rotating vortices was extracted by Maxwell from the domain of fluid media (and its dynamics), and was extended by him—by a process of simulative reasoning—to the problem at hand, i.e., describing the field of forces in electromagnetic phenomena. Thus, Maxwell’s solution is a case of simulative model-based reasoning. With the help of such simulative reasoning, Maxwell enhanced (and gradually refined) his representations of the detailed behavior of the electromagnetic field through its various stages. This allowed him to visualize—and finally calculate and write down—his four equations. This mental model of rotating wheels or vortices in a fluid medium, and their analogy with a sea of electromagnetic field of forces, played the crucial role in the discovery process, mainly by providing Maxwell with a constraint, and allowing him to perform further (mental) manipulations on that image. Thus, the mental models, and the operations done on those models, are the crucial interface that gives rise to new knowledge. Aided by such images, Maxwell was able to create a wholly new concept in the history of science.

What is most important in this analysis is its emphasis on the processes in the mind of the cognitive agent—in this case simulative reasoning done by Maxwell—by means of which a new concept was born in science. Nersessian claims that such processes are both lengthy and organic in nature, comprised of much more than mere chains of deduction or induction, reliable in character but not necessarily truth-preserving. For example, in constructing his model of electromagnetic radiation in terms of the vortices, Maxwell went through a chain of intermediate mental models, each of which helped him to articulate his reasoning to the next level. From his initial modeling of the field of forces as a sea of vortices, Maxwell quickly saw that those vortices require something like ‘idle wheels’ between them for the purposes of smooth motion. Furthermore, those vortices must be made of some sort of elastic medium that allows them to store tension (in the form of an electric charge). Thus the series of models: collection of vortices, idle wheels, and an elastic medium out of which such vortices are made guided his discovery, acting like a series of steps by means of his reasoning finally produced a new concept.
According to Nersessian, an account of Maxwell’s science in terms of such generative processes provide us a better understanding of Maxwell’s science—and his creativity that gave rise to a new conceptual innovation in science. Note that at each of these stages Maxwell’s cognitive strategies did arise from a social context, and were also used by him from a specific social location. Thus, a cognitive analysis naturally takes note of the social factors in science, in the end providing a more inclusive account of the interaction between the two. Between the contexts of discovery and the contexts of justification, there thus stands an entirely new third dimension: the context of development. A full account of scientific practice and its moments of discovery requires that we lay bare this third dimension and reconstruct its dynamics.

Accounts of science that talk about such contexts of development highlight the mental activity of the scientists in the making of new knowledge, the social inputs of their knowledge, and its concrete extension in the form of experimental set-ups, and how all these elements, put together, produce the emergence of a new representation in science. A cognitive exploration of the episodes of science thus reveal lengthy, organic processes buried behind those representations by means of which scientists create and develop new instruments of thinking (Nersessian, 2008, p. 21). In what follows, I shall fully embrace this insight, and indeed, shall seek to extend this insight even further. Not only are such processes operative in science when a
new conceptual change happens within an existing scientific community, such processes are also operative in those cases when a new group of people embrace a body of knowledge, seeking to create for themselves a new epistemic instrument out of such elements. Reiterated interactions with this body of knowledge depend upon the same kind of lengthy and organic processes, and they too can be recovered by unpacking the reasoning patterns of these scientists and their associated experimental practices. It is by means of such processes that a new community evolves an emerging form of scientific practice. Thus, both the metropolitan and the peripheral contexts of science can be explored by the same kind of CPH analysis, in which we focus on the cross-sections of cognitive activity by means of which scientists construct new knowledge.

Naturally, such analysis also highlights the procedures and the experimental knowledge involved in science. Indeed, Gooding (1992) provides us with another exemplar of a CPH analysis by showing us how embodied activities, such as visual reasoning, contributed to the invention of the world’s first electric motor by Michael Faraday. In exploring Faraday’s experimental steps during the development of this artifact, Gooding develops a notion called construals “which are quasi-representations that act both as heuristics (guiding an emerging interpretation of phenomena) and as a basis for communicating and negotiating an emerging understanding” (Gooding, 1990). In other words, Faraday, according to Gooding, had developed a technique of visual reasoning that was embodied in the form of his individual skills and experimental manipulations (Gooding, 2006; 2004), similar to those used by many experimental scientists before and since his time. It is in the development of this sort of embodied expertise — and its gradual refinement — that explains Faraday’s success in developing the first electric motor.

Note that an important implication of this mode of explaining science is to broaden the range of what it means to reason in science. In contrast to the logical positivists, who allowed only deductive or inductive reasoning as the sole resource of scientific thinking, the proponents of the cognitive approach are willing to admit a broader class of candidates that do the inferential work in science. A cognitive approach generally appeals to the knowledge-constructing processes of the human mind — and how those processes take place in the form of analogies, abstractions, mental modeling or other similar operations. This introduces the following revision in the classic position on scientific reasoning:
1. It allows the use of mental models (which stand in for features of the world), whose manipulations can yield new images. Simulative reasoning, or reasoning in terms of analogies thus become a tool for solving problems. Thus, analogies, visual images, and thought experiments all become sources of inference.

2. Scientific knowledge is not confined necessarily to theories or statements alone, but is embodied concretely in the form of practices and skills. Transmission of those skills can take place through personal contacts and other such similar shared ways of doing things. Since acceptance of procedural knowledge and instrumentation becomes a new source of scientific thinking, we can already see how such acceptance and development of skills will take place differently in peripheral contexts.

3. Social factors that support these operations become its crucial inputs by providing the contexts for professional training, transmission of skills, and in general, as acting as its substratum. Thus, there is a big overlap between the cognitive and the social in science, bringing the historical debate between the two to a close.

If a CPH analysis of science can help us in exploring the knowledge-constructing processes of an individual mind, how does it help us in shedding light on the relationships that research communities develop with one another, when each of them, individually, are engaged in the performance of a knowledge-constructing practice?

It is here that the notion of an epistemic network becomes operative. In Michael Gorman’s work, we find a useful way of representing how scientific communities enter into relationship with one another. Their epistemic networking which is capable of further growth and evolution is named Study of Experience and Expertise or SEE (Gorman, 2005). A SEE perspective provides us with a framework for studying the asymmetric properties of peripheral science. Briefly speaking, SEE considers science to be a networking of people and their associated levels of expertise. It is by means of such a network of expertise that scientific practice is negotiated in a variety of settings. Applied to the contexts of peripheral science for example, it tells us how peripheral practitioners turn themselves into normal scientists, and how—once accepted within the metropolitan community as reliable creators of scientific representations—they strive to maintain a professional presence within that network. Applied to the metropolitan contexts of science, SEE has already produced a number of classical studies, e.g., comparing the problem-solving processes of the experts with those of the novices (Chi, Feltovich and Glaser, 1981).
Such reconstructions of scientific knowledge in the form of concrete case studies has produced by now quite a sizeable literature. Generally speaking, the goal of a CPH analysis is to illuminate the episodes of science from the perspective of its actors or agents, focusing especially on those moments when an agent is engaged with an ongoing problem and seeks to put together a solution. To explain a particular episode of science from a cognitive point of view thus means to explore the mental processes as well as the embodied skills of the protagonists and the social resources that they had access to while solving a particular problem. Since a scientist inhabits a particular locale that supplies him/her with those resources and also with the tools and the problems, those elements can no longer be bracketed out as mere contexts of discovery but must be accepted as elements of explanation. Furthermore, mental operations, such as analogies or imagistic thinking can no longer be labeled as weaker forms of reasoning. In spite of their logically invalid character, analogy plays a crucially formative role in giving rise to new representations, as we have seen in the above in the case of Maxwell.\(^{55}\)

Thus, a cognitive exploration of science produces a revised image of scientific activity. In this revised image of a scientific practice, scientific thinking is no longer tied to a central community, but examples of scientific reasoning can be located anywhere—inside as well as outside of the centrally located metropolitan communities. Account of science in terms of such cognitive processes penetrates deeper than the standard contextual histories. No doubt the contextualist histories too provide us with illuminating accounts of scientific knowledge—often by means of their thick descriptions of events that surround an episode of science. But, such accounts do not, in general, dwell upon the lengthy, generative processes operative in human mind and on its sets of embodied skills. An articulation of those processes in the form of an account provides us with a more detailed understanding of scientific practice.

3.4: Peripheral Science as a Trading Zone: A Format for Peripheral Interactions in Science

So far I have mostly shown that a cognitive explanation of science allows us to see the inner dynamics of a scientific practice, showing us in depth how a practicing scientist creates (or refines) new representations. But all my examples so far have been drawn from the contexts of

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\(^{55}\) It is not of course being claimed that the scientists will themselves be aware of these processes, and their own accounts of discovery might considerably vary from that of the philosophers. Nevertheless, reconstructed with philosophical, cognitive and historical resources, it is this wider picture that leads a scientist (or in the peripheral case, a new scientific community) to make progress in science, i.e., to go from their problem situations to proposed conclusions.
Western metropolitan science. In what way can I now claim that these concepts will also apply to the contexts of peripheral practice— that it will show us the ways in which a peripheral practitioner puts together an emerging form of scientific practice? To see what this question entails, let us briefly turn to the issues in chapter 2. In that chapter, we saw that the controversy about alternative science presented science mainly as an intellectualist and a philosophical project. It thus neglected all other aspects of science, e.g., science in the form of an individual practice or the practice of a community. Both the critiques and the defenders of scientific rationality thus engaged with this problem by using the identical assumption that science must be judged only in the form of an intellectual project. Regarding the status of this project however, they differed sharply, the critiques considering the project of science altogether useless for a non-Western society. This was due to their further claim that the non-West had no clear agency in the formation of this scientific knowledge (and thus can serve only as its field of exploitation).

To rebut such a claim, it is essential therefore to find some means of demonstrating the levels of agency/creativity in peripheral science. How do we understand whether a peripheral practitioner is displaying agency or creativity in the formation of new knowledge and how can we discover this in a given historical situation?

It is in this task of determining agency or creativity that we get substantial help from using a CPH analysis. By showing how a peripheral scientist creates a new scientific representation, or changes an existing practice into a new one, or uses a mode of visual reasoning or some form of analogy, we show how a particular scientist is being creative. This demarcates my approach from all other current approaches (such as the approach taken by the alternative science debate), which simply disregards all cognitive contents of peripheral scientific practice. In doing this, this debate simply assumes that science in a peripheral context can only take the shape of an exploitative venture. Yet, other possibilities within such science can become manifest on analysis if we are guided by a different— and perhaps a more nuanced— model of peripheral practice. But such a conclusion will require that we first develop a new account that helps us see the contexts of peripheral science in terms of its agency: its generative processes, its ability to create new representations, and its putting together of a new social context for science, and all this when the practice is in its formative stages.

To take the converse perspective for a moment, we also saw in the first chapter how, by and large, general accounts of science tend to overlook the peripheral contexts— that the experience of contributing to scientific knowledge from the point of view of a peripheral scientist rarely forms the subject matter of any philosophical account. Thus, the cognitive processes of the scientists who work from those contexts and their interactions with their metropolitan peers (in
order to establish a consensus for their work and gain intellectual authority) rarely receive any philosophical attention. Whilst the social empiricists do indeed view science as a social enterprise—modeling scientific knowledge in the form of interactions among its communities—yet, their views on the formation of consensus and intellectual authority in science bear little resemblance to the actual history of peripheral participation in science which shows us many cases of asymmetric participation. Thus, an account of peripheral science must steer a middle course between the two polarized attitudes—a critical attitude towards science on the peripheral side, and the general lack of interest towards such science on the metropolitan side. To develop a more fine-grained account of such science, we must therefore explore the contents of such scientific practice in terms of its generative processes as well as that of its social underpinnings.

An obvious gain of undertaking this kind of analysis of peripheral practice is that it widens the scope of the cognitive analysis itself. The CPH analysis of science was developed by the philosophers who wanted to address the problem of communication and belief changes between different paradigms. The ability of such an analysis to track down the contexts of development between two periods of science provided a way out of the old philosophical impasse of incommensurability. Thus, all the discussions of the CPH analysis that I have reviewed above, come mainly from the episodes of meaning changes or construction of new scientific concepts (in the Western metropolitan contexts). Yet, outside of this standard context, an analysis of this nature could still explore conceptual changes of a different type: how the cognitive and the social interact in the constrained formats of a peripheral practice where science arrives in the shape of a new import, how the epistemic machineries developed in one culture are transported and grasped in another, thereby illuminating the processes of development of modern science in the non-West. Such an analysis too addresses the problem of conceptual change, albeit change in a wider, more diverse, cultural background. To formulate such a new account of science, we require, first of all, an image of science that sees science at the level of communities—thereby showing us how the work of making new knowledge get divided between different groups.

To develop a revised account of peripheral science from this new perspective, let us now further expand the notion of SEE, which interprets scientific activity as an extended network that spans an entire epistemic landscape. SEE can be construed easily as a network of people and their associated levels of expertise that covers an epistemic landscape. The notion of an epistemic landscape is derived from Weisberg and Muldoon (2009), who provides us with a spatial representation of science, allowing us to see science as a field upon which its agents move freely from one region to another. With this notion in hand, Weisberg and Muldoon suggests that
during the development of a research program, scientists work, broadly speaking, on two levels: firstly, at the level of their own individual cognitive processes, using procedures like analogical reasoning, inference and other forms of expertise, but also at the level of a community, dividing themselves and their cognitive labors into different types of research strategies and thus pursuing different approaches. Now, during the pursuit of such approaches, an existing community may quite plausibly be approached by some newcomers from outlying areas of the landscape, especially when such newcomers are seeking to develop a practice of their own. Attempts to build shared zones of expertise with a more established, scientific community will multiply the numbers of trajectories on the epistemic landscape, each trajectory representing the path of an agent who is working on a significant problem.

If all the research topics in science are thus taken to comprise an extended landscape, then the formation of a new scientific community on a distant part of this landscape will add a new trajectory to the set of the existing ones. But when a new research community thus seeks to join an existing network of communities— moving across the landscape in search of significant epistemic problems—they would naturally enter into epistemic transactions with other established groups who are already present (and are contributing) on this landscape. Encounters of this type will quickly lead to the build-up of areas of overlap or trading zones between two such communities (Galison, 1996, 1997; Gorman, 2005). An analysis of such overlap areas or trading zones— and the cognitive activity that the peripheral actors produce within these zones— shows us how peripheral science can be seen as network of expertise that comes to exist among communities. The items in this trade are the various kinds of theories, representations and the mental models associated with those theories. It is within such trading zones that a peripheral scientist does his/her creative work. And it is this zone which we can explore further by using a CPH analysis. Thus, an analysis of peripheral science— and of those scientists who work within such contexts— begins with the recognition of a trading zone that exists in such contexts and how these zones can become transformed by the agency or the activity of those protagonists who work within it.

Why should one see peripheral science as a trading zone? The notion of a trading zone is taken from anthropology, which explains how languages and cultures meet in a hybridized space of overlapping (but partial) contact. Within such overlapping areas of shared expertise two communities meet to exchange goods and services in the midst of their significant differences. In his 1997 book, Image and Logic, Peter Galion used this approach to explore how two different research groups, scientists and engineers, each of whom used a very different brand of strategy— nevertheless were able to develop a common language (for a while) in their work,
which used radars and particle detectors. Applied to the contexts of peripheral science, the notion of a trading zone sheds light on how the cognitive activity initiated by a handful of peripheral scientists gradually creates an ongoing exchange with their metropolitan peers, *bringing into existence a shared area of contact*, within which research programs and scientific representations could be traded from one group to another. Items like this are picked up, extended, redefined (and perhaps modified) by the receiving culture. It is where different scientific communities meet and overlap in this way— and one culture receives the representations from another—that a substantial trade emerges between the two. A projection of this process onto an epistemic landscape leads us to see how a new group—in trying to move across the landscape of science in their search for significant research problems— seeks alliance and contact with other established scientific communities in order to establish a stock of knowledge. An aspiring new community would thus try to develop an overlap area— i.e., a trading zone—with other established scientific communities. Such a zone would in essence be a *network* that would link people and their skills, the trade along the network being carried out in terms of expertise, mental models and scientific representations in the making of new knowledge.

This analysis of a newly-emerging peripheral community which establishes its presence upon an epistemic landscape by trading with other, more established, peer groups calls for grasping the network properties of their trading zone. For the scientists who work from such a zone, it forms their social contexts of science, and naturally they rely upon its properties to produce, communicate, and, finally, to obtain validation for their work. Intuitively speaking, a new community in such a trading zone would try to do two kinds of things: firstly, it would seek to develop an identity of its own, announcing its presence, so to speak, to other communities, and secondly, they will seek to develop contact and communication with their distant peers, thus gaining, eventually, a share in their significant scientific problems. The solution of these problems they would trade in the form of the new knowledge that they have made (with their knowledge-constructing practices) to their metropolitan counterparts. The trade along this network will be sustained in terms of the solved problems, experimental expertise, and the consensus that they would receive from their peers. Analyzing the development of such a peripheral community thus means analyzing the distinct epistemic properties of this trade and this exchange.

A network like this is bound to contain some complex properties for such an epistemic network that grows by repeated transactions cannot be static— it *must exist in different states*. It is here that the notion of SEE becomes especially effective, shedding light upon the difficult contexts of peripheral science. As Gorman (2005) tells us, SEE links people of different research
communities in the form of a network, and has three kinds of stages. In state 1 of the network, the trade is minimal, and whatever exchanges are permitted, they are mainly controlled by a group of elites who hold complete mastery over the items of the trade. In a peripheral context, this is the analog of the stage of diffusion when goods from one culture arrive as items of a “civilizing mission” in the other. However, such a network can slowly evolve into a more equitable zone, during which time people from both sides of the boundary interact by sharing their expertise and their goods, contributing to the development of a shared system. A variety of hierarchical relationships may facilitate or hinder the state 2 network, but if one group come to dominate the hierarchy, the network may, once again, shift to a state 1. Finally, we can envision the possibility of a state 3 network, where as a result of very long and very persistent trade between the two communities, the communities come to share their mental models as well as their goals, and in such a trade the hierarchy is virtually set aside.

<table>
<thead>
<tr>
<th>Trading zone</th>
<th>State 1</th>
<th>State 2</th>
<th>State 3</th>
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<tbody>
<tr>
<td>Elite Control</td>
<td>None</td>
<td>Approximate Parity</td>
<td>Shared Mental Model</td>
</tr>
<tr>
<td>Shared Expertise</td>
<td>Orders</td>
<td>Interactional</td>
<td>Contributing</td>
</tr>
<tr>
<td>Communication</td>
<td>Orders</td>
<td>Creole</td>
<td>Shared Meanings</td>
</tr>
</tbody>
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Fig.3.2: The Three Types of Trading Zones and their Respective Levels of Expertise and Communication. Adapted from Gorman, 2005.

This analysis of the trading zones in peripheral practice in terms of a SEE network—and the evolution of that network by exploring the generative processes of its agents—show us how peripheral science can indeed contain a very complex structure. The complexity of its zones can be seen by noting a point raised briefly by Kitcher in a short essay about scientific controversies (2000). Broadly speaking, the objective of peripheral scientist is to make new knowledge in science—as well as gain consensus and intellectual authority for that knowledge—thereby developing new research programs and perhaps, in the end, establishing a new scientific community. But in an established field where many approaches and many practitioners already exist, what happens when one cannot make knowledge without involving another community? What if another community already controls the skills or the social structure without which scientific knowledge cannot be made? This important consideration shows us the

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56 This is a frequent situation in peripheral science, as I shall show below in Part II.
difficult aspect of the peripheral epistemic engagement, and the constant propensity of its network to shift towards the stage 1.

A network with such basic properties will of course contain many difficult bottlenecks, which shapes the trajectories of the peripheral scientists who work from within those contexts and begin their contacts with their respective metropolitan counterparts. Knowledge-making by peripheral scientists thus can be construed as *peripheries responding to new knowledge*, trying to set up, in effect, *a counter-trade of their own*.\(^{57}\) “Success or failure of (such) diffusion hinges upon the contextual perceptions of *science as a potential resource.*” (Shapin, 1983, my italics). Trades like this often begin with specific objectives in mind: in the Indian context for example, such a trade began when a number of young protagonists, who had brief exposure to some scientific research programs during their phases of colonial education or while traveling abroad, tried to contest the monopoly of the West in passing the final judgments on Indian culture.\(^{58}\)

Thus, the analysis of the network properties of peripheral science allows us to see how a peripheral community begins its journey upon an epistemic landscape in its quest for new knowledge in science. This perspective furthermore enhances our exploration of peripheral science in terms of its underlying generative processes: its conceptual spaces, its network, its trade, and the processes by means of which new concepts are constructed by its protagonists. Unlike the classical analysis of science which explains *all* scientific reasoning in the model of inductive or hypothetical reasoning and thus makes science everywhere alike—for all scientists are after all, identical agents—this new analysis of peripheral science shows how such scientists can be *different* in virtue of being placed upon a different epistemic landscape.\(^{59}\) A CPH analysis of peripheral science thus provides us with a more finely-grained exploration of such landscape, showing us, first, how such science can be done by communities of relative outsiders—but who still manage to make themselves contributors of scientific knowledge. The location of such scientists in their complex, peripheral environment shape their resultant scientific activity, showing why they often reach different conclusions, or use different styles in doing science. To take a concrete example that I will explore below in greater detail, the Indian

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57 Compare Abdus Salam’s speech on Renaissance of Sciences in Arab and Islamic Lands, “We owe a debt to international science, which, in all self-respect, we must discharge.” (Abdul Gani, p.178)

58 Such a nationalistic narrative is of course a prominent motivation for the peripheral scientists to want to contribute to the practice of science. The primary motivation for the Indian physics community in the early 1920’s was of this type.

59 Thanks to James McAllister for making this point (in email communication) that a proper analysis of peripheral science should show us how such scientists differ from their metropolitan counterparts.
scientist Raman routinely used a visual method in setting up his optical experiments, turning his own eyes, in effect, into expert photo-detectors. This was a function of Raman’s general approach to keeping his experimental set-ups inexpensive and affordable, as well as the abundant availability of strong sunlight in tropical Calcutta. Raman’s general eagerness to try out a new speculative theory, such as the quantum theory of light, merely on the grounds of its theoretical elegance, is another example of such peripheral innovativeness in doing science. The sensitivity of the peripheral scientists to their wider socio-political situation is readily attested, e.g., the Indian scientists were turning strongly towards German research, guided mainly by their nationalistic goal of trying to wrest the control of science from the hands of the British metropolitan institutions.

What I argued in the above, is that in order to gain a fuller insight into the contexts of peripheral science—and to arrive at a more informed judgment about such science and its goals—we can usefully employ a CPH account that grasps its scientific creativity. Such an account helps us to see this science from the standpoint of its protagonists and their problem situations. To explore the difficult task of making new knowledge from such peripheral spaces, we require first of all an understanding of the trading zones within which such a peripheral scientist begins his/her career, and how he or she engages in the task of creating new representation within that space. With the new tools of analysis that we have available today, we can trace the context of development of such engagement, their knowledge-constructing practices, and how they develop their networks of knowledge which link them with their metropolitan peers. This of course means that we explore the underpinning of such science at a concrete level—showing what kind of cognitive and social resources such scientists had at hand. Only after we have developed such a full-length account, can we further evaluate whether their outcomes were adequate enough to grant them their coveted status of being a progressive scientific community on par with a metropolis.

3.5. A Format for Analyzing Peripheral Scientists

What this analysis gives us is a new way of looking at the peripheral practitioners of science. Most reconstructions of peripheral science that we have today provide either of the two following kinds of accounts. One type is biographies from the various nationalistic standpoints where the peripheral scientist usually appears as a hero, working in the face of enormous odds, developing a research structure in science from scratch. A competing type of account, which appears often in the accounts of metropolitan scientists and historians, I shall call, for want of a
better term, *the peripheral scientist as a child prodigy*. G.H. Hardy recalling his time with Ramanujan, and John Stachel (2002) and Abraham Pais (1994) writing on S.N. Bose are good examples of this genre of scientific writing. The peripheral researcher who contributes a new perspective to metropolitan science appears to his peers as a wonderfully gifted child, in possession of all sorts of intuitive powers. While they marvel at his contributions, their faith in his continuing scientific abilities often remains shaky—and may even succumb in the face of a prolonged scientific controversy.60

Analyzing peripheral scientists by means of a CPH analysis allows us to develop a new kind of theoretical portrait of such scientists. Broadly speaking, it invites us to make two kinds of shifts from our current established analyses. First, it invites us to pay attention to the knowledge-constructing processes in peripheral science by means of which such scientists develop their contributions, e.g., the reasoning or the analogies that such protagonists employ in solving problems. Second, it requires us to pay attention to the nature of the knowledge-network they develop and how they are located within that network and the subsequent evolution of this network through its different stages. This allows us to grasp the problem-choices of these scientists, and the cognitive pathways that they make use of in developing a particular solution. Thus, we learn to see such scientists neither as intuitive thinkers nor heroes, but simply as cognitive agents, who, being placed in a difficult epistemic situation, engage with this situation by using all their available resources, thereby creating a concrete outcome. Thus, a CPH analysis of peripheral scientists allows us to put together a fine-grained account of such science by paying attention to the following three factors:

1. The problem situations that such scientists face in the creation of new knowledge and the knowledge-constructing practices that are available to them. This constitutes our first layer of analysis.

2. The nature of the trading zone that they develop with their metropolitan community and the shifting state of the network between the two.

3. The goals that such scientists seek in solving these problems, both for their research community, nation as well as for themselves.

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60 See Abha Sur (1999) on the prolonged controversy between Raman and Max Born over lattice dynamics in diamond crystals which lasted until the late 1960’s.
This portrait of a peripheral scientist that emerges from a CPH analysis gives us a theoretical language with which to describe how such a practitioner contributes in science despite his/her difficult location. In Chapter 1, I have already outlined that such scientists face an especially complicated task. Not only do they work on an asymmetric epistemic landscape where they hold less trust and much less authority in the making new knowledge, they also face constant possibilities of oblivion, or never reaching the stage of a consensus. Furthermore, they remain dependent often upon the metropolitan scientists for the acceptance and endorsement of their theories.

But yet, on the positive side, by taking up such a challenge, such peripheral practitioners bring something in general to the scientific practice. First, they often display very high levels of creativity, or an ability to look a problem in an entirely new way. They also often entertain a welcoming attitude towards new ideas and new theories, reaching out especially to those metropolitan scientists who are in the process of developing such novel research programs. (Thus, from the metropolitan point of view, peripheral scientists could be termed as good allies). Furthermore, they show often an ability to handle profound challenges. In sum, they look upon science as an adventure, seeing in science an aesthetic-theoretical form of inquiry, which draws them to it in the first place. Indeed, it is plausible to argue that a diverse population of peripheral and metropolitan scientists could be an optimal team for initiating research in new directions, for such collaborations has, in the past, often brought about significant gains in scientific knowledge.

### 3.6. Conclusion

I have argued that it is possible—using the notion of SEE or that of a network across an epistemic landscape that is capable of multiple levels of collaboration—to find a new framework and a theoretical language to explore the contexts of peripheral science. This framework captures the cognitive creativity of the peripheral scientists, giving us more room to explore their generative processes. To the best of my knowledge this is the first application of the CPH analysis outside of its standard Western contexts. Cognitively speaking, peripheral science has the structure of a trading zone, its epistemic activity setting up new knowledge-networks between two groups of people who formerly had few epistemic transactions with one another. Thus, peripheral science is both a response and a trade, albeit a trade that often proceeds only along very constrained lines. Yet, in spite of its constraint and its complexity, this does not allow us to draw the sweeping conclusions that are often drawn by the post-modern critics, such as
Nandy or Visvanathan. As Gorman (2005) tells us, the states in a network can shift over time. It is true that in the beginning of peripheral scientific activity, the trading zones of such science show themselves to be more adversarial rather than cooperative— the amount of trade in the beginning is small, and the approach is entirely top-down, and thus, necessarily, restrictive. Yet, with the increasing volume of scientific activity in those peripheral contexts, such networks—once they are established by a few handfuls of protagonists—contain the potential to shift towards a more collaborative arrangement, involving more protagonists and more scientific productivity, thus giving rise, over the course of time, to a science that is characterized by greater agency and greater productivity on the peripheral side. Peripheral science thus has more potential and is not necessarily an exploitative venture, and in spite of all its difficulties, it can still be called genuinely creative. In my next chapter, I shall propose a schema that will show us how this kind of a creative trading zone was established in the peripheral location of early 20th century India, when, within the adversarial formats of a colonial system, a few young Indian scientists began to organize themselves into a small research group in physics that contained— in spite of its obscure beginnings— the potential to evolve into a more agency-based network of scientific knowledge.
Chapter 4: The Structure of Knowledge-Networks in Peripheral Science: A CPH Analysis

“I shall in this paper attempt to illustrate… as to how knowledge conceived of within the epistemological framework of one culture is received, adapted, and absorbed by another culture.”

(italics mine)

—Kapil Raj, “Knowledge, Power and Modern Science”

4.1. Introduction: A Peripheral Scientific Community

In the previous chapter, I outlined an analysis of scientific practice that was developed primarily with the aim of illuminating the contexts of Western metropolitan science. With some suitable additions, I argued that this analysis can also be extended to the contexts of peripheral science. Such analysis would provide us with an insight into the dynamics of a scientific community—both at its individual and at its collective levels—primarily by outlining the cognitive pathways of the scientists who construct the new concepts and develop the new practices. The outcome of this analysis would be to highlight the epistemic agency of the (peripheral) scientists, showing how by means of such agency they create new scientific representations (or concepts and practices). Thus, briefly speaking, the goal of a CPH analysis is to show how the expertise or the embodied skills of scientists drive new developments in science. By showing us how the mental agency of the researchers creates new representations—which still maintain their connection with the older cognitive structures—we see how scientific knowledge moves from one stage to the next.

Philosophers like Nancy Nersessian and David Gooding use the CPH approach to demonstrate how significant conceptual changes can occur in scientific knowledge without giving rise to problems such as theoretical incommensurability. Gorman’s work in particular shows us how a network of expertise in science can take shape over time by means of *repeated transactions among its agents*, and how such a network become endowed with dynamic properties. The coming together of groups of people (and their expertise) gradually gives rise to collaborations among different communities who become partners in a trading zone. These zones might contain very differential levels of interactions but nevertheless *allow the possibility of a cooperative future*. Imagining peripheral science in this way allows us to form an image of what happens when new members of a nascent scientific community seek to form collaborations with
their already-established peers at a center in order to realize their goals of making new knowledge or attain the stage of a consensus.

To explore this process in detail, we now need a more expanded version of how a peripheral scientific community develops an interface with its more established metropolis. Such a schema helps us in visualizing how peripheral scientists become reliable producers of scientific knowledge from their difficult vantage points. Although we now have a growing literature that increasingly reflects the concerns of the post-colonial and the post-modern elements in science\textsuperscript{61}, a readymade schema for analyzing peripheral science is yet not available. In the absence of such a schema, peripheral science is still seen, usually, as an example of diffusion, its contexts rarely being used to illustrate creativity and productive reasoning in science.

My claim in this dissertation has, however, been that such science is creative in a genuine sense, personally as well as historically, being examples of what Kitcher calls epistemologically sullied science. But any science can assume such a sullied character once a certain kind of complex relationships comes to prevail among its contributors, e.g., when different groups in science participate in the making of knowledge from their respective vantage points with differential levels of epistemic trust and authority, eventually contributing \textit{differentially} to the final outcome. Peripheral science thus offers us a convenient window to study those processes at close range, giving us a clearer view of how growth of knowledge proceeds under those asymmetric circumstances. In this chapter my goal will therefore be to develop a schema of how a peripheral scientific community (that is either seeking to enter in a collaboration with a metropolis or has already began such a collaboration) establishes its presence by developing an interface with that metropolis—\textit{always in various stages and over a period of time}. It is by means of such an interface that peripheral scientists build their network with their metropolitan peers, which allows them to contribute more extensively in science. Thus, such a schema helps us in uncovering the epistemic agency of a peripheral community, especially when such a community is seeking to enter the field of scientific research for the first time.

In order to be a useful format for such an analysis, a schema like this must do a number of things. It should tell us how the cognitive changes—once initiated by a few peripheral researchers—become the basis for an \textit{expanded network} of expertise between two communities. The creation of such a network creates the possibility of epistemic gains on either side: new ideas and solutions on the metropolitan side, and (possibly) the beginning of a new research tradition on the peripheral side. Communities that become partners in such a network can come from

\textsuperscript{61} See Harding (2003) on the issue of the post-modern and the post-colonial concerns about science.
widely dissimilar contexts, e.g. from different cultures, nations and institutional settings, and yet, they establish a productive network of developing new knowledge (such as extending a current research program). The question is: how is the knowledge produced by a peripheral group received on the other side? How do peripheral researchers work on a research program from their respective contexts and what is the dynamic of exchange that comes to operate among the two? Encounters of this type have often produced significant gains of knowledge (on either side), but we do not yet have any format of analysis that can show us the cognitive activity implicit in such encounters on the peripheral side. Since most of our analysis of scientific knowledge focus currently upon the contexts of metropolitan science, an analysis for the peripheral contexts is not readily available, and thus, in the rest of this chapter, my goal will be to sketch a framework within whose purview such science can be analyzed, allowing us to see what happens when a scientist from a peripheral group seeks to join a more privileged scientific community for the first time in order to become a contributor in scientific knowledge.

4.2. Central Science and Peripheral Science: Newly-Emerging Scientific Communities in the Non-West

The first step in this analysis is to clarify the two contexts of science that I have been speaking of all along: the contexts of a peripheral community and that of its (corresponding) metropolis, and how the making of scientific knowledge differs between the two. Whilst our mainstream analysis of science gives us much information about the metropolitan contexts, peripheral knowledge-making—which takes place under significant constraints—remain little discussed. Thus, before we can discuss the peripheral scientific creativity in the form of case studies, we need a general schema that can help us unpack such peripheral encounters: showing the cognitive agency and the efforts that such a peripheral researcher brings into these engagements. In trying to see how a new scientific community thus begins its journey on the epistemic landscape of science towards its successive stages of collaboration with a resource-rich and privileged, metropolitan community—developing for itself a capital of expertise and a collection of exemplars in the process—we see how an emerging research community gradually becomes a contributor to scientific knowledge.

Who exactly is a peripheral scientist? As I explained above in chapter 1, peripheral science is science done in the capacity of a newcomer or an outsider—it is a contribution made to science by someone who works with another community (or with a single researcher in a
limiting case), but without being perceived as a full-fledged member of that community. By defining peripherality in this way— in terms of being an outsider to an established community— I am of course making the claim that peripherality is best understood in terms of its epistemic impact, i.e., in terms of how it affects the knowledge outcomes in a community during its attempts to develop a new practice. Peripherality is thus more than just a matter of psychological or social exclusion. Therefore, what I am trying to put together in the rest of this chapter is an epistemic analysis of peripherality— how peripherality shapes the work of the practitioners (or that of a community) during their attempts to contribute to scientific knowledge. As I have already argued in chapter 1, the social organization of science, which is supposed to neutralize peripherality, can sometimes end up reproducing peripherality.\(^62\)

Given this analysis of peripherality, a peripheral research community need not mean only those who are separated by a geographical distance from their main community. Geographical distance can of course increase the chances of being peripheral, but physical distance alone is not the true marker of peripherality. Neither does the complexity of modern science, which often distributes the cognitive labor of research among many different groups, make a community peripheral. The crucial markers of peripherality can be seen when one community stands in an unequal epistemic relationship with another, i.e., when one community cannot make knowledge without involving or collaborating with the other group, and when their work remains dependent upon the assent or the endorsement of that more privileged group before it could become incorporated into the corpus of normal science.\(^63\)

This raises the question whether such peripheral scientific communities can be progressive in any significant sense, for progress in science is often associated with independence in inquiry.\(^64\) It is outside of the scope of this chapter to discuss the thorny issue of progress in detail, but an intuitive instrumental notion of progress, I believe, can tide us over the problem (for the time being). While it still remains a controversial issue in philosophy of science about how scientific progress ought to be understood in general, for most practicing scientists progress

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\(^62\) A nice parallel of this situation also exists in matters of education. Education, long believed to be our main tool in equalizing opportunities in society— and thereby end all inequalities— can sometimes end up reproducing inequality due to students’ differential access to the institutions of education. See Annamalai (2004), “Nativization of English in India”, Journal of Language and Politics, p. 158, on this point.

\(^63\) Recall my earlier distinction in chapter 1 why graduate students cannot be classified as peripheral members to a practice. While graduate students are dependent on the senior professionals, maintaining a graduate program is also vital to the career path of the senior members themselves. Thus, the asymmetry is more apparent than real.

\(^64\) Independence of inquiry in such contexts might mean a mixture of several things, being able to solve a problem by relying on the resources of one’s own community, being able to train new members to carry on the practice, integrating a part of the cultural traditions in the practice of science etc.
translates into those accumulations of knowledge that enhance or increase their ability to take on (and thereby solve) more research problems. It is in this instrumental sense that a peripheral community can be progressive (as I shall try to show in the pages below), especially if its projects of collaboration with its central community is going particularly well. But very broadly speaking, from the peripheral point of view, this divides scientific communities into two groups: those that work where people and their knowledge are highly concentrated, i.e., the resource-rich, metropolitan communities, and those that work from places of lesser knowledge concentration—the so-called peripheral groups in science. In this chapter my aim would be to explore the epistemic efforts of the latter group, asking how they manage to put together a newly-emerging network of expertise such as SEE, which is essential for their becoming a contributor to scientific knowledge.

While the metropolitan centers of science are considered to be located mostly in various Euro-American centers, the wider, sparser network of peripheral communities is spread over a larger part of the globe. When this social arrangement is projected onto the epistemic landscape of science, what we see is a differential clustering of communities rather than a set of communities all of whom command nearly-equalized levels of epistemic resources for making new knowledge. Much of late 19th and early 20th century science actually developed within such a landscape, and thus, a general understanding of this kind of scientific situations is at least called for.

To repeat the point that I have just now made in the above, as well as earlier in chapter 1: the landscape of scientific knowledge is not uniform, rather it is organized into differential clusters. The researchers of the latter types remain distant, often socially and psychologically, from their metropolitan counterparts as well as from its invisible colleges, but they still seek a foothold into the network of research. But their distance—whether social, geographic or

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65 See Laudan (1977) on a general discussion of scientific progress as problem solving.

66 Also see Alexander Bird (2007) for a detailed list of how many kinds of progress can take place in science.

67 This division would of course be more visible on the peripheral side than from the main community. From the main community’s point of view, such scientists are just members who contribute to their practice only from time to time.

68 Peripheral science can of course be examined in at least four geographic contexts: South Asian, Latin American, African and the eastern European. This dissertation and its case studies are of course drawn from the South Asian context but other contexts still need to be explored.

69 Abdus Salam’s legacy in establishing an international research institute in theoretical physics at Trieste, Italy, for Third World physicists may be considered a practical solution of this problem.
institutional—places on them various constraints of peripherality, influencing the outcomes of their epistemic activity.

Given this set-up, how should we conceptualize the different stages of peripheral engagement—and of its details—when the work of making new knowledge is distributed between a metropolitan and a peripheral group, and between the two lies an inequality of resources, and thus, the possibility of an unequal epistemic outcome? How should we conceptualize those scientific endeavors when a few peripheral researchers strive to produce progressive science, seeking to found an autonomous research tradition in science (for their home community) and thereby gain a foothold in scientific knowledge? How do these communities create and sustain their encounters with their metropolitan counterparts, by what methods are their outputs validated, and do such communities ever get to create a new paradigm in science?

In the following sections, I shall outline a schema of peripheral engagement (and that of peripheral agency) that I claim is at work when such a group of scientists join the network of scientific practice with the intent of contributing to new knowledge. The cognitive agency of such scientists contributes to the reception and development of new representations, as well as in their putting together of a research program, and the efforts to find a consensus.

Earlier, in Chapter 3, we saw how Collins and Evans (2002) and Gorman (2005) explained the concept of SEE (Studies of Experience and Expertise) as a means of explaining the formation of a scientific network among diverse groups of people who, somehow, learn to speak in one another’s language, switching back and forth between the two groups. SEE can of course exist at various levels, involving different degrees of collaboration among scientists and their collaborators. Collaboration could be altogether absent in a situation, or could be present at the most minimal level. On the other hand, it could also scale up to a level where the production of knowledge becomes highly collaborative in nature. Note that these changes imply different kinds of trading zones, each with its own distinctive network in scientific knowledge. A shared expertise that involves many collaborative exchanges may characterize science at its highest third level, but most peripheral scientific communities do not—as a rule—begin their career from such happy high grounds. More commonly, their first scientific endeavor with another community begins with a set of interactions that are often quite non-egalitarian in character. However, states in a network can always shift over time, developing changes in their subsequent interactions. In his 2005 article for instance, Gorman draws our attention to the example of an adversarial trading zone existing between a number of AIDS activists and that of professional scientists which, eventually, took a happier turn. After an initial unfriendly stance towards one
another, both these groups were able to overcome their mutual lack of trust towards each other, thus developing—eventually—a shared zone of expertise.

4.3. Forming an Interface with a Metropolis: Knowledge Networks at a Periphery

Two striking situations in peripheral science that immediately call for analysis are moments of discovery in a peripheral context and those moments when a peripheral scientist initiates a new conceptual change that, eventually, alters scientific thinking at the metropolis. As I have argued above in chapter 3, such changes can be illuminated further by using a cognitive analysis that explores (and reconstructs) the knowledge-constructing practice with which such scientists negotiate these encounters and produce an outcome. In the case studies that I shall present below, I shall attempt to give an example of each type of peripheral contribution—the extension of an existing research program leading to a new discovery and the creation of a new scientific concept by a peripheral scientist. In each case, I shall seek to show how different forms of reasoning, e.g., visual reasoning or abstraction, were operative in those encounters, precipitating the formation of a knowledge-network between a peripheral and a metropolitan community. For example, in my study of C.V Raman—who discovered a new form of radiation in 1928—I shall show how a peripheral scientist achieves a major result by extending a metropolitan research program, and anticipating others in that discovery. Similarly, I shall also show how S.N. Bose created a new concept in quantum statistics that brought about a new revolution in metropolitan physics (but surprisingly left him out of that revolution).

My goal is now to outline a scheme of peripheral engagement which gives us the different stages by means of which a peripheral scientific community develops an interface and a network of expertise with its metropolitan peers. Such a format gives us a general sketch of peripheral engagements and its possible outcomes. Creativity in such situations clearly has both a cognitive and a social aspect—involving first, the manipulation of representations, creation of concepts or the use of skills but also interests, connections and power relations. In his 1994 article “Mind, Society and Growth of Knowledge” Paul Thagard provides us with such an integrated schema of cognitive/social reasoning in science, which is supposed to capture how a scientist uses his mental representations by relying upon the contexts of his social resources (Thagard, 1994).70

70 Thagard’s integrated schema runs as follows:

“a. The scientists’ had a set of mental representations that includes a set of previous belief and a set of interests.
Thagard makes the intuitive point that a proper analysis of science should include both contexts, an insight of which I shall make good use in analyzing the given peripheral contexts.

Since the phenomenon of interest is here the trading zones of peripheral science and how a peripheral scientist creates a new epistemic product within such a zone, the schema must tell us how a peripheral scientist forms an interface with his/her metropolitan community and what role such an interface plays in his subsequent professional work. This implies that our schema must be psychologically sound and historically plausible, i.e., it should be consistent with that individual scientist’s historic choice of research problems and his known research style. The need to re-create such research styles and the requirement of psychological plausibility brings in much empirical detail, taking us out of the domain of a pure logical analysis.

To sketch out the structure of such a knowledge-network, I begin with the idea that a consolidation of science within the peripheries begins with the reception or manipulation of a number of research programs that a peripheral group receives (and grasps) through a series of historical encounters (this can be either at home or abroad). Those acquired research programs are then manipulated by them using a variety of mental, representational and experimental resources (e.g., model-based reasoning, analogy or abstraction), thereby developing a set of specific research questions. In other words, formulating a research program is less a case of receiving ideas from the outside, and more a matter of agency and activity on the receiving, peripheral side (which creates those expanded research programs). Unlike Kuhnian paradigms, which often function as research orientations operating below the level of consciousness, the notion of research programs can be understood at a more explicit level, as consciously held units of information (and expertise) consisting of both theory and information, and thus capable of traveling from one place to another through the agency, exchange and communication of human beings. In the Indian context for example, encounters with groups of Western research programs began in the context of a colonial education system, a distant tracking of the metropolitan research literature by means of reading its journals and textbooks, or perhaps an occasional brief stint at a metropolitan laboratory. Thus, much of what normally goes under the name of

b. The scientists’ cognitive mechanism included a set of mental procedures.

c. The scientists had social connections and power relations.

d. When applied to the mental representations and previous beliefs in the contexts of social connections and power relations, the procedures produce a set of acquired beliefs.

e. So the scientists adopted the acquired beliefs.”
'diffusion' should instead be re-translated as 'reception' or 'redefinition', i.e., the grasping of new research programs that are thereafter legitimized for use by a receiving peripheral group.

The notion of a research program (RP) was first introduced by Imre Lakatos (1977) to show how science acquires its dynamic and progressive character. Stated simply, a RP stands for a high-level constellation of concepts, or a group of fairly high-level theories and their associated experimental and mathematical expertise. Taken together, those engage the sustained attention of a practicing scientific community. Thus, a scientific practice can be analyzed in terms of a group of research programs and how those programs are extended or modified by the knowledge-constructing practices of the individual scientists.

This analysis of science into a number of smaller, more manageable, and more temporally developing units of scientific activity and achievement provides us with a way of understanding how units like this could be grasped and manipulated by a new community in a new location, using the a mixture of various forms of reasoning, embodied skills or practices.

Applying this rough understanding of how research programs are received in the contexts of a peripheral scientific community, the reasoning, experimentation and the communication used by a peripheral researcher consists in manipulating those RPs (at different stages). Unlike his metropolitan counterpart, who receives his RPS through the smooth process of being introduced to them by the veterans of his community, the peripheral scientist comes into contact with his RPs through a series of historical events, and rarely by the established procedure of being trained by the veterans of his own group. For example, many of the pioneer scientists of India were self-trained in one sense or another, and their contacts with the metropolitan chains of knowledge were often indirect and remote.

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71 “The typical descriptive unit of great scientific achievements is not an isolated hypothesis but rather a research programme” (Lakatos, 1977). Examples of such programs can be Newton’s theory of gravitation, quantum mechanics, Einstein’s Relativity theory, or even Freudian psychoanalysis. Specific episodes of science can also be counted as research programs, e.g., Bohr’s theory of the atom or the Michelson-Morley experiment.

72 The job of a scientific community consists, according to Lakatos, in interpreting greater and greater chunks of the world in terms of their research programs. Since at any given moment this job of interpretation remains incomplete, all research programs are necessarily surrounded by a sea of anomalies, and the main work of a professional scientific community consists in trying to resolve those anomalies into 'resounding victories'. Provided such efforts are successful, the research program attains a state of progressiveness, but if there are persistent failures to achieve a satisfactory fit, the program may degenerate, or may even be abandoned altogether. An apparently degenerating RP can however, always stage a comeback, thus making the progress of science a very complex process. Furthermore, new research programs with few adherents may suddenly gain new converts, following a resounding success achieved by some researcher. A RP thus signifies a number of (mental) models about the world, and some standard ways of handling those mental models. The training of a scientist largely consists in absorbing those models, and in learning to think of problems in their terms.

73 However, this may be possible at a later stage when a considerable trade in expertise has already taken place.
Manipulation of such RPs of course will require a variety of mental (as well as experimental) procedures. Keeping close to our intuitive model of peripheral engagement, I shall suggest that such manipulation involves at least three kinds of basic steps: *that of legitimation, extension and response*. Legitimation of an RP begins when it is picked up by a new research group on account of its novelty and its power. *The trade that is initiated thereby is a trade in mental models and of their associated instrumental and mathematical practices.* Legitimized research programs that have thus been picked up by a new community are then extended into specific research questions, the solution of which then becomes the chosen goal for a peripheral researcher (or his community). This is the stage of response (to scientific research programs), during which a research problem of some sort— theoretical or observational— becomes the focus of attention by that group. This stage of response requires of course that a peripheral scientist employs all the resources available at his or her command: manipulation of representational structures, using different forms of model-based reasoning, or developing new experimental protocols— whether learned at home by self-search or at the different European metropolitan centers—in the solution of his problem. The specifics of his engagements often show how chance elements, such as an analogy, can be used productively by such a researcher to sharpen his focus. Raman’s search for a lower-frequency radiation in a light scattering experiment for example, was motivated by his belief that he was looking for was an optical analog of the recently-discovered Compton Effect. Even though the final explanation of the radiation turned out to be a different one (than the Compton Effect), the analogy between the two was still a great motivator in Raman’s discovery process.\(^{74}\)

Summing up the initial stages of a peripheral encounter with its set of RPs — to which the peripheral scientists’ exposure might have been very piecemeal or partial, but which he nevertheless is determined in using as building blocks of his research— we obtain the following intuitive schema:

\(^{74}\) Indeed, in his Nobel Lecture in 1930, Raman mentions this as the starting point of his research.
So far, I have been talking about only of those processes which involve the use of cognitive resources—such as reasoning, analogies, mental modeling, or the use of experimental or mathematical procedures that allow the extension of a problem into a specific research question. But we must now focus on the goals of those researchers—why they devote themselves to the solutions of such problems in the first place, and what objectives they seek to achieve by means of such research outputs. Intuitively, a peripheral scientist—once s/he develops a research question (and obtains a solution) – seeks recognition for his work and a metropolitan consensus. What s/he wants by this is to become a contributor to scientific knowledge. However, in order to make contributions in science, s/he must go through a process that occurs outside of his or her home community.

Recall that the epistemic dependence of a peripheral scientist implies that such a scientist does not live in a social matrix where s/he can generate a consensus (for his/her work) merely relying upon the internal resources of his/her community. Thus, it follows that the internal aspect of his/her cognitive activity—the three steps of which I have just outlined—must be supplemented by another stage, essential for building a consensus, but which, given the contexts of peripheral science, takes place outside of the peripheral scientist’s home community. Knowledge, made by a peripheral scientist, therefore has to travel outside of his/her home community in order to be received and endorsed by another group—typically, by his/her peer group at a metropolis—thereby becoming a part of their social interactions. Whilst the internal activity of the peripheral researcher consists in developing a research program and a suitable

<table>
<thead>
<tr>
<th>Legitimation</th>
<th>Grasping of an RP. Deciding to focus one’s attention on to it.</th>
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<tbody>
<tr>
<td>Extension</td>
<td>Deriving a research question from an existing problem.</td>
</tr>
<tr>
<td>Response</td>
<td>Using mental procedures such as analogy, inference or embodied skills such as instrumentation etc.</td>
</tr>
<tr>
<td>Outcome:</td>
<td>Could be either a success or a failure for peripheral science is dependent upon the social evaluations done by a metropolitan community.</td>
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</tbody>
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Fig.4.1: How a Peripheral Scientist Develops a Research Program
research question, the second phase of his/her effort is geared towards reaching those centers of consensus, and gaining their acceptance. The most difficult parts of the peripheral endeavor begin at this second stage. Since knowledge cannot be made by a peripheral researcher without collaborating with a metropolitan community, it is only by means of their acceptance and endorsement that a peripheral scientist finally attains the stage of being a normal scientist. Their endorsement allows him/her to develop an interface of expertise and the setting up a network of knowledge—a trading zone by means of his/her research can prosper further. It is by means of such work and such zones that a peripheral researcher gains his/her first footholds in science—and makes a place for himself/herself as well as for his/her community—from where s/he can continue to make further ongoing contributions to science. Note that in contrast, the metropolitan researcher is not obliged to collaborate with a peripheral researcher for making new knowledge.

How shall we think about the process of building a consensus in the contexts of peripheral science? As I said above, the knowledge-making activities of a peripheral scientist do not entirely take place within the contexts of his/her own community—since his/her own community does not yet carry the required levels of trust and intellectual authority. Thus, their assent, by itself, does not yet constitute a scientific consensus. While the peripheral communities can certainly ‘contribute’ to knowledge, a ‘consensus’ for their work requires an acceptance and an endorsement by their metropolitan colleagues before the work of such scientists could become incorporated into the corpus of normal science.

To make this process intuitively obvious, I use a diagram below to show the structure by means of which a peripheral scientist establishes an interface with his/her metropolitan community. The diagram shows us how a peripheral group becomes part of the social structure of a metropolitan community in order to form an interface with it. Once such an interface is put into place, this becomes the platform for more peripheral contribution. Simply put, the metropolitan social structure contains a knowledge-system with an existing structure. Once new knowledge is made, it is picked up and ‘reported’ to other groups by means of this structure. This may be colleagues in one’s own disciplines (as well as other disciplines), public intellectuals, and finally, those who can make use of such knowledge in the form of developing new technologies. This matrix of science is pervaded by an intellectual milieu (IM) that supplies different cognitive resources such as judgments, ideals or tastes (Radnitzky, 1970).

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75 The stage when such a scientist constructs a research question can occur in his/her home contexts or while s/he was briefly visiting a metropolis.
Before a peripheral scientist could be awarded a consensus for his work, he needs to become a part of this structure that is present within a central community, thereby becoming, in effect, a part of their structure of evaluation and assessment. This process of building a consensus thus calls for a collaboration between a metropolitan and a peripheral scientific community. This is what a peripheral researcher must develop, and more importantly, must sustain over time. Once formed, this interface becomes the platform for his or her further research, and allows a peripheral scientist to function as well as maintain his/her epistemic status as a scientist.

Two things must be noted in this peripheral endeavor. First, the need of peripheral scientists to make progress—in the sense of solving more and more research problems that gives
such a scientist a persistent professional presence. But equally importantly, this also implies that they need to make this progress in lockstep with another metropolitan community from which the peripheral researcher hopes to receive a consensus. This metropolitan consensus is thereafter used as a foundation to build a secure tradition of research that provides a basis for such a researcher at home. Because the output of one cycle of knowledge-making activity is (partly) utilized as its next input, the entire process can be described as a feedback loop. This somewhat convoluted process of reaching a consensus—by being a part of the social matrix of another research community and by creating a stable interface of research for oneself via their consensus and their endorsement—can be described as an iterative process. Success in obtaining a metropolitan consensus clearly enhances one’s success in becoming part of a viable scientific community at home, which can then undertake further ambitious episodes of problem-solving. Historically speaking, a peripheral research community reaches a state of progressiveness when, in a given historical situation, these two stages mesh into each other smoothly in the form of a loop, thereby giving rise to a stable basis of research at the home base of the peripheral researcher. With every successful cycle of the loop shown in Fig. 4.3, a peripheral research community gains more stability, which further enhances its ability to make more contributions, and possibly, creating enough exemplars to introduce new subject matters and new applications of science on their own. When a research community can thus generate its own subject matters and applications, and can produce the necessary consensus for its next cycle of scientific knowledge-making, it gains the status of an autonomous scientific community.
The diagram below shows how a peripheral community makes progress via such collaboration with a more privileged metropolitan scientific community. Note however, that this arrangement already contains the seeds of (some) instability. In other words, the interfaces that are built by the peripheral researchers with his/her metropolitan peers may not be permanent in nature and may not be always available as a platform for his or her future work. Interfaces like this may erode over time, especially if the concerned metropolitan community shifts its research emphasis (and its knowledge tradition) to something else, which leaves the peripheral community with a body of research that draws increasingly diminishing responses from the metropolis. While the interface lasts, however, the successful completion of the loop propels a peripheral community towards its next stage of progressiveness, but if this process goes wrong (i.e., if the loop fails), it creates a setback for the peripheral researcher, landing him with a failed research program.

Fig. 4.3: The Feedback Loop of Scientific Progress for a Peripheral Research Community

It is by means of repeated iterations of this type that a viable new scientific community is born at a new social location. What drives this process, i.e., the dynamic element behind this science, is the cognitive activity of the peripheral researcher—his/her inferential capacities, his/her instrumental skills, as well as his/her ability for social and scientific communication. It is
this process and the cognitive activity embedded in that interface that can be illuminated by a CPH analysis, which allows us—for the first time—to develop a clear account of the cognitive activity implicit in peripheral science. As R.N. Giere tells us (in the contexts of metropolitan science): “Theories simply do not develop; they are developed through the cognitive activities of particular scientists. It is the focus on the scientists, as real people, that makes possible the application of notions from cognitive psychology to questions in the philosophy of science.” (Giere, 1992, Minnesota Studies, vol. XII, p. xviii). The same insight holds true about peripheral science as well.

Thus a CPH perspective, which provide us with a tractable format for analyzing the knowledge-constructing practices (as well as the social interfaces) of peripheral science, shows us how new communities could become, eventually, part of the knowledge-making enterprise in science, and how scientific reasoning—suitably extended to include analogies and other forms of model-based reasoning including visual analogy—can give us a handle in understanding the mental processes of the peripheral researchers. The strength of the CPH account lies in the fact that it allows us to see the cognitive activity of the peripheral researchers during this engagement, showing us how such a scientist receives, develops and extends certain research programs in order to contribute to new knowledge, and how, in that process, s/he creates a new tradition for his/her own home community. This makes visible (for us) the cognitive agency of those researchers, thereby actually producing a more illuminating account of peripheral science.

The schema that I sketched above now needs to be cashed out in the form of case studies of peripheral science. This is the task I set myself in part II of the dissertation. In the next two chapters therefore, I shall sketch two case studies from the history of Indian peripheral science during the period of 1910-1930 by focusing mainly on three questions. First, I shall explore why certain kinds of problems became significant for those peripheral Indian researchers, i.e., why they focused on those problems and not on others. Second, I shall ask what kind of cognitive tools they used in the work of developing their solutions: the arguments, reasoning procedures and the mental models. In this task, naturally, I shall make use of the expanded view of scientific reasoning that I have outlined above, according to which mental processes such as simulative modeling, analogy, and visual imaging can all count as important constituents in scientific inference (in addition to the standard deductive or inductive arguments). S.N. Bose, for instance, was strongly motivated by his mental imagery of blackbody radiation as a free radiation gas, and it is this image that did most of the inferential work in his proof of Planck’s law. Similarly, Raman relied upon a strategy of visual reasoning during the design of his experimental set-ups, which allowed him to infer quickly from his experimental data to new models of the light
scattering. Finally, I shall consider the knowledge network by means of which these research outputs were exported to their metropolis to reach a consensus and be accepted into normal science (or alternatively, rejected and set aside). The result of this analysis will be to allow us to see what kind of science was produced by such early peripheral efforts and what were the aims of this science. Was this science simply an imitative endeavor or could it be called independent? In what sense were they able to start an autonomous tradition of scientific research in India? What were their strengths, their motivations, and their persistent roadblocks?

This reconstruction of the ‘creative processes of scientific discovery’ within the contexts of a peripheral scientific community will allow us to see how a peripheral group builds an interface with its metropolitan peers under their constrained conditions, and yet is able to use that interface productively as a platform for further research (at least for a while) by means of which more problem-solving can be undertaken. In other words, though modest in beginnings, such new efforts do contain the first stages of a network that eventually can become more collaborative, thus starting a new self-replicating scientific community.

Responding to a body of transmitted concepts is thus not a passive process, but involves a considerable measure of cognitive activity on the part of the responding peripheral side. First, it involves extending a number of research programs to reach particular types of goal. To be sure, this process begins with an input that comes from outside—in the Indian context for example, such processes began in the form of a cross-cultural encounter. Thus, there is a surface similarity in my analysis with the diffusion model, according to which science is viewed as something which is transmitted to a community from the outside. But note that the CPH analysis actually tells us the opposite: the mere presence of a number of RPs on one’s horizon does not, in itself, guarantee the formation of a scientific community. To create a new tradition of scientific research, a nascent community must begin by playing the game of science—by engaging with its problems, by responding to its concepts, by putting together a new epistemic machinery in the form of an institutional and a social network that can in the future replicate itself in the form of more research efforts. It is by means of such an activity that a distant research program is gradually internalized so as to become one’s tool for work in founding a new research tradition.

Characteristically, such peripheral processes are threaded tightly to their wider social situation and often use its elements to build a new national identity. In the Indian context for instance, for want of other options, the pursuit of theoretical knowledge had became, for a while, a way to challenge the limited opportunities of a colonial state, thus contributing to its intellectual productivity. For the small scientific community in India, the peripheral period of 1910-1930 was
indeed an exciting time, despite their small size and their not-so-promising location at a colonial periphery.

In the next three sections, I shall sketch some emerging accounts of peripheral science, finally summing up the basic trends of Indian peripheral science between 1910-1930.

4.4. Models for Emerging Scientific Communities at a Periphery: Some Historical Sketches

Apart from the predominantly diffusional model of peripheral science, there have been some recent sociological and historical contributions that allow us to pay more attention to how peripheral communities engage with Western science in their various contexts. A good sketch of the formation of a scientific culture in Japan is given in Bartholomew (1989), and in her well-known account of Japanese high-energy physicists (1989), Sharon Traweek provided us with an analysis of how hi-tech science is done in a non-Western context in Japan. More recently, Xiang Huang (2005) has provided us with an account of the Jesuit dissemination of scientific knowledge in China, and similarly, Kapil Raj (2007) provided us an account of how a flow of knowledge started between Europe and the Indian subcontinent during the period of 17th-18th centuries, contributing greatly to the European stock of knowledge.

But more detailed and more context-specific accounts are now needed to flesh out how scientific knowledge travels through space when it is taken out of its standard Euro-American context, and how these provisional models of peripheral science can be fine-tuned to suit different contextual settings. For instance, in the colonial context of India, the concept of a trading zone must be qualified to accommodate the presence of a “civilizing mission”, a standing feature in most colonies. The colonial peripheries thus contain always a disparity of power, and this disparity of power is reflected in the circulation of scientific knowledge and in the shared development of new knowledge. Furthermore, it also shapes the formation of a scientific consensus and the development of scientific controversies.

In the specific context of India, a semi-historical account has been proposed by several historians and sociologists, such as Gyan Prakash (1999), Rajesh Kohar (2005) and Deepak Kumar (1997), spanning from the decades of 1876 to 1930, which claim the existence of three major stages.

Scientific activity, as this account goes, first began in the sub-continent in the shape of numerous field enterprises, such as the different geological or botanical surveys, undertaken by
colonial officials in their attempts to organize and map the subcontinent. In the second stage, these largely European field enterprises were sometimes supplemented by a few peripheral natives, e.g., when some Indians were inducted into those surveys as junior assistants or helpers. Such people might have received occasional compliments for efficiency from their colonial superiors, but since they neither determined the agenda of research, nor contributed any personal exemplars to those efforts, such inclusions carried little—if any—scientific import. From the perspective of the formation of a national research tradition, it is only the third stage—the formation of an indigenous community—that counts as a significant beginning of a research activity.

Even though such accounts give us more information about peripheral science, they still show some important kinship with Basalla’s three-stage model, according to which the non-West simply received a stock of knowledge in the capacity of a passive recipient and that stock of knowledge was what produced a new community. Looking at scientific knowledge by means of this transmission model, all the agency and activity in peripheral science becomes obscured, and we are encouraged to see the growth of science in terms of an import, and not a natural cognitive response. The movement for an alternative science is a direct consequence of this mind-set, for it declares that it is impossible to contribute to high European science from a peripheral perspective. Furthermore, since science was the tool of hegemonic political powers, the later inclusions of (some) indigenous people in this activity cannot fundamentally alter the nature of the activity. This view makes the researchers from the non-West working at something that has no conceivable link with the rest of their own tradition.

In contrast, I have emphasized the agency and activity of the peripheral researcher, who, when placed in a difficult cross-cultural context, uses the representations or the mental tools that he has available through a hybrid mix of historical events, and makes creative use of reasoning and analogies in order to grasp, manipulate, and redefine a body of imported knowledge. Thus, my analysis shows that peripheral science is a natural cognitive response (of a society or a group) towards a body of newly-received knowledge, which it seeks to turn into a new resource for its own future use, and as such, such effort can be as creative as science done anywhere. Such

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76 William Jones for example, hired many Indian artists and collectors in his attempts to collect and classify Indian botanical plants.

77 But see Subrata Dasgupta, 2007, chap. 6, for some borderline cases, such as Radhanath Sikdar and his alleged measurement of the height of Mt. Everest.
Science too involves representational and social processes—even though such processes operate within a rather constrained social matrix. However, (as I shall shortly show below), the many bottlenecks and the difficulties that such scientists face along the way makes it harder for such communities to reach their goals of making new knowledge and of professional achievement. It is in bridging the distance between making a new beginning—and in its difficult execution—we see the creativity of a peripheral scientist.

Clearly, unpacking such a project requires thinking about human reasoning in a different way, and inquiring how such reasoning functions when a new import requires to be adapted within the settings of a different culture, and how, once legitimized, it is practiced there by a new group of people.

Before proceeding to the second part of my dissertation, let me consider, briefly, an objection against my general project of analyzing such science from a cognitive point of view. A relevant objection to my project could be that a cognitive-historical analysis is a too artificial format for addressing such contexts because such analysis tends to reduce concrete thought processes to a set of boxes and algorithms.

My reply to the above is to argue as follows. First, contrary to what appears at first sight, a philosophical analysis of the cognitive strategies/contents/activities of those peripheral scientists need not be couched in terms of an algorithm that seeks to reproduce that activity in the shape of a flowchart or a series of boxes. A more flexible cognitive account (of peripheral science and the contexts of its development) can certainly be provided by showing how vaguely felt problems got articulated into theories or specific results, how they were developed by means of a specific model-based reasoning—to be presented for communication and persuasion—and how those responses were accepted or rejected in the form of a consensus. Thus, by unpacking the cognitive and the social processes behind the scientific encounter at the peripheries, we see how the activity of a few individuals—in engaging with a new body of knowledge, and in that process re-defining and internalizing them—create, in the end, a new epistemic machinery for a society. To that extent, peripheral science is a natural cognitive response of a society accepting a new body of knowledge.

4.5. A Peripheral Scientific Community: British Colonial India from 1910 to 1930

The cognitive-historical analytic schema that I have developed in this chapter must be used to explain specific episodes of science. In this dissertation, my choice of a specific periphery
will be British colonial India and its small pre-independence physics community during the years of 1910-1930. This was a time when India had begun acquiring a tradition in the university-based sciences, but was still a community lacking in any significant critical mass. Thus, the focus of my analysis will be on those scientists whose work spanned the first three decades of the 20th century, and who comprised one of the first professional scientific groups on the Indian subcontinent.

For a number of reasons, this period of Indian science serves as a good candidate for a CPH analysis. Thanks to the existence of the British colonial state, a Western-style educational system had been implanted in India for some time—wiping out, in the process, most of the society’s organized traditional knowledge in Sanskrit and Persian. During the decades of 1910-1930, a small group of (mostly self-trained) physicists gathered around a newly-formed institution called the University College of Sciences, Calcutta, inspired by their nationalistic goal of founding a new culture in scientific research. Deeming such science essential for modernity—and perhaps a replacement for the lost knowledge of their society—this small group of scientists sought to find their first foothold in the epistemic structure of scientific research. Since most of these scientists were physicists, in this dissertation I shall be concerned with how contributions in theoretical physics—the highest point of pride of European science—were made by a very peripheral community in a new location. Their attempts to establish a new network of expertise and communication (in scientific matters) with their European peers—thus winning for themselves a share in the reward structure of science—was therefore permeated by the environment of a cross-cultural encounter and how that encounters must be negotiated successfully. In other words, it was driven by their goal of establishing a new epistemic culture in the sciences for India, and thereby defeat the so-called “civilizing mission”. It is in this charged cognitive environment that these peripheral engagements take place, which were constrained further by an imperial and a colonial setting.

4.5.1. An Indian Peripheral Trend: Basic Science

The research culture that took shape within that community had some distinctive features of its own that deserves brief mention. A look at the research output of these early practitioners show that they were intent—almost exclusively—upon basic science. Thus, science was seen by

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78 This point and its significance will be discussed in further in chapter 5.
79 While there had been some successful individual practitioners in science in India before this generation, such as J.C. Bose, it is during this time that we see the emergence of a self-replicating research community.
them as primarily a source of knowledge about the fundamental structure of the world, and only secondarily as a source of wealth or power for their society. Counter-examples of this dominant tendency can of course be found, for example, in the chemical researches of P.C. Ray, who sought to put science into immediate industrial and commercial use. Similarly, M.N. Saha, another contemporary of Bose and Raman, was drawn to the pragmatic potential of science for India, and was instrumental in putting together a nuclear program for India in 1948. But apart from these exceptions, until the 1940’s, the dominant culture of the Indian peripheral community was on theoretical knowledge, on grasping the fundamental structure of the world rather than converting those insights into immediate socio-economic use. Out of this dominant theoretical leaning came their heavy concentration in physics, especially physics of a most theoretical kind. This theory-first attitude later underwent a transformation after 1947, when in a massive shift of science policy the newly-formed post-colonial Indian state opted for science as its dominant partner in industrialization.

The reason why such a strangely theoretical science flourished within an imperial and colonial setting, where serious material and political deprivations could be seen as the order of the day, invites some speculation. The early stages of colonial science in India had begun with state-dominated field enterprises. According to Rajesh Kochar (2005), this phase of science was completely colonial in character, i.e., both its agenda and its methods were determined by the political and the commercial objectives of the empire. In contrast, the scientific research that flourished during the peripheral phase in the 1920’s sought to develop a university and laboratory-based culture in science, which also coincided with the peaking of nationalistic spirit. Scientific research during this period in fact became the primary tool for fashioning a new political identity. Thus, a conscious effort was made to overturn the previous colonial agendas of scientific research. In contrast to the field sciences, basic sciences offered more room for agency, control over one’s own research, and a place to contest the intellectual hegemony of the colonial state. Science, especially basic science, in the sense of understanding the structure of the world, provided a way to escape the fundamental feature of colonial experience— “the continuous reminder of one’s low political status in the hierarchy”. (Raychaudhury, 2002, p. 361). If participation in political decision-making was closed, books and ideas were still accessible, and furthermore, they offered a way into a different world where one could participate on an equal footing in the making of theoretical knowledge.

Before I move on to my case studies, some preliminary comments about the research styles of the scientists who will form the subject matter of those studies may be illuminating. A quick comparison of Bose, Saha and Raman shows great diversity of scientific styles among
Bose was perhaps the most comfortable of all the three in penetrating new areas and grappling with new problems, but he tended to leave old problems behind after a period of intense work, especially once something new came onto his horizon. Thus, not all of Bose’s work survives in the shape of his publications, and the span of his ideas can often be best understood by looking into the work of other people who were informed by his ideas. This is not puzzling, if we keep in mind that the task of his life was not just to attain the status of a scientist for himself, but to build a tradition of scientific research for his home country.

In the task of building a research tradition in physics—but no less so in chemistry—Bose left a lasting contribution for science in India. His great love of science as an aesthetic-theoretical form of inquiry, his insightful solution to a long-standing problem in metropolitan science, and his later complete indifference to his own declining scientific reputation in the West, provided a personal template of what it is to be a scientist in the Indian context.

Among these three figures who pushed the Indian research community from its amateur to its professional stage, Raman comes perhaps closest to the portrait of a normal scientist in a Kuhnian sense, one who always paid close attention to the institutional structures and to his own social visibility. He was also responsible for putting together a crucial organizational structure in science, such as a peer review system and a number of research institutes in India. Saha was involved deeply later in Big Science. He devoted considerable energies in the final phase of his life to craft a nuclear program for India, and popularized science by means of a journal named Science and Culture. Among the three of them, the period of transition from amateur efforts to a professional discipline was complete, and having gained a number of exemplars, the small peripheral scientific community in India was set to emerge as a larger and more organized community, where since 1948, science has become reliable enough to be pursued as a profession and as a means of living.

In the next part of this dissertation, I shall take up two highlights of this peripheral transition in the form of case studies. In exploring Bose and Raman’s pioneering work in quantum mechanics and in optics, my goal will be to show how new concepts were put together in the contexts of the Indian colonial periphery as a response to the introduction of scientific knowledge in that periphery and how those responses had to travel back to its corresponding

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80 The original plan of this dissertation was to do THREE case studies on those transitional figures. However, subsequently, I decided to develop only TWO studies on Bose and Raman, leaving the case of M.N Saha for a future effort.
metropolitan community before they could become part of the established paradigm of 20th century physics. Thus, the shape of the peripheral practice that we see in Bose and Raman conforms to the general two-part schema that I have laid out in this chapter.

What follows in part II is not a computational model of their science but rather a *philosophical reconstruction* informed by certain insights and points of view from cognitive science, which in future could be used as a *basis* for such as a full-fledged study. In the Part II, I see these two peripheral scientists as *agents* who were placed in an extraordinary situation, and how, by navigating their problems situations with their tools—both given to them by their situation—each of them produced an important outcome in science. Such philosophical reconstruction of science can be traced to Thomas Kuhn’s seminal work, where in a short passage in the *Structure* (1970, p. 62) Kuhn offers us a reconstruction of the discovery of the Leyden jar, stating briefly what sort of mental models and reasoning steps went into that discovery process.

The goal of this kind of reconstruction of scientific practice is of course to show how scientific practice is threaded to its locations, and how the knowledge that is produced by this interchange is crucially shaped by it.
Part II: Scientific Reasoning in a Peripheral Context

Chap 5. Birth of a New Statistics: Satyendra Nath Bose

“Needles and threads, even boxes of matches,
They all come to us by the Sea.
For lighting a lamp or to eat or to sleep
For every little thing we are not free.”

–Bengali patriotic song, 1914 (approx.)

Dear Colleague: 2.7.1924

I have translated your work and communicated it to Zeitschrift für Physik for publication. It signifies an important step forward and I liked it very much. In fact, I find your objections against my work not correct. For Wien’s displacement law does not assume the wave (undulation) theory and Bohr’s correspondence principle is not at all applicable. However, this does not matter. You are the first to derive the factor quantum theoretically, even though because of the polarization factor 2 not wholly rigorously. It is a beautiful step forward.

With friendly greeting,
Yours
A. Einstein

5.1. Introduction

“The ultimate aim of scientific inquiry is to arrive at a minimum number of hypotheses, which will explain the maximum number of facts.” Thus Satyendra Nath Bose, the co-founder of quantum statistics, began his Presidential Address at the Indian Science Congress in 1929, held at the Asiatic Society in Calcutta. For the philosophers of science, there will be no surprise in what Bose was saying: after all, he might have been simply repeating the standard official view about science. Closer to our own times, such views have been expressed by many other philosophers of science on different occasions. But there was something unusual about the fact that Bose, a scientist of non-Western origin, was making this claim and this statement about science, and that he had a lifelong concern and a deep preoccupation with scientific knowledge. In order to appreciate this, we have to consider a few facts about Bose: first of all, he was a first-generation scientist hailing from a colony whose own introduction to science had come via a complex path—

81 “The growth of science is driven in part by the desire for explanation, and to explain is to fit the phenomena into a unified picture insofar as we can” (Kitcher, Minnesota Studies in the Philosophy of Science, 1989, p.500).
by being part of a colony that was part of the British empire for a long time. Thus Bose received his first introduction to science, and to its sets of research programs, via a colonial education system, which contained in itself an important kind of epistemic asymmetry. By definition, colonialism signifies an epistemic dependence, a persistent inequality between the colonizer and the colonized, especially in matters of knowledge. From this vantage-point Bose had managed to become a scientist, finally leaving behind a lasting contribution in quantum mechanics. This contribution came in the shape of an important concept—the concept of the indistinguishablity of particles in quantum mechanics, which today forms the cornerstone of quantum physics. Having made this contribution however, Bose moved altogether away from quantum theory, never returning to it again for the rest of his life. Thus, Bose’s contribution to Western-style science was only partially successful and it was made from his curious vantage-point of an outsider.

In the Part I, I argued that scientific practice is not usually analyzed from the standpoint of the peripheral practitioners in science. Yet, we can put together such a framework for analysis (for outsiders like Bose) by bringing in the notion of an evolving trading zone that takes shape between two scientific communities. Bose’s example indeed gives us an excellent opportunity to begin this analysis. Notice that the interaction between Bose and his Western scientific community cannot be easily captured by falling back on our present social models of science. This is because under our current social models of science, science is viewed, by and large, as an egalitarian community where all knowledge-makers are vested with (nearly) equalized amounts of epistemic authority and who begin their journey from a consensus practice, i.e., a socially shared body of professional skill and theoretical competence, and a training procedure by means of which the veterans of a community transfer their skills to its newly-initiated members. But Bose’s example shows us that this standpoint—so naturally assumed while reflecting about science—do not tell us the whole story about science. As we shall see below, Bose did not belong to a uniform body of knowledge-makers and was never trained by the veterans of an established community. Bose was in fact a peripheral scientist, i.e., one who joins the community of metropolitan science from outside (in Bose’s case from a distant British colony) by means of two processes: first, his ability to identify important problems within the metropolitan research programs by a critical reading of their research literature, and secondly, by peer endorsement from that metropolitan community. The goal of this chapter will be to see if Bose was at all successful in building a trading zone with his European scientific community, and if once built, this zone permitted him to contribute further in science.
5.2. A Clean Derivation of Planck’s Law

In June 1924 Satyendra Nath Bose, an unknown Indian mathematician, sent Einstein a proof of Planck’s law from the recently-established colonial university of Dacca (presently in Bangladesh), where he derived the coefficient $\frac{8\pi v^3}{c^3}$ by using a particle picture of radiation, without assuming any concepts from classical electrodynamics. In this same paper, Bose laid also the foundations of a new statistics that today provides our modern understanding of a family of fundamental particles known as Bosons, which, not surprisingly, obey Bose-Einstein statistics.

Bose’s reputation in the West thus rests primarily upon his derivation of the thermal equilibrium law for blackbody radiation problem (henceforth BBR) that soon led to the discovery of quantum mechanics beyond the world of classical physics, precipitating a flood of theoretical activity among the European physicists from 1925 onwards. With Einstein’s extension of Bose’s counting procedure to monatomic gases, predicting Bose-Einstein condensation, Bose became forever a part of the Western normal science, and soon, of its textbooks on quantum theory. But Bose had lived and died in distant India, away from the centers of those (largely) European activities. How then did this problem become so significant for him that he spent four years of his life searching for a solution — how he chanced upon it, what resources were available to him, and what kind of reasoning did he use to arrive at a solution during those years when he was looking for a solution of the blackbody radiation problem?

Like the story of his predecessor, the Indian mathematician Srinivas Ramanujan, the story of Satyendra Nath Bose has been treated in the West with a mixture of disbelief, fascination and wonder. Bose had done something which, to put it bluntly, could not have been done: he created a fundamental concept in quantum mechanics apparently out of thin air even before the quantum mechanics was officially born. This was considered to be such an extraordinary event (on Bose’s part) that in 1980 in a short paper Max Delbruck argued that this could not have been done by Bose in any straightforward manner, e.g., by using ordinary scientific reasoning, but must have been entirely a product of serendipity. Here Delbruck offered a novel suggestion: Bose must have made an elementary mistake in statistics that, amazingly, turned out to be the right answer, and the significance of this ‘mistake’ was grasped first by Einstein. Thus, according to Delbruck, it was really Einstein who saw the significance of what Bose had achieved in his paper. Similarly, Abraham Pais, in spite of according extraordinary high praise to Bose (Pais, 1999), calls Bose’s

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83 “It is my contention that it arose from an elementary mistake in statistics that Bose made.” (Delbruck, 1980)
work a "successful shot in the dark." The perception that Bose had reached his conclusions guided by some kind of untrained, naïve intuition (that he was afterwards unable to repeat) is strong indeed. Could these views be really defended? Was Bose merely driven by some sort of intuition or made a statistical mistake, or did he in fact know what he was doing?

In the following sections, I shall try to provide some answers to these questions by reconstructing Bose’s introduction— and his subsequent solution— to the BBR problem, his development of a solution that welded the contributions of his European predecessors into a coherent mathematical framework, and the outcome of his scientific effort that finally led him into a controversy with Einstein. The outcome of that controversy was Bose’s premature departure from quantum physics. I shall argue that Bose’s brief success in international science and his 'failure' was not an example of serendipity or intuition that he had subsequently failed to repeat, but was part of a more complex story. His success was the result of a cognitive process called abstraction, which allows a scientist to put together new concepts on the basis of moving between different domains of representation (Nersessian, 2008, p.60). His controversy with Einstein and his subsequent failure to extend his ideas into a new research program shows Bose's fragile status as a peripheral scientist. Thus, Bose’s brief career in international science is an illustration of the Matthew Effect in science when a newcomer and an established authority receive differential amounts of recognition on the basis of the same work that they do jointly together. However, Bose’s example also shows us how situations like the Matthew Effect can sometimes lead to productive and progressive science. Indeed, situations like this can mark the beginning of a new research tradition within a peripheral scientific community. Thus, in the words of William Blanpied— who met Bose many years afterwards— what Bose did was to perform the task of being a 'tribal leader,' leading his community into a new and difficult intellectual terrain, to establish a first track record in science (Blanpied, May 1986).

5.3. Bose’s Problem Situation

Creating a new concept, such as Bose did, always takes place in the contexts of a problem situation. In other words, in explaining Bose's trajectory, we must be aware of two things about him: first, the situation that he was faced with, both socially and cognitively, and the problem-solving practices or the knowledge-constructing practices that he had available at hand. In reconstructing these two aspects of Bose’s situation, I will be guided by the current theories of knowledge-constructing practices that the human mind uses when it is faced with a problem

84 “I believe there has been no such successful shot in the dark since Planck introduced the quantum in 1900 (Pais, 2005, p. 428).”
85 See chapter 1, section 1.4.1. for an explanation of the Matthew Effect in science.
situation. This is the Cognitive-Historical approach in explaining science that tries to grasp the creativity of scientific practice by placing it within a cognitive format (Giere, 1988; Nersessian, 1992; 2008).

Bose was a scientist who was entirely raised and trained in a colony, and thus was a peripheral scientist in every sense of the term. Born in 1894 in the colonial city of Calcutta, the nerve-center of the overseas trade of the British Empire, Bose’s first exposure to the tradition of Western science came from his introduction to a colonial system of education. He was part of the famous 1909 cohort of the Presidency College in Calcutta (from which institution he obtained a Masters in Mixed Mathematics in 1915)\(^\text{86}\) for whom the project of recovery from colonialism by means of contributing in the sciences was important, and indeed, this project merged with their overall stance of nationalism. Thus, Bose’s goal was not merely to be a beneficiary of scientific knowledge, or a science teacher, but to become a contributor in scientific knowledge, i.e., a scientist, on behalf of his society.

Central to becoming such a scientist (from a peripheral location) is the act of becoming a member in two different (and often opposed) worlds. Being a scientist from a colony entails becoming an insider to what remains largely outside of one’s grasp— to be an independent maker of scientific knowledge. Even when this task is accomplished, as I have pointed out already, the nature of this membership remains difficult, and may even be lost due to a long controversy. The difficult character of this relationship has been studied in some detail by scholars who analyzed selected controversies of peripheral scientists with their metropolitan peers (see Sur, 1999; Dasgupta, 1999), but no general theory of peripheral science has been proposed yet by the philosophers of science. It is of course not part of my claim that all metropolitan scientists occupy identical social locations for certainly ‘outsiders’ can and do exist within metropolitan communities as well. But the peculiar mixture of the epistemic and the political factors that preface the introduction of a scientist of non-Western origin into science — particularly one who has been raised and trained in a colony — and which colors his subsequent professional life, is rarely experienced by someone working from a metropolis. For such scientists, doing science amounts to accepting a challenge. The complexity of their psychological profile is well captured in the following reminiscence of Abdus Salam: “I can still recall a Nobel Prize winner in physics some years ago from a European country say this to me: ‘Salam, do you really think we have an obligation to succour, aid and keep alive those nations, who have never created or added an iota

\(^{86}\) The syllabus, which would be called Applied Mathematics today, consisted of astronomy, dynamics, hydrodynamics and a few other mathematical topics. The corresponding physics syllabi of the Calcutta University at that time consisted of Mechanics, Acoustics, Optics, Heat and Electricity (courtesy: Rajinder Singh, by email communication).
to man’s stock of knowledge?” Salam reports that the lash of contempt that he experienced in those words became his main spring for further scientific discovery (Salam, 1982).

Why then attempt this difficult task of bridging two worlds and two cultures? Why would a colonial researcher want to immerse himself into a tradition that, historically, has contributed to his own society’s (political) powerlessness? The answer to this question seems to be at least two-fold: first, the enormous success of the sciences in the colonies as tools of power (which accords to science the essential mark of a nationhood) and secondly, the sudden death of Sanskrit knowledge, caused by the introduction of Western education (1857) into the Indian sub-continent. This produced an abrupt epistemic vacuum in the sub-continent (for more details see Kaviraj, 2005, pp. 119-142). While the colonized elites of the Indian society welcomed this step into ‘modernity’, the tangible consequence of this event was that all the previously organized system(s) of knowledge were thrown out, along with their languages of intellect (Sanskrit and Persian). Not only did the society thus suffer a massive epistemic loss within a short period of time, those systems of traditional knowledge were also pointed out as sources of ‘backwardness’.

Sociologically speaking, this is equivalent to a loss of knowledge societies, i.e., loss of “technical and professional expertise that organizes large areas of material and social environment” (Knorr-Cetina, 1999). The replacement system offered by the British colonial administration—a structure built on the model of the University of London—was limited both in its aim as well as in sheer physical size, being confined mostly to the port cities of India. The task of re-building a knowledge base in sciences assumes some importance in view of this epistemic crisis.

Against this background, in 1917 Bose joined the physics department of the newly-established University College of Sciences in Calcutta, along with his classmate and friend, M.N. Saha. They were handpicked by Sir Ashutosh Mukherjee, the dynamic vice-chancellor of the Calcutta University, who was intent on creating a full teaching university at Calcutta, out of its existing colonial framework. However, Bose soon moved away further east in 1921 to the newly-established Dacca University, now situated in Bangladesh. While in Calcutta Bose’s usual task

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87 Abdus Salam, “Renaissance of Sciences in Arab and Islamic Lands” in Abdus Salam: A Nobel Laureate from a Muslim Country. 1982.
88 Frequently, such scientists show a period of immersion in two cultures before they consider themselves equipped to negotiate this task. In his guest lecture before the US National Academy of Science in 1955, Bose’s younger contemporary, K.S. Krishnan, gave a detailed account of his absorption into two cultures. The many influences that Krishnan mentioned in his 1955 lecture included that of Lord Rayleigh and Whitehead as well as Sanskrit and Tamil literature, showing why such scientists can often easily span the divide of two cultures. Bose himself spoke French and German as well as English, he received his BA and MA degrees under a colonial education system, and was well conversant with the European culture.
89 This was of course the famous rationale that Thomas Babington Macaulay offered in his 1835 Minutes to the East India Company, justifying the introduction of a colonial educational policy in India.
was to teach electromagnetism and relativity, in Dacca University he taught radiation theory, and was thus confronted daily with the unsatisfactory task of explaining Planck's law to his students. The problem was that in all available existing proofs of Planck's law discontinuous quantum relations appeared side by side with the classical continuous quantities.

Bose’s problem situation was thus twofold: not only did he inherit a specific problem which demanded a solution, i.e., the problem of blackbody radiation from his European predecessors but his peripheral situation (due to the political situation of his country) demanded a contribution in scientific knowledge as well. In engaging with this task therefore, he would thus be engaging in a two-fold effort: first, with a metropolitan research problem, but also with a new body of knowledge, thus creating a track record in this knowledge tradition for the first time for his home community. This is creation of a new concept in the context of a problem situation, and such problem situations are located within a social, cultural and material context, which often also provide the tools for solving those problems. In Bose’s case, his problem and his tools came from his (limited) exposure to the research literature of metropolitan science, to which he had (some) access, thanks to his position of being a lecturer in Physics, first in the University College of Sciences at Calcutta, and then later, from 1921, at the Dacca University. He also had access to some materials through personal connections, such as Dr. Brühl, an Austrian who had settled in Calcutta, and who allowed Bose to use his personal library. But first let us consider what the BBR problem was that Bose was confronted with and how this problem created an anomaly for the European physicists within the heart of metropolitan science.

5.4. The Problem of Blackbody Radiation

Theoretically, blackbodies absorb (and later emit) all radiation that falls upon them. This seemingly simple phenomenon called blackbody radiation presented a major mystery to the European physicists around 1900, largely because the spectra predicted by the classical wave theory of light did not agree with the spectra that the experimentalists were actually observing.90 Blackbody radiation can be pictured as the glow coming from the inside of a red-hot oven or from a heap of burning charcoal, which, according to the classical theory, should lead to a rising “ultraviolet catastrophe” at the higher end of the frequencies. However, the actually observed radiance always presented a bell-shaped curve. To find a theoretical explanation of this mismatch, in 1900 Planck made a desperate attempt. Trying to find an explanation that would fit with the

90For a lucid description of the history of BBR problem, see galileo.phys.virginia.edu/classes/252/black_body_radiation.pdf, accessed on 4.27.2010. Experimental work on blackbodies was advanced enough during 1900, thus making the mismatch quite obvious.
experimental data, Planck hit upon a bizarre assumption to calculate the density of radiation in the oven cavity: let’s assume, he said, for the sake of argument, that the charcoal or the oven walls do not radiate energy continuously as demanded by the classical theory. Let's assume instead that energy is radiated by the wall in tiny discrete jumps. Thus was born $E = h\nu$; Planck’s quantum of action.

Much to his own astonishment, this bizarre assumption actually agreed with the observed results: Planck’s theoretical curve now fitted the spectra perfectly. The only problem was that no one, including Planck, could explain the significance of this move. Was this assumption only a mathematical fixes, or could this be taken as something with a deep physical significance?

Einstein was the first person to take this assumption seriously. He showed in his 1905 paper (See Stachel, 2002) that those observations were exactly what one should expect if the blackbody radiation were not a collection of waves, but a gas of discrete particles, where each particle is endowed with a directed momentum. Light radiation, Einstein insisted, has an atomic structure just like matter, thus revealing a pleasing unity in nature. Prior to the 1900's, a theory of how particles behave in large numbers was explained by the theories of Maxwell and Boltzmann that explained successfully the origin of the ideal gas law and the velocity distribution of gas molecules. What Bose’s contribution did was to reveal that this explanatory structure stands upon a fundamental assumption: that each particle in the gas in some way maintains its distinguishable character from all the others. In a heated gas, we now know that this is indeed true for at such temperatures the number of available quantum states far exceeds the total number of atoms in the system, making each molecule behave as if it has a unique individuality. But this does not hold true in case of a phenomenon like BBR because of its high uncertainty relations, and which can be treated—as in fact Bose did treat it in his paper—as a gas of light quanta. Planck’s law can be easily obtained, once one is prepared to drop this assumption of individuality.

5.4.1. Metropolitan Attempts to Derive Planck’s Law: Planck, Debye and Einstein

Planck had found that the energy density of electromagnetic radiation (i.e., the amount of radiation present inside the blackbody cavity) in equilibrium at a given temperature $T$ is a function of its given temperature $T$ and its frequency $\nu$. This leads us to Planck’s law in the following form:
\[
\rho(v, T) = \left( \frac{8\pi v^3}{c^3} \right) \cdot \left( \frac{h v}{\exp^{\frac{h v}{kT}} - 1} \right)
\]

\( \rho \) is the energy density per unit frequency interval at a specific frequency \( v \), \( T \) is temperature, \( c \) is the speed of light, and \( h \) is Planck’s constant.

Planck’s derivation of this law came from his considering the activity of the material oscillators that he imagined were embedded within the oven walls of the cavity, alternatively absorbing and radiating energy (Venkataraman, 1992). In Planck’s view, those oscillators transferred their energy to the standing electromagnetic waves of the same frequency. His ‘act of desperation’ lay in assuming that this transfer only takes place in the form of discrete packages or quanta, in other words, though energy was being absorbed continuously, it can only be emitted discretely. In calculating the energy density of the oscillators plus the radiation field Planck therefore proceeded as follows. He found the average energy at temperature \( T \) of an oscillator of frequency \( v \). This may be called \( E_v(T) \). Next he calculated the number of modes of electromagnetic waves that the oven cavity could sustain, dividing this quantity by the cavity volume \( V \) to obtain \( \rho(v) \, dv \) or the normal mode density. Finally, he multiplied this quantity by \( (E_v(T)) \) so as to obtain the desired result. (Venkataraman, 1992, p.37)

Notice that when we have a large number of oscillators, the heat energy is distributed among those various oscillators in an optimum manner. This yardstick (\( kT \)) of optimal distribution varies of course according to the energy levels available to the system. Thus, at lower temperatures, i.e., at lower energy levels, the quantum effects of the system show up prominently. It is this insight that Einstein generalized upon in the form of Bose-Einstein condensation, once he read Bose’s paper.

To get back to Planck, since an oscillator can take only discrete energy values, the average energy of an oscillator divides the cavity space into a number of discrete cells in the phase space, each cell separated from the next by an interval of \( h \). In employing this phase space argument, Planck was thus led to interpret \( h \), Planck’s constant, as a finite extension of the elementary area in phase space. At this point he commented that “one should confine oneself to the principle that the elementary region of probability \( h \) has an ascertainable finite value and avoid all further speculation about the physical significance of this remarkable constant (Ghosh, 1992, p. 40).” In other words, Planck was persuaded that classical electrodynamics is essentially...
incompatible with his radiation law, but avoided further speculation on this issue. This is a conclusion that Bose took most seriously.

Peter Debye, on the other hand, proceeded to derive the same law in 1910 without using Planck’s relations between the radiation density $\rho_v$ and the average energy of an oscillator $U_v$. Instead, Debye calculated the energy in the radiant cavity by following an approach also used by Rayleigh before him—by simply counting the standing waves of radiation inside the cavity, or by considering the properties of its state alone without bringing in any resonators. In accordance with the classical theory—and perhaps trying to avoid any Planck-like ‘inconsistencies’—Debye calculated the number $N_v dv$ vibrational modes standing inside the cavity and obtained $\frac{8\pi v^2 V dv}{c^3}$, as the average energy inside the cavity. Assuming that an amount of energy $hv$ gets distributed over each of these standing vibrational modes, he calculated the probability of distributing $N_v f dv$ (f, being an arbitrary distribution function) quanta of energy among those modes by means of the following relation:

$$\omega_v = \frac{(N_v dv + N_v f dv)!}{(N_v dv)!(N_v f dv)!}$$

Bose notes in his second paper (1924b) that Debye had derived Planck’s law by using statistical mechanics, even though he still made use of the classical theory in appealing to those normal modes of vibrations.

Einstein’s own proof in 1916 followed a remarkably different pattern because he derived the same law without using any classical theory about resonators, but instead used Bohr’s notion of an atom with its discrete energy levels. He used Boltzmann’s principle to calculate the probability $W_v$ for an atom to be in a stationary state. He next assumed that a stationary state $m$ can pass on to another stationary state of higher energy $n$ by absorbing a light quantum. However, the reverse process by which an atom shifts down to lower energy state (by emitting light quanta) can happen in two ways. Either the atoms emit radiation independently of the external field, as in radioactivity, or they may be ‘persuaded’ to emit an energy quantum by the radiation field. This induced transition, dependent on the radiation field, was called ‘negative radiation’. In a thermal equilibrium of course both these upward and downward processes must be held in balance. Finally, Einstein took guidance from Bohr’s correspondence principle which states that classical

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91 As we shall shortly see below, this proposed mechanism became his (future) bone of contention with Bose.
theory should be treated as the limit of the quantum theory, used Wien’s displacement law, and thereby derived Planck’s law.

Implicitly Einstein’s proof uses Maxwell-Boltzmann statistics, combining it with other elements such as the correspondence principle, Bohr’s atomic theory, as well as his own hypothesis of light quanta.

This is the structure that Bose inherited from his predecessors. Note that this set of proofs contains all the elements that Bose himself used shortly thereafter: phase-space arguments, consideration of only the properties of radiation trapped inside the blackbody cavity, and finally, the photon hypothesis proposed by Einstein. The main problem that his predecessors left him with—in spite of their elegant constructions—was that their different strategies failed to add up to any mathematically coherent structure. In fact, they all used elements from the classical theory as well as from discontinuous relations, a fact that Bose himself notes in the opening lines of his 1924 paper. As we shall see, what Bose did was to combine all the three strategies but he combined them to achieve a remarkably different effect in his proof. In other words, he took a motley collection of ideas and put them on a secure mathematical foundation, thereby producing a coherent, logical framework upon which further contributions could be built (Home and Gribbin. 1994).  

5.4.2. A Rigorous Proof of Planck’s Law: “The Man who Chopped up Light”

The essence of Bose’s light quantum paper in 1924 came as a proposed amendment to the prevalent Maxwell-Boltzmann statistics (implicitly or explicitly used by all his predecessors), demanded by his use of the mental model of blackbody radiation as a gas of light quanta. Located far away from the metropolitan community that he was tracking with his critical reading, Bose’s method of joining their game was to try to make sense of everything to himself, never being satisfied until he himself saw the coherence and the consistency of those ideas. Years later, in his conversation with Mehra, Bose offered this as his main scientific motto (Mehra, 1982, p.

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92 In conversation with Mehra, this is how Bose recalled his introduction to the BBR problem: “I had studied Planck’s derivation of his radiation formula and also his Theorie der Wärmestrahlung. I knew about phase space and Boltzmann statistics from Gibb’s book. Also I had Boltzmann’s Vorlesungen über Gastheorie from Brühl. I knew about Einstein’s derivation of Planck’s formula and how he had never been satisfied with the derivation of this quantum law; he always kept on coming back to it. Planck's condition ran counter to the classical ideas. Planck was also aware of the difficulties, but he had never been able to resolve them. He had relations which were derived from Maxwell’s electromagnetic theory and others in which discontinuities appeared. He wanted to reconcile his theory with classical theory (Mehra, 1982, vol.1, part II, p. 564-565).”

93 This is the title of the 1994 paper on Bose by Dipankar Home and John Gribbin in New Scientist.
Since teaching aided him in this task, by his own accounts teaching became for him the great stimulus for his scientific work.\footnote{“Never accept an idea as long as you are yourself not satisfied with its consistency and the logical structure upon which the concepts are based. Study the masters... lesser authorities cleverly bypass the difficult points”.}{\footnote{See Mehra, 1982. “As a teacher who had to make these things clear to his students, I was aware of the conflicts involved and had thought about them.”}}

His paper on Planck’s law embodied this critical spirit. Bose began his paper with the statement that all previous derivations lack justification from a logical point of view because they all use classical continuous relationships as well as Planck’s quantum of action, which stated that energy is emitted in the Hohlraum cavity in discrete jumps. Thus, considering all previous derivations logically unsound, and hence unsatisfactory, Bose claimed that Planck’s law could be derived with just two assumptions—the light quantum hypothesis combined with statistical mechanics. (Bose, 1924a) Thus, Bose’s conscious goal was to find an elegant derivation that satisfies two objectives: removing the standing logical inconsistency inherent in all the arguments of his metropolitan peers, and contributing successfully to a research program already initiated by Einstein. This same goal, as we shall shortly see, also animated his second paper which he wrote shortly afterwards (Bose, 1924b).

In his very short four-page paper Bose outlined his reasoning in four basic steps.\footnote{In my reconstruction of Bose’s proof in four steps, I rely heavily upon Partha Ghosh’s reconstruction of Bose’s 1924 paper into four components, each of which, according to Ghosh, was constituent in Bose’s reasoning.}

Taking seriously Einstein’s view that BBR is a collection of light quanta enclosed in volume $V$, Bose made use of a phase space argument (also used by Planck before him) to model the location and momentum of the light quanta:

Step 1: Each light quantum has a location in space and a directed momentum. Therefore, its phase space coordinates are $(x, y, z)$, and the momenta along those coordinates are $(p_x, p_y, p_z)$. This simple reasoning neatly quantizes the radiation field into elementary cells of volume $\hbar^3$ each (momentum of a light quanta is $\hbar v/c$ in the direction of its motion; 3 possible coordinates, hence $\hbar^3$), and $4\pi v^2/c^3$, which had until then been modeled only as a collection of normal modes (of waves) between frequency $v$ and $dv$, now receives an alternative new interpretation. Throwing in a factor of 2 to account for what he called the ‘two states of polarization’, Bose easily derived $8\pi v^2/c^3$ — the co-efficient in Planck’s law, without even mentioning the classical wave picture. This move completes what Stachel calls Bose’s derivation of the first factor (see Stachel, 2002).
Step 2: The next task is clearly the counting of $A_s$, or the number of cells available for occupation by the light quanta. Since each cell can only contain one quantum state, the problem of the counting the number of possible quantum states now reduces to the problem of counting the number of cells (in various states of occupation).

Step 3: In determining the thermodynamic probability of each micro-state $W$ that taken together constitutes a macro-state, Bose now proposed a new amendment of the old statistics, demanded by his modified phase space argument that he had taken over from Planck. Since the permutations of light-quanta within a cell no longer produce a new macro-state, particles can all be treated indistinguishably. Bose thus made the necessary adjustments in his statistical step, counting only the occupation number of those cells, but did not comment that the statistics were being so modified.

Step 4: Having thus completed his most crucial step, Bose next followed the standard statistical mechanical manipulations, maximizing $\ln W$, holding it subject to the constraints on $E$ and $N$, thus rigorously deriving Planck’s law.

Since Bose’s paper is astonishingly brief, containing neither explanations nor comments about why he thought it necessary to take that unusual third step, the question always remained: how did Bose do it? Was this a case of remarkable intuition, plain serendipity, or just a “shot in the dark”? In the literature on Bose, there exist two opposing views on this issue, both emanating from the metropolitan community in the West. In addition to this, there exists also a third view from Bose’s own home community in India, especially in the form of his last doctoral student Partha Ghosh (Ghosh, 1994). Ghosh rejects the intuitive hypothesis proposed by Pais, arguing that Bose was indeed building productively upon the work of his predecessors, though naturally, he could not have been expected to know the future consequences of his work. Thus, Bose was neither indulging in intuitions nor shooting in the dark, but was simply exercising normal scientific rationality just like any other mainstream scientist. This defensive reply is not unusual from a scientist who himself belongs to that peripheral community, and is reluctant to lose his hard-earned foothold in the mainstream “rational” science. But can we, with the theories of knowing subjects and their cognitive operations which are available with us today, construct Bose’s reasoning any better? What were the stages of knowledge-construction that Bose used in his conceptual innovation?
Bose’s derivation consists of two main parts (this is what Stachel calls as the derivation of the first and the second factor): first, his consistent, logical expansion of the photon hypothesis of Einstein—thereby introducing a new interpretation of the coefficient $8 \pi v^2/c^3$ in terms of the photon picture, in essence creating a new scientific representation. Secondly, his subtle change in the statistical counting procedure in view of this particle picture of things. This looks analogous to the older Boltzmann statistics, but in fact, resulted into an amendment of that statistics. In other words, Bose had taken over Planck’s phase space arguments but had employed those arguments to reach a different goal, thereby in effect solving a new problem. While Bose did not leave any record of the path that he had taken (in the form of notebooks or diaries), a reconstruction of his thinking could still be attempted in view of the conversations that he had later in life with Mehra (as well as with Blanpied and Ramsheshan). There also remain the four crucial letters that he wrote to Einstein between 1924-1925.97

**Developing A Particle Picture of Radiation**

Recall that Planck had found that the energy distribution of electromagnetic radiation in equilibrium at a given temperature $T$ is a function of its given temperature $T$ and its frequency $v$. This can be expressed as a product of two factors:

$$p(v, T) = \left( \frac{8 \pi v^3}{c^3} \right) \left( \frac{h v}{h v \exp \frac{hv}{kT} - 1} \right)$$

Bose’s reasoning thus began with the position that if there are $N_s$ light quanta of energy $h v_s$, then total energy $E$ in the cavity can be represented by the following relation:

$$E = \sum_s N_s h v_s = v \int p_v dv$$

So, $\rho_v$, the energy density, can be determined, if $N_v$ is known.

97 See Appendix I.

98 $p$ is the energy density per unit frequency interval at a specific frequency $v$, $T$ is temperature, $c$ is the speed of light, and $h$ is Planck’s constant.
The first statistical problem therefore is to compute $N_s$. Here Bose says, “if we can give the probability of each distribution for any random values of $N$, then the solution is determined by the condition that this probability is a maximum, subject to the subsidiary condition” (Bose, 1924a).

Bose’s important step in the construction of his proof thus lay in seriously modeling BBR as a collection of light quanta enclosed in volume $V$. In Gibb’s texts Bose had encountered phase space arguments, also used by Planck before him. His knowledge of Debye and Einstein’s proofs came from his association with other peers such as Saha and D.M Bose. But unlike his two European predecessors, Bose applied his phase space arguments consistently, contemplating throughout a cavity filled only with particles. Debye for example, had occupied himself with a cavity that was filled with the standing waves of radiation, and Einstein, in his March 1916 paper, talked about the processes of emission and transmission by the atoms in the blackbody cavity (Mehra, p.515). Recall that even according to Planck, the classical wave theory remained the established picture of things, and the quantum effects in BBR only showed up in the “peculiar interaction between radiation and the elementary resonators (or material particles) in the cavity walls” (Mehra, p.512). It was thus Bose’s unique contribution to the problem to draw the discussion away from those processes of interaction, and associate the light-quanta with a new interpretation as well as with a new statistics.

The state of a single light quantum can be characterized, as per this new model, by a point in 6-D phase space lying on the surface of a cylinder.\(^9\)

\[\text{Fig. 5.1. Phase-space diagram of a Simple Harmonic Oscillator, showing how the phase space of an individual particle quantizes the field. Readapted from C.A. Gearhart, “Planck, the Quantum and the Historians,” Physics in Perspective, 2002. Vol. 4. p.197.}\]

To compute the number of states of a light quantum whose frequency lies between $v_s$ and $(v_s + dv_s)$, the phase space has to be divided into tiny cells of volume $\hbar^3$ each, thus extending

\(^9\) Many thanks to my friend Somaditya Bannerjee, Department of History of Science at the University of Minnesota, for explaining Bose’s derivation of Planck’s law to me.
Planck’s quantizing of the action of the oscillators to the entire radiation field. Thus, in accordance with his professed motto of consistency and coherence, Bose abandoned the oscillator picture altogether, shifting wholly over to a new particle model. Multiplying by $V$ to get the total volume of the spherical shell of the phase space, dividing it again by $\hbar^3$, and multiplying again by 2, Bose quickly obtained:

$$A_s = \frac{8\pi v_s^2 V d\nu_s}{c^3}$$

The co-efficient $8\pi v^2/s^3$ thus receives a new interpretation, completely consistent with the particle picture of radiation. On this alternative picture, what is being counted is not a collection of waves in the normal mode, but rather the number of cells in the phase space of a gas of light quanta. Bose’s consistent use of a phase space argument in conjunction with the hypothesis of light quanta had modified the older problem into a new one: measuring the density of radiation is no longer a matter of counting waves in a cavity.

Why did Bose feel compelled to multiply the expression by a factor of 2? His published paper simply says: “to take care of the fact of polarization”. Again, no explanation is given why polarization, essentially a wave concept, is used in a proof that claims to be based solely upon the particle picture. Bose maintained for the rest of his life that he did provide a particle explanation at this point, which is that the light quanta carry an angular momentum that could take only the values of $\pm h/2\pi$. This, he claimed further, was edited out of his paper during the process of translation by Einstein. Over a period of nearly forty years, he seems to have retained this as a consistent memory, but since he did not make any priority claims over this concept, nor did he retain a copy of his English original, his claim to priority on this issue will forever rest on oral history alone. The one independent mention of this affair appears in a paper written by C.V. Raman in 1931, where Raman mentions that the notion of photon spin had originated first with Bose.

**Phase Space Cells**

Since the total number of cells is all the possible arrangements (states) of a quantum in a given volume, the next step is to compute the number $A_s$: the number of cells available for

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100 See Blanpied, 1972; Mehra, 1982 and Ramasheshan, 2000.
101 “The few sentences which Einstein crossed out about the angular momentum of the light quanta is evidence of this,” (S. Ramaseshan, “A Conversation with Satyendranath Bose Five Decades ago”, *Current Science*, March 2000, Vol. 78. 5).”
occupancy by the photons of frequency \( v \). Classically, there would have been infinitely many such arrangements, but on the particle picture that Bose was working with, these possible quantum states, following Planck, would have to be separated by the finite amount of energy \( h v \).

An elementary cell of volume \( h^3 \) would thus contain at most one such quantum state, and the whole problem of counting the number of quantum states now reduces to the much simpler problem of counting those cells.

**Cell Occupation Numbers and Indistinguishability**

If all cells were distinct, the number of such cells would clearly be \( A_s! \) But since each cell can contain only one quantum state, and since the permutation of light quanta within a cell does not produce any new state, all cells can be partitioned into classes solely by their occupation numbers, i.e., by noting how many quanta are sitting within each cell. This proposal indicates a radical break with the traditional method of calculating a macrostate by looking up which particle goes into which cell. So, in place of the older Boltzmann statistics used by Planck:

\[
W = \frac{N!}{N_0!N_1!N_2!...} \tag{1}
\]

We now get:

\[
W_s = \frac{A_s^!}{p_0^!p_1^!p_2^!...} \tag{2}
\]

(p\(_0\) denotes states that are empty, p\(_1\) denotes states with one quantum and so on…)

The proposed change simply assumes that light quanta within cells are indistinguishable in the following way:

\[^{102} \text{Planck represents this as a case of } N \text{ gas molecules being distributed among the elementary phase-space regions of 0, 1, 2 and so on.}\]
Fig. 5.2. Re-adapted from Max Delbruck, 1980, showing the result of Bose’s amended statistics. Notice that in Fig. 2 the ‘individuality’ of the particles has disappeared, all individuals now appear indistinguishable.

Calculating the Equilibrium

Finally, since the radiation within the cavity has to be brought to an equilibrium, $W$ must be maximized. This Bose did via the standard manipulations of statistical mechanics, also used by Debye before him, holding the total energy $E$ and the total number of quanta $N$ constant. Planck’s law was thereby easily obtained as a logical conclusion.

While Planck had counted the number of oscillators and Debye the number of standing waves in the cavity, arguing that the energy was being exchanged between the oscillators and the standing waves, Bose simply thought of BBR as a gas of free particles from the beginning, in accordance with the light-quantum hypothesis of Einstein, and simply asked how such a radiation field would achieve its equilibrium. In an important sense, it is Bose’s modification of the problem that had provided the right answer. Under Bose’s coarse-grained counting, which dropped the individuality assumption from the particles, the quanta became endowed with the
new ‘herd instinct’ at very low temperatures when their numbers of energy states are reduced to a minimum. This consequence of Bose’s work was immediately grasped by Einstein, who quickly applied this to his theory of monatomic gases, thereby predicting the Bose-Einstein condensation. If in the first half of his proof, Bose’s substantial contribution lay in introducing a phase-space argument that allowed him to re-arrange the problem—consistent with the particle model of radiation—his most decisive contribution in the second half lay in dropping the individuality assumption that was weighing down all his European predecessors.

5.4.3. Cognitive Processes or a Shot in the Dark?

Starting from the existing representations of his European predecessors, all of whom used the classical view of light as a wave, how did Bose arrive at a non-classical representation—as well as a statistics—of blackbody radiation?

As I argued in the Part I, new conceptual structures emerge in those situations in which an individual scientist develops a response to a specific problem situation, using his or her knowledge-constructing resources, such as reasoning, analogy or visual modeling, which are sophisticated forms of everyday thinking (Nersessian, 2008, pp. 8-10). Thus, our language to describe such new creations must be couched in terms of these generative processes that take place within the minds of such agents, essentially allowing them to construct a new solution. Furthermore, according to the analysis that I laid out in chapter 3, these processes should not be considered as mere ‘aids to reasoning’; indeed, they are the driving mechanisms behind new conceptual changes. Accounts in terms of such processes can be of especial value in explaining peripheral science, for such accounts help us to see how a peripheral scientist engages with his or her two-fold task in the context of a problem situation; how s/he handles the business of manipulating the newly-imported representations by means of his/her knowledge-constructing practices, thereby creating a new track record in science for the first time for his or her home community.

In Nersessian’s example of a Cognitive-Historical study, her account of Maxwell begins with a striking sequence of mental models, and of their gradual manipulation in the course of reasoning. First, Maxwell modeled the electromagnetic field in the shape of a fluid media filled with vortices, introduced idle-wheels between them, thus separating those vortices from one another. Finally, those vortices were taken to be made of an elastic medium, which allowed them to store tension in the form of electric charge. Bose did not reason with diagrams or with experimental artifacts but his use of mathematical and statistical reasoning still allows us to see
the reasoning procedure that he was relying upon to get his derivation. The central part of his
inference came from his modeling of BBR as a gas of photons in the cavity. This allowed him to
see, first of all, the energy density as a case of free radiation, and not as an activity tied to any
oscillators or waves, something that was the usual practice of his European predecessors. This
modeling of BBR as a gas of photons (or in modern terms, as an ensemble) furthermore allowed
him to incorporate the phase space arguments from Planck, albeit with a different goal in mind. In
Bose’s hand the phase space argument no longer simply described the activity of the oscillators,
but applied to the cavity radiation itself. In moving between these two domains of resources,
Planck’s oscillators and Einstein’s energy quanta or photons, Bose was able to shed the oscillator
picture altogether from his reasoning, and thus was able to convert Planck’s heuristic point of
view (Planck’s ‘formal device’) into a new ontological picture (energy quanta) endowed with
novel statistical properties. Thus, Bose used Planck’s argument in conjunction with Einstein’s
model so as to generate a new picture of reality, thereby producing a significant conceptual
change. His amended statistics was set up on the analogy of the older Boltzmann statistics, but
the amended statistics was employed to solve the new problem that was generated by his
modified phase space arguments. Since the switch between these two domains—his source and
his target—could only be effected by adjusting the statistics, Bose adjusted his statistics, in that
process dropping the one assumption (of individuality of particles) that had stopped everybody
from finding the correct solution. Bose was not sure how far he had diverged from the established
statistical procedure of his predecessors; indeed, in his second paper he still seems to think that
they were analogous procedures. Thus, like many others before him, the gap between the old and
new was closed on the strength of an analogy, a productive source of scientific thinking.

Thus, far from shooting in the dark, or having a sudden flash of intuition, I submit what
Bose did in his proof was to perform an abstractive process, which is reasoning in the extended,
cognitive sense of the term.103 Moving between the arguments of Planck, Debye and Einstein,
Bose was able to drop the selective constraints of the oscillator model and attain a level of
generality that allowed the emergence of a new ontological picture. In the extended cognitive
sense of the term, this is scientific reasoning at one of its most creative moments, indicating the
presence of a lengthy construction process behind Bose’s thinking (which historically spanned
nearly the length of a year).

Was Bose conscious about the importance of his result? Contrary to the popular view that
assumes that Bose did not at all understand the significance of his work, I claim that Bose was

103 I have argued in Part I that ‘reasoning’ must be understood to be broader than mere deduction or induction, and
processes such as thought experiments, analogy or abstraction must all be included within the wider family of
‘reasoning’.
conscious of the originality of his derivation of the first factor. His first letter to Einstein clearly shows that he thought his own work to be important.\textsuperscript{104} By his own light therefore, he had provided a solution to the BBR problem that Einstein was looking for for a long time, thus completing his (Einstein’s) research program. Bose thus felt justified in asking his work to be published in Zeitschrift für Physik. Towards the end of his life, in an interview with Raman’s student Ramasheshan, Bose very modestly rejected the widely prevalent notion that he was unable to understand his own work, recalling in conversation his four years of intense struggle that he then likened to a real physical pain.\textsuperscript{105} Bose might have been unaware that a revolution in physics would soon follow his short paper, but this would have been equally true of Planck himself, and yet, to the best of my knowledge, no one has ever claimed\textit{ this} as evidence that Planck did not understand his own arguments. Thus, there seems to be no shred of evidence for Delbruck’s remarkable suggestion that Bose had only committed a statistical blunder, which Einstein understood to be a truly significant step forward. Bose certainly knew that he had solved the most troublesome problem in radiation theory,\textit{ as that theory stood at that particular point of time}.

More than anything else perhaps, Bose’s success came from his willingness to take a revolutionary step. Unlike his European predecessors, Bose was not seeking to reconcile a theory to an established body of knowledge, neither was he seeking to repair an established research program. On the other hand, he was certainly seeking to make a radical impact on a distant research program in order to join it in the role of a maverick.\textsuperscript{106} His attitude of critical revision towards the metropolitan program made it clear to him to see what was missing, i.e., precisely where the statistics must be amended. Unlike the European physicists, whose goal was to repair—and possibly uphold— the established classical picture, Bose was an outsider who could have profited by a radical change, and thus was the only man to see where to step outside of the box.

5.5. A Controversy Over Radiation and Its Processes: Bose’s Second Paper

Bose had, on the face of it, provided a derivation of Planck’s law that was clean, i.e., free from all classical assumptions of light as a wave, based entirely on the concept of light as energy quanta. He had succeeded in the task that he had set for himself— providing a derivation of

\textsuperscript{104} See Appendix 1 for Bose’s letter (dated June 4\textsuperscript{th}) to Einstein.
\textsuperscript{105} “But because of my attitude people think I did not understand what I was saying. The few sentences which Einstein crossed out about the angular momentum of light quanta is evidence of this.” (This conversation with Ramaseshan took place in 1950)
\textsuperscript{106} Bose’s first paper on radiation theory clearly shows that he had chosen to play the role of a maverick by offering a striking view about radiation—i.e., radiation should be viewed as a free gas of indistinguishable photon particles that obey a new statistics.
Planck’s law, which Bose’s metropolitan community, the physicists in Europe, were searching for over nearly quarter of a century. Seen from Einstein’s point of view, Bose had provided the last piece in his puzzle, putting Einstein’s light quantum hypothesis beyond all reasonable doubt. But Bose had modified Boltzmann’s statistics in that process, setting aside a major presupposition that helped to usher in a new era in physics. Henceforth, a fundamental family of elementary particles of relativistic nature would be handled in a different manner known as Bose statistics, and soon the notion of symmetric wave function introduced by Dirac would tell us why Bose statistics applies to such particles. Thus, Bose laid the foundations of the quantum theory of the electromagnetic field in his two papers well before the rest of the theory was officially born. It is this feature of Bose’s proof that intrigued his metropolitan peers and earned him high praise in Pais’ book (See Pais, 1982, pp. 427-428). Yet, having achieved this success, Bose left the scene of international science, took no part in further controversies on quantum theory, and remained obscure for the rest of his life. What could explain this extraordinary turn of events in Bose’s life? Did Bose, as some clearly suspected, become a dropout from science?

To provide an answer to this question, it is now necessary to turn to Bose’s next two papers on radiation theory, the second of which was sent to Einstein immediately after his first paper (June 15th, 1924), as well as the third paper that existed only in the form of a manuscript (once again sent to Einstein) but never saw the light of publication. In October 1924, Bose arrived in Paris, having secured his two years of study leave from the Dacca University on the strength of Einstein’s endorsement of his first paper. His visit was inspired by the idea of meeting Einstein face to face, taking his own views on radiation theory to the next level (the foundation of which had been laid already by his first paper), and if possible, become Einstein’s collaborator in this task. Bose failed in his second and third objective, although he got to meet Einstein face to face.¹⁰⁷

¹⁰⁷ The communication between Einstein and Bose can be reconstructed by looking at the following timeline:

4th June, 1924: Bose sends his First Paper to Einstein from Dacca
15th June, 1924: Bose sends his Second Paper to Einstein from Dacca
2nd July, 1924: Einstein responds to Bose in Dacca
26th Oct, 1924: Bose informs Einstein of his arrival in Paris
3rd Nov, 1924: Einstein writes to Bose in Paris, criticizing his second paper
27th Jan, 1925: Bose informs Einstein about his third paper sent under separate cover
8th Oct, 1925: Bose informs Einstein about his arrival in Berlin.
Bose’s second paper on the interaction of matter and radiation (his first paper had dealt with radiation only) was much longer than his first one, and considerably more ambitious. In two parts, in this paper, Bose sought to derive the general conditions for the statistical equilibrium of a system consisting of both matter and radiation, but independent of any special assumptions about the causal nature of those radiative processes. Hoping again to build upon Einstein’s work and remove all arbitrary assumptions from that work—just as he had done in his first paper—Bose now rejected Einstein’s special assumptions about there being two kinds of radiative processes, spontaneous and induced, by means of which an atom of higher energy shifts down to a lower energy level. Recall that this was the mechanism with which Einstein had constructed his version of the proof of Planck’s law. According to Einstein, while a photon moves up a higher energy level by the process of absorption, it moves down to an energy level by means of two processes: spontaneous and induced. To model these interactions of atoms and radiation, Bose allowed for photons to have many frequencies and atoms many energy levels—Einstein’s 1916 proof of Planck’s law had been somewhat artificially constructed with two energy levels only. Bose also took note of the fact that the electrons will scatter photons by means of the recently-discovered (1923) Compton Effect (Venkataraman, 1992).

Bose now claimed that the transition from the higher energy state to the lower energy state can be explained more elegantly, without bringing in Einstein’s (additional) hypothesis of an induced transition. Furthermore, he claimed that spontaneous transition (as in case of radioactivity) is enough to explain the transition from a higher energy state to a lower energy state, which can in turn be understood as a property arising from the statistical character of the radiation field itself, consistent with all the equilibrium conditions. The emission of light thus could be viewed as a unified single process—arising purely from the statistical property of the radiation field itself—and not something which is dependent upon the specific causal mechanisms of energy transfer. Note that this conclusion is analogous to Bose’s own earlier conclusion in the first paper, where he had derived Planck’s law by essentially arguing in the same manner. Thus, what Bose proposed in his second paper was essentially a generalization—as well as an amendment—of Einstein’s views, just as Einstein’s own paper on Bose-Einstein condensation had been a generalization of Bose’s view.

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108 Both of Bose’s papers were translated from English to German by Einstein himself. The title of the second paper (in English translation) is: “Thermal Equilibrium in the Radiation Field in the Presence of Matter”, submitted to Festschrift für Physic on 7th July, 1924. For an English translation of the paper, see “S.N. Bose: the Man and His Work.”

109 For a detailed discussion of this issue, see Partha Ghosh in S.N. Bose: the Man and his work, vol. I., pp. 58-60.

110 An induced transition is much like the case when an atom is ‘persuaded’ to jump down one level by some frequency of the radiation field. (Venkataraman, 1992, p. 95)
Einstein had strong objections to these proposals.\footnote{See Appendix 1, Einstein’s letter to Bose, dated November 3rd. The controversy between Bose and Einstein could perhaps be attributed to their very different styles in scientific thinking. While Einstein relied manifestly upon illuminating physical analogies and thought experiments, Bose’s signature style in physics was using statistical thinking in terms of field properties. For more on styles of scientific thinking, see Hacking, 1994.} He considered Bose’s hypothesis as not applicable to the elementary radiative processes and appended two critical footnotes to Bose’s second paper.\footnote{Bose always remained of the opinion that his second paper was an \textit{advancement} upon his first. However, this paper has been virtually ignored in the subsequent literature in quantum physics due to the first critical remarks of Einstein.} In his note to Bose on 3\textsuperscript{rd} Nov, 1924, Einstein objected that the absorption coefficient is independent of the radiation density, as confirmed by experimental evidence from infra-red radiation. To respond to this objection, Bose wrote a third paper and sent it to Einstein from Paris. The reply failed to impress Einstein. The paper was never published, suggesting that Bose had abandoned the draft, possibly after hearing Einstein’s further critical responses on spontaneous and induced radiation, which, Einstein maintained, are \textit{two very distinct processes}. Since this last paper does not appear in the Einstein archives, and since, astonishingly, Bose did not keep a copy of it for himself, we cannot know about its exact contents, except via the only document that still exists: Bose’s third letter to Einstein. On 27\textsuperscript{th} January, 1925, Bose thus writes to Einstein from Paris:

“I have tried to look at the radiation field from a new standpoint and have sought to separate the propagation of quantum of energy from the propagation of electromagnetic influence. I seem to feel vaguely that some such separation is necessary if Quantum theory is to be brought in line with Generalized Relativity theory.”\footnote{The discovery of the laser shows that while \textit{both} Einstein and Bose were (partially) right, they approached the problem of radiation from very different places. While Einstein’s understanding of radiation involved manifest appeal to causal processes, Bose thought about those processes \textit{simply in terms of statistical properties}, thereby making a causal account quite unnecessary (from his point of view). The history of the new quantum theory evolved of course without Bose (after his first paper), but it is this statistical point of view that remains Bose’s singular contribution in quantum mechanics.}

Partha Ghosh, who has produced a speculative reconstruction of what the lost paper might have contained (see Ghosh, 1994), suggests that Bose was on the track of a notion now called an empty wave (an EM wave carrying no energy-momentum), which subsequently did become an area of theoretical activity in later decades. If Bose had indeed conceived this idea, clearly, he was unable to hold on to it in the face of Einstein’s non-acceptance. His main idea in the second paper—which the emission of light should be seen as a unified, single process and induced radiative processes should simply be understood in terms of the \textit{statistical property} of the radiation field itself—was thus abandoned.\footnote{See Appendix 1.}
developed in order to counter his colonial isolation, tells us something about the processes of peripheral science by means of which nascent communities like Bose’s participate in the making of new knowledge and how, tentatively, they take their first steps towards a trading zone. If Bose’s success is inspiring, his “failure” over his second and third paper is equally informative for an adequate understanding of peripheral science. Bose made no further attempt to publish his paper, even though his note to Einstein mentions that he had shown it to Paul Langevin, who had thought it worthy of publication. What indeed had stopped Bose?

A Dacca University publication (Harun Ar-Rashid, 1994) which gives us information about Bose’s service file provides a plausible answer. Mehra or Stachel’s account make no mention of this material, for Mehra suggests that Bose sent his paper to Einstein on a note of sudden inspiration, based simply on the fact that he had used Einstein’s photon concept consistently in his proof. Bose’s service file however shows us that on August 27th, 1923, Bose had applied for a two-year study leave to Europe, planning to proceed on this leave from September 1924. Bose thus had already made plans to visit Europe, and his paper on Planck’s law was to be the basis on which he was hoping to obtain his leave. When the Philosophical Magazine, one of the leading physics journals of his day, rejected his paper, Bose found himself suddenly in the midst of a crisis, and sent his paper to Einstein, hoping to be saved from this crisis. Einstein’s prompt response actually saved the day for Bose and he was able to proceed on leave on time. Bose’s sense of deep personal gratitude to Einstein originated from the fact that not only Einstein’s theory, but Einstein himself, made it possible for Bose to reach his goal of becoming a scientist.

It is unlikely however, that Einstein ever noticed this strong emotional investment (in the form of a master-disciple relationship) on Bose’s part. He was interested in Bose’s ideas, but not in Bose himself, and since Bose’s further proposals went against his deeply-held convictions, he firmly rejected those proposals. But for Bose this rejection had a significant impact. Having projected onto Einstein a master-disciple relationship—a long-standing, epistemic tradition in his own home culture—Bose perhaps considered himself bound by the ‘loyalty’ of that relationship, even when that ‘loyalty’ was going against his own interests. Faced with Einstein’s lack of interest, Bose thus thought it appropriate to drop the whole line of thinking, turn his attention onto something else, and leave the entire field of inquiry.

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115 Though this application was accepted provisionally on 2nd March, 1924, but Bose’s employers did not act on his application until Einstein’s postcard arrived on the scene.
116 Academic knowledge in Sanskrit had traditionally been handed down through a master-disciple (Guru-Shishya) succession, in which the loyalty of each side to the other remains crucial.
Thus, while Einstein's extension of Bose's statistical insight became quickly a part of normal science, Bose ambitious proposal for a more substantial extension—equilibrium of radiation in the presence of material particles, and its generalization into a unified ‘fundamental’ view of the radiative processes, was entirely ignored. This is a substantial case of the Matthew Effect in science, where the credit of extending a research program goes entirely to the more established authority. Note that in this paper too, Bose had proceeded logically, just as he had done in his first paper. Both these papers in fact claim to have established a ‘more fundamental’ form of derivation by dropping all extra presuppositions and only using statistical mechanical procedures. Both show Bose’s dominant strategy of always relying upon mathematical and statistical arguments for the solution of theoretical problems.

Could Bose have succeeded in putting forth his version of the extension of the radiation theory in 1926? In other words, could he have prevailed over the controversy with Einstein over the radiative processes and develop his own ideas into a longer-lasting and more successful research program? If not, was this inability simply due to his personal qualities, such as his rather disorganized work habits, his propensity of throwing away manuscripts, writing several undated letters, and his ascription of the role of a master on to Einstein? Was this simply due to his general bent of dabbling into too many things instead of devoting sustained efforts in a few chosen directions?

In the light of the framework that I have developed in this dissertation, my claim is that this outcome stemmed from Bose’s location within an adversarial trading zone, which he was unable to take it to its next level. One would be tempted indeed to attribute this outcome to Bose’s personal habits alone, for this restores our rational image of scientific practice, according to which there is no peripherality in science. But Bose’s contemporary C.V. Raman also exhibited much of the same pattern in his later work on the lattice structure of diamonds. While Raman’s earlier work on light scattering paid off handsomely, winning him Nobel Prize within two years of his discovery (1928), his later work in lattice dynamics and on the physiology of human vision in Bangalore drew gradually diminishing returns. But unlike Bose, Raman was anything but chaotic: nor did he rely on personal contacts to fulfill his scientific goals.

Even apart from Bose’s peculiar personal characteristics, such as his generally disorganized work habits, his habit of putting too many irons into the fire at the same time, not to mention his excessive worship of Einstein, Bose had reached the limit of what could have been accomplished by the assorted strategies of a peripheral scientist. Aside from the ambivalence and the criticism that he had faced from Einstein, Bose’s proposals would have required extensive work—in the shape of careful elucidation and nurturing of new theories—a task for which he
was perhaps not prepared himself, and for which he would have needed an extended network of resources. Ideas similar to his second paper came back within a couple of years in the shape of Dirac’s method of second quantization, but by then Bose’s proposals lay long forgotten. It is a point to be noted that Einstein’s first reaction to Dirac was also decidedly negative, but Dirac did not share Bose’s peripheral location, and was thus fortunate enough to have escaped Bose’s outcome. Bose’s later conversations with Mehra suggest that he was conscious of having reached a limit of some kind: ‘I was like a comet, who came once, never to return again’. Thus, the career trajectory of Bose as a peripheral scientist tells us something about the trading zones of peripheral science and how such zones can briefly appear to be collaborative and return thereafter to their state 1 form of elite control. Note that Bose was a peripheral and a colonial scientist in all senses of the term. I have argued above that peripherality implies an epistemic dependence, a persistent inequality between two scientific communities in matters of knowledge. A peripheral scientist can certainly enter the field in the role of a maverick—this is what Bose did himself—thereby establishing the first stages of a trading zone with his metropolitan community. But such first steps are usually accompanied by considerable difficulty, even by adversarial interactions. Thus, while metropolitan scientists can make new knowledge by usually relying upon the resources of their home community, such is not the case with the peripheral scientists. Indeed, peripherality can be defined as the very condition when one cannot make new knowledge without involving another resource-rich and a more privileged epistemic community. This inequality impacts all further operations that the peripheral scientists undertake in the making of scientific knowledge. In a more intuitive language, this means that while such scientists can join in a metropolitan research program by filling in the gaps and by offering clarifications of existing research programs, their power of initiating new programs remain limited—which in turn essentially limit their power to achieve higher levels of theoretical synthesis (in which others join in and contribute). In the absence of such powers, their function in the making of new knowledge remains largely limited to the tasks of clarification and critical elucidation of the metropolitan research programs, to which they have allied themselves in their attempt to find a first foothold in research. While such scientists can perform many ‘mopping up’ operations in science, the higher level tasks of theoretical synthesis (and initiating new research

117 John Stachel suggests that Bose might have been uncomfortable in the later quantum theory just like many of his contemporaries, which would explain his early departure from the subject (Stachel, 2002). It is interesting to note that while in Europe, Einstein suggested that Bose look into two kinds of problems: whether the new statistics of Bose imply a novel kind of interaction among the light quanta, and secondly, what the new statistics and the transition probabilities of the radiation field looks like in the new version of quantum mechanics (Mehra, 1982, p. 571).

118 “I was really not in science any more. I was like a comet, a comet which came once and never returned again (Mehra, 1982).”

119 As I have said in the Chapter 1, such inequality can of course exist within as well as outside of a metropolis.
programs), remain largely a prerogative of their metropolitan centers. The success of such scientists thus becomes critically bound up with the roles that they get to play within these metropolitan programs. Thus the difficulties that they experience stem not only from their small critical mass at home, or from their encounter with sciences in the form of a ‘civilizing mission’—it comes from their deeper problem that the job of building an autonomous tradition in science must be accomplished while being quite dependent upon another community—for research problems, for methodology, and last, but not the least, for consensus and for professional rewards. The result is a complex epistemic trajectory, flashes of success combined with an overall peripherality, and a frequent return to the earlier adversarial form of trading zones after a short collaborative period.

Bose’s career after the failure of his second paper is evidence of this asymmetry in knowledge production that often prevails in peripheral science. With his first paper, Bose had amended successfully a metropolitan research program that led eventually to the new quantum theory, but his proposals in the second and third paper were pushing him beyond that limited peripheral role. His attachment to explaining things in terms of the statistical and the field properties had taken him to the territory where quantum mechanics sprang up in the subsequent decades. Given the tight space within which Bose had developed his science, it would not have been possible for him to hold on to his dissenting view in the face of strong metropolitan non-acceptance. But if dissenting opinions cannot be nurtured, new knowledge cannot be produced either. Faced with opposition from one of the highest authorities in metropolitan science, Bose did what would have been natural for any peripheral scientist to do in his position: he abandoned the field opting for diversification of his research problems, and moved on to new areas like crystallography and chemistry. These were the fields in which he stayed on for many years after his first love affair with radiation theory ended in 1926.

5.6. Trust, Conflict and Consensus: Interactions between a Metropolitan and a Peripheral Scientific Community

In chapter 1, I discussed the notion of a scientific community, which pursues science not necessarily out of epistemically pure goals, such as truth or unification of science, but for many other non-epistemic motives (Kitcher, 1993). Kitcher calls such science as epistemologically sullied, but goes on to argue that such sullied science can nevertheless be very productive from a community point of view—both in terms of the new results that are achieved, and also the new traditions that get founded on its basis. What we saw in the study above with Bose is an example
of such epistemologically sullied science, which was clearly productive from both the metropolitan and the peripheral side, and which brought into existence—though only for the brief space of two years—a trading zone in quantum mechanics comprised of both the European and Indian scientists. The controversy over radiation however soon led to the dissolution of this zone, causing Bose to abandon the field altogether.

A peripheral scientific community arises when there is a case of collaboration between two scientific communities, where one is resource-rich and the other is a newcomer, and thus the two are unequal in power and privilege. Nevertheless, the two unequal communities work together in the development of a research program, often to mutual advantage. The dominant motive on the peripheral side—who are frequently the ones to initiate these relationships—is to create a track record in scientific research for their own community, and thereby to forge a new instrument of knowledge for their own society. On the metropolitan side, such newcomers are often the productive bearers of new theories and new ideas. This is the situation that we see exemplified in Bose. However, this unequal relationship also raises several questions, which invite further philosophical clarification. When a dominant metropolitan community thus shares its research programs with a group of newcomers, what exactly is the role that these newcomers play in the development of that program? Does their work consist only in filling up the gaps of the metropolitan program? Or, do they also sometimes get to propose new programs of their own? How are trust and consensus handled in case there arises a controversy? In view of the case study that I have just presented above, and my framework in Part I, one can infer that in such situations there is always the possibility of a Matthew Effect on the peripheral side.

Still, such situations are productive opportunities for peripheral scientists to make new knowledge and establish their first track record within a new research tradition. In Part I, I considered an important approach by Weisberg (2009) which allow us to model such differential interactions in science, even though such analysis was not developed with a view to represent the peripheral scientists. Referring back to the distinction introduced by Michael Weisberg in chapter 1, mavericks and followers, let us now see what Bose achieved by his brief entry into the domain of quantum mechanics playing the role of a maverick—both for his home community as well as for the international community of science.

As for his home community, Bose clearly brought into existence a successful record in basic science, stimulating others to join the practice of science. As Weisberg tells us, followers are naturally disposed to follow mavericks, which eventually lead to an optimal condition in research. In the peripheral contexts, this can be the beginning of a new scientific community. Within the metropolitan community, Bose’s statistical approach created an abiding mathematical
framework that soon became a tool in the hands of the quantum physicists, giving them a new way to think about quantum interactions which began a new revolution in physics. While as a peripheral scientist, Bose’s personal success lasted only two brief years, located as he was in the adversarial first stages of a trading zone, he was nevertheless able to leave behind a substantial track record that others could follow—perhaps more productively—in the near future.

5.7. A Middle View of Peripheral Science

In considering these patterns in peripheral science, we see that peripheral science—at its most creative moments—is not necessarily a case of a diffusion as it is commonly supposed to be (Basalla, 1967). *Instead, it is the natural cognitive response of a society,* which, confronted with a new body of knowledge, seeks to forge a new epistemic instrument for itself out of that knowledge. Thus, in spite of its differential patterns, its asymmetric distributions of trust and authority, and the possibility of Matthew Effect, peripheral science should be considered *as a zone of possibilities,* which, if properly nurtured, could scale up to a progressive and productive science, eventually producing a sophisticated scientific community at a new social and institutional setting. Depending on the contexts of this community, it could be the beginning of a new national tradition in science.120 Thus, what we require is a philosophical theory about peripheral science that gives us new models of thinking about such scientific engagements and thus allows us to avoid the two extremes usually associated with evaluating science outside of its Western context: the first, to view it as a tool of enlightenment, or alternatively, as a surrender to a foreign body of knowledge. Instead, a proper philosophical analysis of peripheral science shows us both the possibilities and the difficulties inherent in that enterprise, thus leading us, eventually, to a middle view about peripheral science.

5.8. Conclusion

The conceptual space of the scientists who work from the peripheries of scientific knowledge requires a new analysis in order to show how they function as productive reasoners in science from their peripheral locations, how they function against a background and how they seek to achieve particular types of goals. Cognitive-historical analysis is a tool that can be

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120 A ‘national science’ might mean that the goal of this new community would be to address the problems that are most urgent in the contexts of their nation. Note that this does not prevent those results to be relevant for other communities, even supplying them with new inputs or data for *their* research. In other contexts, the outcome could be a new tradition of research *within* a previously-existing community, which now occupies itself with a different group of problems.
plausibly used for these purposes. The construction of such an analysis shows us how such scientists reach their conclusions by means of a variety of generative processes, while being located in a network of knowledge that is significantly different in kind than their metropolitan counterparts. This offers us a window to understand those complex, multi-layered processes that create and sustain science in such locations. This study presents a first attempt at putting together such an analysis, showing how scientists in such settings produce their concepts in a difficult environment that they are obliged to live in by virtue of their membership in two worlds and two cultures. To enter the Galilean-Newtonian tradition of Western science—and become known as its productive puzzle solvers—they first have to make an entry into the Western normal science. Only by making such an entry, can they gain the further ability to create a national science for their own community. But this raises the possibility of being placed in a difficult trading zone, with its consequent outcomes of asymmetry, and the possibility of never attaining a consensus.

My analysis suggests that Bose did this job well, laying the foundations of a research tradition both in theoretical (and later) in experimental physics in early 20th century India, but he succumbed, predictably, to the asymmetries of peripheral knowledge production. Thus, he failed to push the extension of his ideas to the next level, losing priority on his ideas that soon became active areas in research in metropolitan science—areas in which he could have perhaps made a more substantial contribution. What he did manage to leave behind was an exemplar, which he was successful in creating out of his asymmetric environment, finally leaving behind a track record (and a set of new research problems) for his successors. His partial success showed the difficulties that his peripheral successors would encounter, but his career also clearly signposted the promises inherent in their situation. Thus, Bose’s remarkable success—and his inevitable disappointment—gives us the classic case of a first-generation scientist who participates in science from a periphery.
Chap 6. The Raman Effect: Discovering Light of a Different Color

“When the Nobel award was announced I saw it as a personal triumph, an achievement for me and my collaborators — a recognition for a very remarkable discovery, for reaching the goal I had pursued for 7 years. But when I sat in that crowded hall and I saw the sea of western faces surrounding me, and I, the only Indian, in my turban and closed coat, it dawned on me that I was really representing my people and my country. I felt truly humble when I received the Prize from King Gustav; it was a moment of great emotion but I could restrain myself. Then I turned round and saw the British Union Jack under which I had been sitting, and it was then that I realized that my poor country, India, did not even have a flag of her own — and it was this that triggered off my complete breakdown.”

— C.V. Raman

“The first thing we scanned (with the Raman probe) was coffee grounds. Clearly organic”, he joked. “We’ll be able to tell if there is Starbucks on Mars.”

—Michael Storrie-Lombardi, NASA Astrobiology Center

6.1. Introduction

In contrast to S.N. Bose, C.V Raman, another scientist from the same peripheral Indian community, was able to maintain a steady presence in the scene of international science for more than forty years. His engagement with his metropolitan community was thus more complex, more prolonged, and consisted of several cycles of iteration.

Raman is known for his discovery of a new form of radiation, first seen in a light scattering experiment in his laboratory at Calcutta in 1928. This discovery is described by the American Chemical Society (on their website) as a historical chemical landmark. Since 1930, Raman spectroscopy has been a standard technique both in physics and in chemistry. Not only did it provide the physicists with a proof that light indeed possesses quantum structure (due the energy-exchange between the light quanta and the molecules of the substance), it allowed the chemists —and still later, biologists and forensic chemists— to analyze any number of substances non-invasively (i.e., without destroying the physical samples). Since the discovery of the laser, Raman spectroscopy has become the standard technique for analyzing anything whatsoever, from quality control processes to detecting malignant transformations in biological tissues.

Yet, Raman produced this discovery from his vantage-point of a peripheral scientist, relying entirely upon his ability to grasp a metropolitan research program from a distance. He was assisted by a small group of students— all of whom were trained indigenously— and although never actually deficient, his resources always remained quite modest. His discovery of a new
form of radiation is thus a good example where new knowledge in science was indeed made from a peripheral location.

To explore what lay behind this discovery, and how Raman was able to arrive at a specific research question by extending a current metropolitan research program in a specific direction, we will have to re-construct Raman’s cognitive trajectory— which will involve putting together his chains of reasoning—that took him to this result. We will also have to explore the knowledge-networks within which he was situated (and which he helped to extend further). We will have to show how this network varied in the course of Raman’s scientific practice, eventually creating for him a stable interface with his metropolis: the European scientific community. Thus, the task of making new knowledge in science in Raman’s case must be explored by means of a two-fold analysis: first, at the individual level, exploring the reasoning and the knowledge-constructing practices that Raman (and his group) made use of, but also at the level of how the two communities related with each other, producing, eventually, for Raman, a recognition and a consensus.

While there had been contributions in science from India prior to the period of 1910-1930, those contributions were made by individuals rather than by an emerging scientific community. But during the period of 1920-30, we see the emergence of a small scientific community with a group of people, an education system, and an environment that guarantees the future replication of such a professional community. To this building of a macro-environment for Indian science Raman contributed crucially, both by producing a personal exemplar as well as developing the general networks of communication with which such a community sustains itself.

Unlike S.N. Bose, whose style of scientific communication consisted, mostly, of developing a number of personal contacts with his metropolitan peers, Chandrasekhar Venkataraman— commonly known in the West as C.V. Raman— preferred an institution-based approach. Furthermore, Raman maintained his contacts with his metropolitan community for a prolonged period of time. Thus, he embodied the two aspects of peripheral practice that I have outlined above: firstly, the building of an overall structure that can sustain such an activity, and secondly, the creation of an exemplar, which gives a nascent scientific community its first foothold in scientific research, creating the possibility of further contributions in science. It is this second activity that Raman accomplished from his IACS laboratory at colonial Calcutta. Thus,

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121 For example, Ardaseer Cursetjee in 1841 and Srinivas Ramanujan in 1918 had become the first two Indian Fellows of the Royal Society.

122 Indian Association for the Cultivation of Science (IACS) was established in 1876, in the model of the British Institution by Mahendra Lal Sircar, a prominent physician in Calcutta.
in Raman’s contribution to science in India, we see the full-scale peripheral schema at work: first, the framing of a research question derived from a major metropolitan research program—*a problem whose solution would be valued at a metropolis*—and at a later stage, interaction with that community with respect to that result, with the ultimate goal of gaining an endorsement and a consensus for further work. Together, these two stages lead to the formation of an *interface* with a metropolitan scientific community, and as I have argued already, it is by means of such an interface that a peripheral community begins its career in the work of sharing a metropolitan research program. When successful, such collaborations lay the groundwork for a new scientific tradition within the peripheral community, thereby starting a new loop of knowledge. With further iterations of this loop over time, the network between the two communities gradually assumes the shape of a *collaborative zone*.

In what follows, I shall trace this building of a collaborative zone by exploring Raman’s optical researches on light scattering, finally leading to his discovery of a new form of secondary radiation in 1928, now known as the Raman Effect. This research on light scattering that Raman undertook in 1927 at the IACS laboratory in British Calcutta was largely a result of his still earlier researches on the color of the blue sea, to which he offered a new explanation, and in general, from his intrinsic interest in the optical phenomena of diffraction and light scattering. In tracing Raman’s footsteps through these problems (and to the solutions that he developed to some of them), I shall briefly refer to his still earlier researches on acoustics, mainly to show how by doing that work, he had made himself highly skilled in the analysis of wave phenomena and the use of a visual mode of experimentation. Of his later work done in Bangalore on the lattice structure in diamonds, I shall only include a passing reference, mainly to observe how, by that time, Raman had once again become a peripheral researcher, having lost much of the metropolitan interface that he had gained earlier by means of his work on light scattering. Thus, by the end of his career, we find Raman--once again--as a scientifically isolated (and somewhat out-of-touch) peripheral researcher, despite his generally acknowledged great skill in analyzing wave phenomena.

In what follows, my analysis of Raman’s work, and his eventual discovery, shall be guided by two main questions: what kind of knowledge-constructing practices were Raman using to reach his conclusions, and how did his thinking develop from one stage to the next? Put differently, this will lead to a consideration of three factors: what motivated him to select his research problems, what concepts and what forms of reasoning were guiding his thinking, and what kinds of experimental expertise made his results visible for observation? The answer to
these questions will give us an insider’s view of the science that Raman developed— for a while at least—at colonial Calcutta.

6.2. On the Color of the Blue Sea

In 1921, while returning home from a tour of Europe and his first visit to the West, where he had gone to participate in the Universities Congress of the British Empire, Chandrasekhar Venkataraman, a professor in the Calcutta University in its newly-established graduate department in Physics, became attracted by the unusually brilliant blue color of the Mediterranean Sea. Contrary to the prevalent view proposed by Lord Rayleigh—who believed that the blue color was the effect of the reflection of the sky in water—Raman proposed that the blue color emerges from the sea water itself. Thus, the color of the sea is a distinct phenomenon due to the nature of the water molecules themselves; in other words, the blue color is a result of molecular diffraction.

In two quick notes sent to *Nature* (108, p. 367 and p. 402, 1921) right from his ship at S.S. Narkunda even before it reached the Bombay harbor, and one year later in a longer paper published in the *Proceedings of the Royal Society* (101, 1922), Raman outlined his arguments for his molecular diffraction view against Rayleigh’s and Tyndall’s then prevailing reflection-of-the-sky view. What convinced Raman of Rayleigh’s mistake was a set of visual observations that he had made right on the ship itself by using a Nicol prism. By viewing the surface of the water with the prism approximately at the same polarizing angle as the surface of the sea, Raman managed to quench the reflection of the sky on the sea water and found that far from eliminating the effect, the blue of the sea was thereby wonderfully improved. Furthermore, with the variation of the azimuth of observation relative to the plane of incidence the quality of the effect (i.e., the color and intensity of light) varied as well. These two observations convinced Raman that the effect could only be explained by appealing to the diffraction mechanisms that takes place during the passage of light through the sea water. Raman then asked the all-important question: “what is it that diffracts the light and makes its passage visible?” (Raman, *Nature*, 101, 1921, p.368)

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123 This was definitely not Raman’s first sight of the sea for during his student days in the Madras Presidency College, he must have been looking out at the Marina Beach and the Bay of Bengal almost every day. See Rajinder Singh’s timeline on Raman (Singh, dissertation, 2004) where Singh argues that Raman had already been aware of the existing literature on optics but this may not have been at the center of his attention.
124 Diffraction implies changes in the direction and intensities of a group of light waves, such as bending or stretching, after passing by an obstacle (similar or smaller in size to that of the wavelength). Luckily, in Raman’s time, observation with simple instruments (such as a Nicol prism) still went a long way.
125 Azimuth is the angle of compass direction from which the sunlight comes. It is commonly measured in angles in navigation and astronomy.
question provided Raman with the starting point of his researches on the optical scattering of light, which lasted for the next seven years and occupied his entire attention, and which gained him, eventually, a discovery and the Nobel Prize in Physics in 1930.

Lord Rayleigh had shown earlier that the color of the sky is a product of the scattering of light by the air molecules. Rayleigh’s scattering law was thus the general framework for explaining wave-phenomena in Raman’s time. Raman’s explanation of the color of the blue sea thus utilizes the same argument as that of Rayleigh, but points out the shortcomings of Rayleigh’s explanation in another media. In contrast to Rayleigh’s theory, according to which the molecules scattered energy independently of one another (and the phases of the scattered waves remain uncorrelated), Raman was dealing with the liquid media, where the molecules enjoy much less freedom, being far more densely packed.

The next step is clearly the staging of an experiment that confirms Raman’s theory under experimental conditions. Since Rayleigh’s standard theory was obviously applicable only to gases, Raman decided to use the Einstein-Smoluchowski (E-S) diffusion equation to obtain a quantitative measurement of the phenomenon. What he was seeking thereby was to obtain a precise measurement of the intensity and the polarization of the light scattered by sea water. As his next short note to Nature showed (108, p. 402, 1921), this is exactly what he did once he arrived back at his laboratory at 210 Bowbazar Street, Calcutta (October, 1921). Using ordinary city water as samples, and repeatedly filtering those samples with alkali and alum, eventually using distilled water, Raman succeeded in obtaining a feeble track of the azure blue of the sea under experimental conditions. A quantitative measurement of the scattering phenomenon was obtained after comparing its results with the scattering intensity of dust-free air: “the scattering power of a pure sample of water was 175 times that of dust-free air under standard conditions”, Raman reported in the 1922 Royal Society Proceedings. This was well within the predicted range of the E-S equation (160 times); the presence of the $1/\lambda^4$ factor in the E-S equation ensured that blue light would be scattered more than any other wave length of the spectrum, the longer wave-lengths being selectively absorbed (see Table 5.1). Taken together, this is the mechanism that explains the intense blue color of the Mediterranean Sea.

\[126\] The E-S equation, independently proposed by both Marion Smoluchowsky and Einstein, provided a way to account for the diffusion or scattering phenomena in liquids. In the case of an ideal gas, it also easily reduces to Rayleigh’s formula.
Equivalent Km. of dust-free air

<table>
<thead>
<tr>
<th>(\lambda) in (\mu\mu)</th>
<th>658</th>
<th>622</th>
<th>602</th>
<th>590</th>
<th>579</th>
<th>558</th>
<th>522</th>
<th>494</th>
<th>450</th>
<th>410</th>
</tr>
</thead>
<tbody>
<tr>
<td>Equivalent Km. of dust-free air</td>
<td>0.4</td>
<td>0.5</td>
<td>0.7</td>
<td>1.4</td>
<td>2.5</td>
<td>3.0</td>
<td>50</td>
<td>40</td>
<td>24</td>
<td>15</td>
</tr>
</tbody>
</table>

TABLE 6.1: Selective absorption of the Wavelengths of Sunlight in Sea Water
Taken from C.V. Raman (Proc. of the Royal Society, 101: 1922, p. 70)

With this explanation of the blue color of the sea, Raman accomplished several important tasks. First, he made an attempt to improve upon—as well as join in—an existing metropolitan research program in the domain of optics. Secondly, he identified a problem area within that program and offered a suitable contribution to it. By doing this, he had extended an optical problem from gases to liquid media, and experimentally produced an accurate and precise result, using a different law than that of Lord Rayleigh.

Scattering of light in liquids and vapors was indeed the area where Raman made his most prominent mark, and this is the beginning of that research. Also, Raman embarked upon this work right after his return from abroad. This clearly shows his response to the scene of international science—and his grasp of a new research program—for until 1920, Raman’s main research focus was acoustics, not optics. However, the focus of his research changed permanently once he solved the problem of the blue sea. Light scattering was a dominant research area in the physics laboratories worldwide during the 1920s. The behavior of light was being studied by Lord Rayleigh in England, Jean Cabannes in Paris, Robert W. Wood in New York, and Grigory Landsberg and Leonid Mandelstam at the Institute of Physics in Moscow (Barry Masters, 2009, pp 42-43). In moving away from his earlier research in acoustics and taking up optics as his all-encompassing interest, Raman, a peripheral researcher from one of the distant dominions of the British Empire, who until then had worked mainly on a number of self-chosen topics, had entered a vibrant research area in metropolitan science.

6.3. A Brief History of Molecular Scattering

The blue color of the sky attracted much speculation ever since the days of Leonardo da Vinci, who believed that a finely divided matter filled the space from the human eye to the

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127 Mandelstam and Landsberg would in fact independently go on to observe the same effect in crystals in 1928 that Raman did in liquids and vapors, just a few months ahead of the Russians.
outer parts of the space. A modern explanation of this phenomenon was attempted first by Tyndall (1820-1893), who suggested that the blue color of the sky is due to the scattering of light by the numerous suspended particles in the atmosphere. Against this view, Lord Rayleigh (1842-1919) showed that the blue color of the sky is really due to the scattering of light by the molecules of air. The intensity of light scattered by the molecules was explained by Raleigh by means of the following equation:

\[ I = I_o \frac{8\pi^4 \alpha^2}{\lambda^4 R^2} (1 + \cos^2 \theta) \]

Where \( I \) is the intensity of the scattering, \( R \) is the distance to the particle, \( \theta \) is the scattering angle, and \( \alpha \) is the polarizability of the particle.

Note that according to this equation, the intensity of the scattered light varies inversely to the fourth power of the wave-length, which means that the shorter blue waves will scatter more than the longer waves like red or yellow, thus giving the sky its characteristic blue appearance. Rayleigh’s equation on light scattering started a new experimental field—known as the molecular scattering of light—and various theories were proposed to explain the propagation of light in homogeneous media, measuring its intensity and polarization during such propagation. Applying Rayleigh’s formula to the denser medium of liquids proved more difficult however. Attempts of early workers, such as Tyndall and Spring, to demonstrate a scattering effect in liquids were not successful, and it was not until 1913 that W.H. Martin was able show a scattering effect in liquids by using an optically pure (i.e., free from suspended particles) sample of water and alcohol. By addressing the problem of the blue sea on his return sea voyage to India, it is this research area in which Raman had made his first contribution.

Raman soon expanded on his 1922 paper, published in the *Proceedings of the Royal Society*, by means of a 50-page monograph on optics. In this monograph, known as *Molecular Diffraction of Light* (Calcutta University, 1922), Raman summed up the prevailing state-of-the-art knowledge in optics of his time, outlining the main areas of controversy, and laying out, finally, an ambitious research program for himself and his group. Dividing the discussion into nine chapters, he addressed his future research topic—molecular scattering in liquids— in Chap IV, finally ending by sketching how molecular diffraction can be used as a key to provide information about the quantum nature of light.

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128 In the 19th century, it was also believed that the atmosphere is pervaded by some kind of coloring agent, e.g., a blue gas, which produces the constant blue color of the sky.
Two things especially stand out in Raman’s reasoning in that monograph: firstly, his statement that the wave theory of light might require an amendment (in the future) in the light of the experimental anomalies discovered in the ES equation, and that the phenomenon of optical scattering could be used as a probe to find out the nature of matter and how matter interacts with light. The monograph ends by suggesting two possible lines of experimentation that Raman reports his group had already undertaken to decide matters one way or the other.

What claims did Raman make in that monograph? He began with the statement that the whole edifice of physics is built upon the hypothesis of atomic/molecular constitution of matter, and physical optics has to conform to this ultimate hypothesis. The propagation of light through refractive media and the phenomenon of scattering thus give us a window through which we can view how the aggregation of matter causes properties of substances. Thus, Raman’s interest in light scattering is not purely experimental: what he seeks to gain from it is evidence for an improved theory. The need for such a theory has been suggested by several anomalous experiences: for example, in the junior Rayleigh’s experiments on the scattering power of saturated carbon dioxide vapors (at 21°C), which was found to be only 102 times more than that of gas while the E-S equation predicts it to be 855 times greater. A similar anomaly was found also in the behavior of liquids near their critical point, which suggests that the Einstein-Smoluchowski formula, which until then has been used as a reliable framework for explaining all scattering and diffusion phenomena, may not be true to the facts. Investigations in Raman’s laboratory by K.R. Ramanathan showed that a similar discrepancy exists in the case of scattering power of liquid carbon dioxide. On the basis of such anomalous data, Raman asks the most important question in his monograph: “does any departure from perfect regularity of the light propagation arise from the discontinuous structure of the medium?” (Raman, 1922, p.39).

Deviations in simple wave propagation (and its theoretical implications) can thus give us a window to discover the ultimate, underlying structure of matter and how matter interacts with light. This is the thinking that lies behind the Raman Effect, and his 1922 publication shows that Raman had already formulated this as his research goal. The problem of the disagreement between the experimental data (scattering power of liquids and vapors) and the predictions of the Einstein-Smoluchowski equation showed Raman that there lies a deep problem within the presuppositions of that formula. Since the E-S equation expresses the scattering power of the substances in terms of their refractive index and their compressibility, and since both of these presuppose Maxwell’s theory of light (as well as kinetic theory of matter), the predictions from this equation in effect become a test for the wave theory of light. If those predictions are not

129 Near the phase change point, for instance, when matter changes from liquid to gases or vice versa.
borne out by actual observations, then this calls for new experimental investigations and perhaps a theoretical revision. Raman’s innate aesthetic response to color phenomena, his perception that he had finally found an important problem area within metropolitan science where he could make a prominent mark, his intense sense of nationalism, all combined together to determine that he would enter this new research area. As Rajinder Singh points out (Singh, dissertation, 2004) optics had interested Raman even before his trip to Europe (he studied corona and haloes), but it was the aesthetic experience of the blue Mediterranean sea—and his perception of a problem situation at the heart of physical optics—that transformed it into a dominant interest.130

6.4. Constructing A Research Problem in Optics

The conceptual implications of Raman’s explanation of the blue color of the sea were tremendous for this lead him into his subsequent cycle of research. First, there was the growing dissatisfaction with the E-S equation on the basis of his experimental data, which in turn led him to determine the scattering coefficients of liquids, gases and vapors under different temperatures and pressures. It is this phase of research that finally lead to the discovery of the Raman Effect. While his explanation about the blue sea could well be construed as a straightforward application of the wave theory of light—an already well-established metropolitan research program—a careful analysis of its circumstances show that this was not quite the case. Far from being the routine application of an existing theory, the episode of the blue sea in fact made Raman aware of a challenge, thereby drawing his attention to the existing anomalies within the research program of optics. Raman had used the ES equation as a tool to explain the natural phenomena of the blue sea, but as he reports himself in that monograph, the discrepancy between the predicted values of the E-S formula and his own (as well as Rayleigh’s) experimental data—set him thinking about the possible future lines of research. Thus, his work on the blue color of the sea formed a watershed in his thinking, bringing him experimentally face-to-face with the molecular nature of light.

Thus, from his peripheral vantage point, Raman was quick to grasp an emerging research program in the European metropolitan science, solely on the grounds of its consistency and its promise of unification. In 1922, the general consensus in the physics community at different European metropolis (es) was not in favor of the view of light quanta. Planck’s formula

130 S. Ramasheshan suggests that the roots of his interests in color may go even earlier, to his early reading of Helmholtz and Helmholtz’s description of the phenomena of ice and water in that work. This is a plausible suggestion, for which of course, more historic evidence is required.
was considered to hold only in cases of emission and absorption of light, leaving the rest of the wave theory quite intact. Yet, Raman in 1922 argues that light quanta have the clear virtue of consistency from a purely philosophical point of view. According to Raman, it is not possible to hold the clearly inconsistent position that while emission and absorption of light will obey discrete laws, the rest of the propagation of light will remain a continuous process. To eliminate this theoretical inconsistency, Raman suggests that there must be a re-thinking of the whole problem. Perhaps light could be viewed as composed of many smaller vorticences that could exist separately within a continuous stream of energy. All of this suggests however that an extensive program of experimentation must be undertaken in the area of light scattering to find out more evidence either for or against the wave theory of light. Thus, towards the end of his monograph, Raman sketches an ambitious plan, indicating that such research is already under way in his laboratory, and that there would be a breakthrough soon. To show that this was indeed his view back in 1922, I quote Raman extensively from the chap. IX of his monograph:

If… we view the matter from a purely philosophic standpoint, Einstein’s original conception of the discontinuous nature of light has much to recommend it. It fits in with the assumed discontinuous character of the emission and absorption of energy as part of a consistent and homogeneous theory, whereas the idea that emission and absorption are discontinuous while propagation of light itself is continuous belongs to the class which Poincare has described as ‘hybrid hypotheses’. Historically, Maxwell’s theory is the embodiment of the belief of 19th century physicists in the validity of Newtonian dynamics as applied to phenomena occurring in the medium which was postulated as pervading all space. The belief in the validity of Newtonian dynamics as applied to the ultimate particles of matter has however received a rude shock from the success of the quantum theory as applied to the theory of specific heats, and there seems no particular reason why we should necessarily cling to Newtonian dynamics in constructing the mathematical framework of field-equations which form the kernel of Maxwell’s theory. Rather, to be consistent, it is necessary that the field-equations should be modified so as to introduce the concept of the quantum of action. In other words, the electrical and magnetic circuits should be conceived not as continuously distributed in the field but as discrete units each representing a quantum of action, and possessing an independent existence, somewhat in the manner of vortex rings in a perfect fluid. Interference and diffraction phenomena may then be conceived as arising from the approach or separation, i.e., crinkling of the mean ‘lines of flow’ of energy in the field. (Emphasis added)

Gathering new evidence for such a revised theory should begin, according to Raman, by considering the non-catastrophic behavior of light, such as the phenomenon of light scattering, which is clearly most intimately connected with the propagation of light. Scattering is, in fact, is
the very same process as the propagation of light. Thus, Raman sought to extend his consideration about the nature of light away from the occasional and violent occurrences, such as the photo-electric effect. The phenomenon of light scattering in fact provides the ideal backdrop to investigate the general nature of light. If all scattering phenomena—a most common process in the propagation of light—can be explained by using the wave theory, then the case against Einstein’s position is enormously strengthened. If on the other hand, the wave theory of light fails to explain all observed facts of scattering, then we must revise our current ideas about the nature of light.

Raman thus suggests that the failure of the ES equation could be due to three possible factors: first, the equation is not valid under the particular circumstances in which Rayleigh made his observations, secondly, the kinetic theory of matter is not valid, and finally and most importantly, the continuous nature of light requires revision. As Raman saw it in 1922, the discrete nature of light could indeed provide an answer for the observed low values of the ES equation in the following manner.

Consider light as a stream of quanta passing through a highly compressed gas. Scattering of light would thus occur only when one of those quanta collides with a particle according to the laws of chance, and gets deviated from its path by a large angle. Such encounters naturally would involve only a small number of molecules at a given time, and would thus be proportional to the number of molecules per unit volume. This suggests a low value for the scattering coefficient, precisely what both Rayleigh and Raman had found in their experiments. On the other hand, if we think of light as a continuous stream of energy, then we obtain a much higher value, because then we are committed to the position that all the molecules are scattering light all the time.

Thus, with the evidence of anomaly in the predicted values of the E-S equation, supported by his own experimental results observed in carbon dioxide vapors, Raman developed an ambitious research agenda in optics. In the last two sections of his monograph he outlined two possible lines of research, which, according to him, could be used as two sets of crucial experiments. In the first set of experiments, Raman’s student, Ramanathan, was seeking to confirm Rayleigh’s results for scattering in compressed carbon dioxide—extending it to unsaturated vapors and to gases at temperatures considerably higher than the critical temperature. In the second set of experiments, done by Kameshwara Rao, Brownian motion was being quantitatively studied in gases and vapors under high pressures. Raman had thus already started thinking about the scattering of light from a microscopic point of view (Venkataraman, p. 186), holding henceforth that “scattering of light can take place even in molecules”. (Singh, Current
The behavior of scattered light is thereafter linked directly to the ultimate constitution of matter.

The theoretical point that Raman was trying to decide by his proposed experiments is now easy for us to see. If the first series of experiments supported Rayleigh’s lower values of diffraction coefficients, then it is clear that the Einstein-Smoluchowski equation does not represent the facts, and must be revised in favor of the quantum theory of light. The second set of experiments was aimed to find out if the energy in the molecular movement agrees with the predictions of the kinetic theory. As we know now, Raman’s first set of scattering experiments (in liquids and vapors) did indeed lead him to the discovery of a light of different frequency than the incident frequency, thereby providing confirmation for the quantum nature of light.

What experimental set-up did Raman use by means of which these scattering experiments would provide evidence? The experimental set-up that Raman developed for his scattering experiments evolved naturally from his earlier work on acoustics, where he had used a similarly visual method for observation. Intuitive, visual arrangements were characteristic of all of Raman’s experimental set-ups, shaping in the end, his views on the relationship between theory and observation. He departed radically from the practice of the European physicists of his time who usually photographed their data—using sometimes very long periods of exposure time. Against this established procedure, Raman decided to rely upon the deceptively simple technique of making visual observations in real time. Sunlight was the primary light source for all his experimentation. Thus, he took advantage of the strong tropical sunlight, available abundantly at Calcutta. It was however still necessary to produce a more intense beam of light. When the IACS acquired a 7-inch refracting telescope in 1927, Raman was able to use this to create a more intense beam of light, and in 1928, once mercury arc lamps became commercially available, he finally used this to produce an even more intense beam of light. While making observations, Raman’s practice was to take data visually at first, and only then use a quantitative analysis, such as a baby Hilger spectroscope, which he finally used to take the spectra of his modified scattering.

Most interestingly, it seems that Raman had developed a technique to use his own dark-adapted eyes as photo-detectors—all frequency shifts were first identified visually and only then subjected to further spectrographic analysis. The trained human eye can, in fact, detect even a single photon, and thus over time can be turned into a highly sensitive optical instrument (Masters, 2009, Optics and Photonics News). Indeed, in all his notes to Nature reporting his discovery, Raman consistently mentions the feebleness of the secondary radiation, and yet, he still reports his success (and that of his group’s) in isolating the feeble phenomenon. This suggests clearly a high degree of embodied skill and an ability to manipulate and prepare the experimental
set-ups and samples. The purified and prepared samples in Raman’s laboratory were in fact playing the role of *epistemic artifacts*, providing an interface between the observer and the inaccessible properties of the world (Tweney, 2006). It is noteworthy that other researchers like P.A. Ross had tried to find such a radiation before Raman in 1923 — deducing the existence of such a phenomenon from the works of the Austrian physicist Smekal and Werner Heisenberg—but could detect nothing in their samples. Thus, by using the strong tropical sunlight in Calcutta as a source, a telescope as a collector and a condenser, and his own trained eyes as the ultimate photo-detector, Raman had managed to evolve an experimental set-up that was both the simplest and the most effective at the same time.
FIG. 6.1. Raman at His Baby Hilger Spectroscope, Taken from *C.V. Raman: a Pictorial Biography* (1988)
FIG. 6.2. Raman’s Experimental Arrangement and the Photograph of the First Raman Spectra, Taken from *C.V. Raman: a Pictorial Biography*
6.4.1. An Optical Analog of the Compton Effect?

Raman ended his 1922 monograph with the question: “does any departure from perfect regularity of the light propagation arise from the discontinuous structure of the medium?” The first proof that such departures exist came in 1923 from the work of Arthur Holly Compton, an American physicist, with whom Raman had become personally acquainted during his lecture tour to North America and Europe in 1924. Compton had shown that when an X-ray or a gamma ray photon collides with matter (knocking out an electron), the photon loses energy, thus shifting to a longer wavelength. This departure from a perfect regularity, technically called inelastic scattering, was thus confirmed in the case of X-rays. While in elastic scattering or Rayleigh scattering, the scattered light maintains the same level of energy-- and thus the same frequency-- in inelastic scattering the scattered light changes frequency, either by losing (or occasionally gaining) energy. Given the state of optical researches in that time, Compton’s discovery proved a major breakthrough, fetching him the Nobel Prize in 1927.

Raman, who in the meanwhile had fully occupied himself with his extensive research program of finding the scattering coefficients of liquids, dense vapors, and gases over a wide range of pressures and temperatures— as well as examining carbon dioxide at high pressures and light scattering at liquid boundaries— correctly deduces that there must be a similar optical analog of the Compton Effect in the visible spectrum of light, which experimentally means that in a light scattering situation, normal Rayleigh scattering would be accompanied by a modified scattering, i.e., scattering of a different frequency, and thus light of a different color, thereby in effect, getting two colors out of one. This gives Raman (as well as his group) a definite phenomenon to look for in their samples. Thus, historically, Compton’s results became a powerful psychological motivator for Raman, giving him an analogy, and starting a race towards discovery.131

131 The molecular level explanation of the Raman Effect is now known to be different from that of the Compton Effect. While both demonstrates the quantum nature of light, in the case of Compton Effect, radiations of high energy (X-rays) interact with the electrons surrounding the atom, causing the X-ray photon to lose energy to the electron. In case of Raman Effect, radiations of much lower energy, i.e., visible light, interacts with the whole molecule and the photon thereby can either gain or lose energy. The interaction however causes the molecules to part with energy in the shape of an EM wave, which produces the light of a different color. Hence, the analogy between the two is not quite perfect. Much has been written on the issue whether Raman really understood his own effect, but I would like to bypass that issue here. From a discovery point of view, Compton’s prior discovery did motivate Raman to look for a similar effect in the visible spectrum of light, and therefore the analogy between the two still remains appropriate.
As it turned out, an unexplained light of a different color had indeed been seen for some time by Raman’s group, well before the researchers realized what they were looking at in their samples. In 1923, while looking into the vapors, Ramanathan had found some “feeble fluorescence” in his samples that stubbornly persisted, despite his repeated attempts at purification. This same “fluorescence” was once again seen in 1927 by Venkateswaran, and was once again dismissed by him as due to the impurity in the liquid sample. Even in 1923, Raman felt dissatisfied with the explanation that this strange “feeble effect” comes entirely from fluorescence. Thus, in late January 1928, Raman brought his student K.S. Krishnan to look for a possible secondary radiation by examining the passages of light in various forms of liquids. Thus, it was Krishnan who undertook the final laborious task of examining 60 samples of liquids, eventually generalizing his search into vapors and gases. Furthermore, Krishnan maintained also a daily journal of his observations, which has now become the standard source for the first moments of the discovery (Mallik, 2000). In all their experimental arrangements, the Calcutta researchers followed the same simple, real-time set-up. Using a monochromatic beam of 4358 Å, violet filters were placed in the path of the incident sunlight so as to isolate only violet rays. The incident beam was then passed through the liquid (or vapor or condensed gas) sample. Seated at 90 degrees to the sample, the observer was equipped with a green filter before him so as to detect any modified scattering. While most of the emerging track (from the sample) was due to the usual Rayleigh scattering, Krishnan found the presence of a modified scattering by using the green filter. Krishnan’s diary reports that he observed this “polarized fluorescence” both on the 7th and 8th of February, and drew Raman’s attention to it. Their joint note to Nature, reporting this observation, was published promptly on February 16th. On February 28th, the modified scattering was confirmed by using a direct vision spectroscope; the highly polarized character and the feebleness of light confirming that it was indeed a case of true scattering, and not simply fluorescence.
Raman wasted no time in making this discovery known. He immediately alerted the Associated Press of India, and sent off another quick joint note to *Nature*, which was promptly published on March 8th. One week after this publication, on 15\textsuperscript{th} March, Raman gave a well-publicized lecture in Bangalore on his new Effect, this time accompanied by the spectra of his modified scattering. On 7\textsuperscript{th} May, *Indian Journal of Physics* received Raman’s detailed paper on the discovery, and 2000 reprints of his lecture, titled “A New Radiation” were sent off to physicists in Germany, France, Russia, Canada and USA. Another detailed paper was sent to the *Proceedings of the Royal Society* as well as three other short notes to *Nature*.

What did the discovery of the modified scattering establish about the nature of light and its interaction with matter? This was, after all, Raman’s original research project back in the 1922. Rutherford explained this quite simply: “Scattering changes the color of light, and thus can be used as a key to understand the nature of the substance that scatters it.” This explanation shows indeed why the effect soon became a tool in the hands of analytic chemists. Conceptually, the Raman effect means that when a beam of light is incident on a liquid, (or solid or gas), it will be scattered by that medium instead of being absorbed, and in the process of being so scattered the frequency of its wavelength will shift from $\nu$ to $\nu'$. While in Rayleigh scattering there is no
change in frequency, and thus no change in color, in Raman scattering there is always a change in frequency and thus always a change in color. Thus, this deceptively simple phenomenon holds the most profound implications. As Raman himself said, "The character of the scattered radiations enables us to obtain an insight into the ultimate structure of the scattering substance." Raman scattering thus can be used to discover the molecular signatures of substances that produce the scattering, and hence, Raman scattering can investigate matter “from a microscopic point of view”. The difficulties of Raman spectroscopy comes from its extreme feebleness, but once the laser was invented in the 1960’s, this problem was completely overcome, thus making modern-day commercial Raman spectroscopes (fitted with a Fourier transformation screen) a universal tool for analyzing the molecular composition of any element.

Fig 6.4. A Modern-day commercial Raman Spectroscope. Image courtesy of www.thermoscientific.com, accessed on 08.12.2010. Note the same experimental set-up despite the modern look of the equipment.

How do we analyze Raman’s path to the discovery? Fortunately, Raman himself gives us ample data to answer this question. What Raman was able to create in his laboratory was a process (his experimental manipulation of prepared samples plus the condensed light beam) that interacted reliably and dynamically with a structure (the molecular nature of matter), thereby making that structure amenable to observation and inference. Thus, in his research on light scattering Raman used experimentation as well as his prepared set of samples as epistemic artifacts, seeking thereby to construct a theory of how light interacts with matter. Such use of
artifacts as exploratory tools in research is a well-documented cognitive strategy in science, and earlier metropolitan scientists, such as Michael Faraday, had used this strategy quite extensively (See Tweney, 2006 for a detailed account of Faraday’s experimentation on gold colloids). It is a point to be noted that by his self-taught method, Raman had made himself a part of the 19th century tradition of the experimental naturalists, that included Faraday, Helmholtz and Rayleigh.

The search for a better theory of light, and the details of its interaction with matter, was the goal with which Raman had set out in his optical researches. The long interaction between his laboratory practices and his experimental artifacts can be tracked in more detail in Raman’s different published papers until 1930. The span of his light scattering research, which lasted from 1922 to 1930, this goal was modulated by two things: first, a gradual refinement of a visual style of experimentation (upon which he relied for the rest of his life) and his manipulation (and preparation) of various artifacts that revealed their properties specifically upon such manipulation. Behind this, of course lay a theoretical framework in which we see at least three elements: his own discovery of the experimental anomalies in the E-S equation, his awareness of the Kramers-Heisenberg Effect, and finally, the most powerful motivator of all— an analogy with the Compton Effect. Compton had reported a similar, degenerative shift in the frequency of X-rays once they are diffracted by a molecule. It is this last factor that gave the Indian researchers a definite goal to look for in their research. This is well-confirmed in Raman’s note to Nature (Feb 16th, 1928)—as well as in the Nobel Lecture that he gave two years afterwards.Raman’s first note to Nature begins by clearly linking his discovery to Compton’s work: “If we assume that the X-ray scattering of the 'unmodified' type observed by Prof. Compton corresponds to the normal or average state of the atoms and molecules, while the 'modified' scattering of altered wave-length corresponds to their fluctuations from that state, it would follow that we should expect also in the case of ordinary light two types of scattering, one determined by the normal optical properties of the atoms or molecules, and another representing the effect of their fluctuations from their normal state…[My italics]” Raman thereafter reports that the experiments they have made have confirms this anticipation, and in every case in which light is scattered by the molecules in dust-free liquids or gases, the usual Rayleigh radiation is accompanied by a modified scattered radiation of degraded frequency. Since the effect is observed universally, the phenomenon is independent of the incident frequency of light.

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132 This is the strategy he relied upon in his latter-day researches on crystals and diamonds.
133 This is also evident from Raman’s response to Krishnan upon hearing the announcement of Compton’s Nobel Prize. Raman’s remark in 1927 runs as follows: “…But look here, Krishan. If this is true of X-rays, this must be true of light too. I have always thought so…We must pursue it and we are on the right lines. It must and shall be found. The Nobel Prize must be won” (G.H. Keswani, Raman and His Effect, p.44).
Raman’s trajectory towards this discovery thus consists of several distinct stages: it begins with his grasping of a new field of research in metropolitan science (optical scattering), by providing an explanation for a natural phenomenon, i.e., the color of the blue sea. Raman then gradually becomes aware that the ES equation—his main tool in explaining diffusion phenomena—is beset by several anomalies and will soon require a revision on theoretical grounds. This opens two possible paths of research before him—both of which he records in his monograph: first, the undertaking of an ambitious research program of studying the coefficients of diffraction in liquids, gases and vapors, extending his search finally to the optical and the electric properties of liquids (Raman, *Proc. of the Royal Society*, 1927). But secondly, this also means constructing a new theoretical framework that will explain those observed anomalies, and produce a set of suitable representations about how molecules behave when irradiated with visible light. (As an ambitious, dominant researcher, Raman, of course, thinks himself to be always more than an experimentalist.) Against the prevailing metropolitan consensus of his time, Raman thinks that the quantum theory of light can be such a framework. His next problem lay in getting his evidence accessible to observation, e.g., *devising an appropriate experimental set-up*. Seeking to observe the propagation of light, Raman chooses a scattering situation to be the most ideal condition, and relies upon his trusted, self-taught method of visually (and intuitively) observing the phenomena. This, in turn, was derived from his earlier skill in studying acoustic phenomena. Once the Indian researchers realized that the so-called fluorescence they were looking at was polarized— and thus a case of true scattering—the discovery was within their grasp.

6.4.2. Defending Priority

Raman maintained a high visibility in the European metropolitan community for a period of forty years, by his method of quick reporting of observations and quick communication with his metropolitan colleagues. Thus, he built a robust interface with his metropolitan peers, especially in his researches on light scattering. He was also ready to vigorously defend his own priority to the discovery.

Indeed, in Raman’s case we see two peripheral communities interacting with each other and the European metropolis over the priority of the discovery. Two Russian physicists, Grigory Landsberg and Leonid Madelstam, had also independently observed the same effect in

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134 His choice of liquids as the ideal material to examine the phenomenon of light scattering is clearly derived from his earlier research on the blue sea which had showed him that water has very strong scattering properties, compared to all other substances.
crystals within four months of Raman’s discovery, but they did not publish their results until May 1928, and by that time Raman’s priority had been firmly established, thanks to its confirmation by the German, French and the American colleagues. After the replication and confirmation of his work in the USA and Europe by R.W. Wood, Sommerfeld and Pringsheim respectively (who finally coined the term “Raman Effect”), Raman unanimously clinched his priority to his Effect.\textsuperscript{135} The metropolitan spectroscopists were quick to take up Raman’s work, and indeed Wood soon improved on Raman’s instrumentation, using a helium vacuum tube instead of Raman’s sunlight and mercury arc lamps, gaining in that process a much better spectra. From the hands of the physicists—who used it primarily to demonstrate the vibrations and the rotations of molecules (and thus its architecture)—Raman’s discovery soon passed to the hands of the chemists, who, after 1930 used its powers of analysis to determine the various quantities of (different) substances in their samples. The race towards discovery had indeed paid off this time, and with notes and communications promptly published in Nature as well as in the Indian Journal of Physics and Proceedings of the Royal Society Raman found himself unanimously nominated two years later for the 1930 Nobel Prize in physics.

In his method of communication with his European colleagues (as well as at home), Raman made liberal use of the method of multiple witnessing. He liked to show his results to others, and give spectacular displays on the behavior of light, such as turning on ultra-violet illumination on his research collection of three hundred diamonds. His method of quick institutional communication by means of swapping the issues of the Proceedings of the IACS (later named Indian Journal of Physics) — and still later, the Proceedings of the Indian Academy of Science (Bangalore) — in exchange for the European journals had given his work a high visibility in the West. Thus, the trading zone that Raman built with his European scientific community not only lasted for several decades, towards the end of his researches on light scattering, this zone had become collaborative to a great degree.

6.5. Becoming a Scientist: Acoustic Research at the IACS Laboratory

In considering a peripheral scientist such as Raman, it may be worthwhile to ask how he made his transition from being a colonial subject to a practicing scientist in the first place.

\textsuperscript{135} The cable that R.W. Wood sent to Nature after Raman’s discovery reads as follows: “Prof. Raman’s brilliant and surprising discovery that transparent substances illuminated by very intense monochromatic light scatter radiations of modified wave-length…opens up a wholly new field of molecular structure. I have verified his discovery in every particular…It appears to me that this very beautiful discovery which resulted from Raman’s long and patient study of phenomena of light scattering is one of the most convincing proofs of quantum theory of light which we have at present time (R.S. Krishnan, Raman Effect: Discovery and After, 1978, p.7).”
Why was he motivated to take up an interest in the optical phenomena of light, or earlier, in the tone of the musical instruments? To see the origins of Raman’s interest in science, we have to go back—briefly—to his early interests in acoustics, and see how those researches prepared him for his later, and more well-known, work on the scattering of light.

Raman started his career in science as an amateur. Raman was born in 1888 in the small southern township of Tiruchirapalli and arrived in the city of Calcutta in 1907 to work as an Accounts Officer in the Imperial Financial Services. He came from a Brahmin family that traditionally combined intellectual accomplishments with a Spartan lifestyle. Since a Western-style university system had already been in place in the port cities of India since 1857, Raman’s father made a transition to this system by becoming a teacher in physics and mathematics at the A.V.N. College in Vishakhapatnam. From this college, and later from the Madras Presidency College, Raman obtained a B.A. and an M.A respectively, thus getting his first introduction to Western-style science, and since the Madras Presidency College offered specialization only in two subjects: acoustics and optics (along with some dynamics and heat), these two areas became Raman’s lifelong research interests. It is also during this time that he read Helmholtz’s book *The Sensations of Tone*, which, by his own admission, drew him to scientific research, suggesting a number of problems that he could investigate in the future.

Thus, Raman’s interest in sound and light began with a deep intellectual curiosity reinforced by a fascination with the aesthetics of sound and color phenomena. His family was musically accomplished: his father was a well-known player of the classic Indian string instrument *Veena*, and he had married his wife, Lokasundari, after being impressed with her ability to play the same instrument. Raman himself played violin tolerably well. Thus, not surprisingly, his early researches began with acoustics—with a study of the vibrations in the Indian musical instruments.

Working as an amateur, Raman managed to publish some occasional articles in the *Philosophical Magazine* but he did not yet have a place where he could do his research. But upon his arrival in Calcutta, he spotted the Indian Association for the Cultivation of Science (IACS), and asked its Secretary permission to use its premises for his work. The laboratory of the IACS had been established back in 1876, in the model of the Royal Institution in England, under the initiative of Dr. M.L. Sircar. But apart from hosting some public lectures and some science demonstrations, the IACS did not have any regular researchers on its premises until Raman

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136 His deep fascination with light can be read in the language that he used in a popular lecture to describe the optic properties of opals: “Precious opal exhibits a striking play of colour…some specimens show numerous small glittering of colour, and others almost a continuous sheen of iridescence…” (Raman, 1951, *The New Physics*, p. 141).

137 Founded in 1799, Royal Institution in England is the oldest independent scientific body in the world, which especially devotes itself to dissemination of scientific knowledge among the general population.
arrived at its doorstep. Thus began a happy association that lasted until 1933, when Raman finally left for Bangalore.

Raman’s early researches in the IACS began by studying the acoustic properties of several musical instruments, especially the violin, and Indian percussion musical instruments such as *Tabla* and *Mridangam*. Raman was also a professional photographer, which helped him considerably in his gathering of acoustic data. He obtained data on stringed instruments by measuring their vibrations, and made extensive use of photography in recording how a vibrating system maintains itself through all its stages, given an external source of energy.

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**Fig 6.5.** Raman’s Experimental Arrangements: Vibration Curves. Adapted from Venkataraman, 1988.
For studying percussion instruments, he devised a similar visual protocol, which later helped him in designing his optical experimental set-ups, the objective being to measure the normal modes of the “vibrations of a uniform circular membrane held in tension around its circumference” (Venkataraman, 1988, p. 105). To make visible the evolution of those nodes over time in a percussion instrument, Raman used the simple but the ingenious trick of strewing a little sand on the surface before striking the instrument (Venkataraman, p. 108), exactly the similar trick used before by Michael Faraday (see Gooding, 2006, p.54). Once the sand settled down on the surface, the nodes were clearly revealed, producing a striking picture, which could then be easily photographed.  

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138 Between 1909 and 1919, Raman published nearly 30 papers, using *Philosophical Magazine, Nature* and the *Bulletin of the IACS* as the vehicles of his research. He became known to the international community by means of those publications, and was asked to contribute an article on the topic of musical instruments and their tone, in the impressive German volume *Handbuch der Physik*. He was the sole non-European author in that volume. Giving a quantitative analysis of the three physical properties of musical instruments: pitch, timber and loudness, Raman discussed the physical properties of the violin, percussion instruments, church bells and glass bells in his article, but significantly, omitted to mention his researches on Indian musical instruments, perhaps because of their unfamiliarity to a Western audience.
In his acoustic research, Raman generally sought to understand the nature of the vibrations maintained by the system by using a mechanical-theoretical approach, the data helping him to visualize the changing relationships in the system (Sen, 1988). His analytic approach to sound phenomena was elegant, simple and intuitive; his experimental set-ups were rarely
expensive, but always ingenious. Primarily on the strength of these acoustic researches, Raman was nominated in 1924 as a Fellow of the Royal Society in England, which completed his final transformation from an amateur to a professional.

A decisive moment of transition in Raman’s life came in 1917, when he was invited to take up the chair of the Palit Professorship in Physics in the newly-established University College of Sciences in Calcutta, thus helping to initiate his transition from an amateur to a professional scientist. Like S.N. Bose, he too was handpicked by Ashutosh Mukherjee, the dynamic Vice-Chancellor of the Calcutta University, whose goal was to build a full teaching university at Calcutta. As part of an intense nationalistic movement that was then sweeping India, Calcutta University became the first department in India where the study of science was introduced on a graduate level. In spite of a serious loss in pay, Raman was happy to make the transition, thereby gaining his first formal academic appointment. His work in the IACS continued however, and until the end of his stay in Calcutta in 1933, Raman combined these two places under his leadership, using the IACS as a research arm of the University College of Sciences, Calcutta. His work on the scattering of light that led to the Nobel Prize in 1930, was done from this laboratory, with the assistance of the students of the Association, such as Ramanathan or K.S. Krishnan.

At the IACS, Raman gathered around him a group of young, eager students, who, in turn, assisted him with great ability. As a group leader, Raman emphasized energy, innovation and national pride, urging his students to do novel things, such as teaching themselves German so as to be able to read the research in the German scientific journals—sometimes personally showing them how to do hands-on science on a shoestring budget.  

6.6. Conceptual Changes in a Peripheral Context

The central task in an analysis of science is to focus our attention on the knowledge-constructing processes of the scientists by means of which they create new concepts and new representations. It is with such tools that they understand and communicate about physical phenomena. To understand how scientists develop such tools however, we need to know what Tweney calls the “cognitive pathways”: the mental representations, the artifacts of the

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139 Raman’s student, Pisharoty, thus describes his first encounter with Raman: “He …suggested a scientific problem involving high magnification microscopes and microphotography for my study, and gave [me] a German monograph to get acquainted with the background knowledge necessary for the study. With one piercing look, he found out that I knew no German. He said: ‘Don’t be afraid. This article (100 pages) will not contain more than 500 scientific words; you can translate the whole thing in two days using a dictionary; you will see that half the material consists of equations.’ Thus, in less than half an hour, he had shown me the microscope and how to operate it, what to look for, the literature necessary, and more than all that provided great encouragement.” (Sen, 1988, p. 144)
experimental processes, and the written texts by means of which scientists communicate with one
another. Taken together, those items tell us about the knowledge-constructing practices of the
scientists, and it is with such accounts in hand, that we get a better understanding of how
knowledge in science is threaded to its social and cultural contexts. For peripheral scientists, who
work always at a distance from their metropolitan counterparts, this process is particularly
important to unpack, for their task of joining a research program (and extending it into a suitable
research question) can be particularly challenging. Yet, this is also one of the most productive
phases in peripheral science for it is during such a phase that a peripheral scientist often takes a
new position, or allies himself with an emerging metropolitan program that takes him finally to a
new line of thinking—thereby starting research in a new direction. This is something that we
have observed in Bose as well, who started taking the quantum theory of light seriously well
ahead of his metropolitan peers. Raman, who joined the 19th century experimental tradition in
natural science, following the footsteps of Faraday, Helmholtz and Rayleigh, while using his self-
taught, visual style of experimentation, is another example of the same process. Having first
allied himself with the experimental program of light scattering, Raman gradually turned himself
into an expert in analyzing the behavior of light. When we remember that the colonial Indian
scientists such as Raman or Bose picked up their scientific skills from their own reading of the
research in European journals and from an odd assortment of hands-on experiences, we begin to
see how a study of peripheral science can show us not only how concepts are put together in
science, but also how concepts are learned—thereby giving us a window to the problem of
science learning.

6.7. Raman as a Scientist: The Problem of Intellectual Authority in Peripheral Science

Peripheral scientists participate in science from a distance, and thus naturally, they
enter scientific research guided by the prevailing metropolitan research norms and research
programs. Furthermore, for obtaining a consensus for their work, they remain dependent on their
metropolitan community. During their moments of collaboration with this metropolis, they have
to conform to its microstructure of scientific research, which very often contains a hierarchy.
Factors like this exacerbate the difficulties of making scientific knowledge from peripheral
locations. Yet, as we have just seen, creative extension of concepts into new research programs,
and early responses to new theories also come from the peripheries. Peripheries thus should be
seen as important centers of innovation.
The quick response and confirmation that Raman received from his metropolitan community on his work on the scattering of light, helped to turn his discovery into an accepted part of Western normal science. The award of the Nobel Prize in 1930 and the fellowship of the Royal Society in 1924 added to his scientific stature, making him an established authority in spectroscopy, and generally, in the domain of optics. While describing Raman to Rutherford, Max Born described him as endowed with a “European intensity”. Yet, this Europeanness conferred on Raman could also be quickly withdrawn, once his work failed to gain metropolitan acceptance, thereby severely diminishing the intellectual authority of a peripheral scientist.

The use of visual observation, and reasoning solely on the strength of such observation remained Raman’s signature research style, and, his trained band of spectroscopists at the Indian Institute of Science, Bangalore, continued this tradition until late 1960, eventually becoming the forerunners of fMRI research in India in the late 1980s. In Bangalore, three decades after the discovery of the Raman Effect, Raman’s visual style of doing research precipitated him into a prolonged controversy with the German theoretician Max Born. Their dispute centered on the nature of the phases in the lattice structure in crystals, especially in diamonds, which were then Raman’s main focus of research. Raman’s great faith in his visual data, his insistence that no scientific theory, however mathematical, can claim epistemic primacy over and against such detailed data, his great love for science as an aesthetic exploration of nature, and finally, his confident stance that he had at last become an authority in optics after his Nobel prize, began one of the longest, and the most bitter controversies with the German theoreticians in the history of Indian science. This dispute set the research agenda of his group in Bangalore until the late 60’s. Against the mathematical arguments proposed by Max Born, who argued in favor of a continuous structure in the diamond lattice, Raman’s group continued to urge the primacy of their discrete, visual data, repeatedly arguing that their visual evidence must be accepted as a sufficient reason for the existence of such discrete structures in the diamond lattice (See Sur, 1999). By the time the controversy settled down, Raman had lost the consensus and much of his high visibility in the West. During the last twenty years of his life he published no research in any other journal, except in the *Journal of the Indian Institute of Science*, and during the last ten years of his life, he worked completely alone without any students. It is also during this time that he resigned from his fellowship of the Royal Society.
6.8. Conclusion

The process by which new concepts emerge, change and are transformed—allowing scientists to develop and construct new research programs and begin working in new directions—remains a central concern in explaining science. Conceptual extensions and conceptual changes of this type help us to understand the practices of science and the consequent gains in knowledge. Furthermore, it is during such processes that we observe how the embodied skills of the scientists interact with their interpretative concepts in order to give their practice a concrete shape. While this process has been actively analyzed in the contexts of metropolitan science by a number of philosophers and historians, with regard to peripheral science there exists hardly any literature from which such an account might be obtained. Peripheral science and peripheral scientists are still viewed mainly through the lenses of history, and the science that they produce is rarely taken up by philosophers to illustrate any conceptual processes in science. A consequence of this asymmetry is that we remain generally in the dark about how peripheral scientists construct and extend their research programs, how they undertake their cognitive transformations, and if and how they succeed in this process to endow their home cultures with a new epistemic tradition.

Viewing Raman’s work during the decade of 1921-1930 through the lenses of a CPH analysis, we see that Raman, a self-made, peripheral scientist of dominant personality, responded to the intrinsic aesthetics of sound and color phenomena. Driven by his sense of achieving epistemic independence from the imported knowledge that was part of his colonial condition, he grasped a metropolitan research program, thus giving himself and his group an ambitious research agenda, choosing to ally himself with a new theory of light. It is this theoretical stance that became for him the basis of his extensive researches on light scattering, thus starting a new trade in knowledge and a strong interface between him and his metropolitan community. Behind his experiments lay the conceptual framework that he put together based on his hands-on experience, his contact with his peers in Europe, and his previous research on the musical vibrations of bowed strings. The outcome of this complex process was a salient evidence for the quantum theory of light and a reliable technique for chemical analysis. While personally still based in the traditional wave theory of light, he correctly identified the area where one would observe the next important result, made the required observations, and was able to make a discovery ahead of others. He managed, also, to generate a consensus for his work from his metropolitan peers by means of his method of multiple witnessing and quick communication, and managed to put together a knowledge-network at home that would be capable of making further knowledge in the future. By training groups of new students, who would continue later with similar activities, Raman laid the
basis of a self-replicating professional scientific community in India. Yet, despite his manifest success, his intellectual authority could become quickly eroded through a lack of metropolitan acceptance, and as a result of his controversy with Max Born, he lost the chance of integrating his later researches with the stock of normal science.

Against the default model of peripheral science that sees such science mostly as examples of followers’ science characterized by limited agency and limited creativity, we see in Raman’s researches all the hallmarks of a maverick— the conscious grasping of a research program, the use of different resources to articulate (several) problems, and finally, the development of a visual mode of reasoning, suitably aided by artifacts. Yet, in spite of this creativity, the science that Raman produced from his peripheral locations, first in Calcutta and then in Bangalore, could hardly be called a fully participatory trading zone. In spite of the collaborative nature of its early phases, it still carried a strong element of asymmetry that manifested later clearly in his difficulty in getting a consensus. Peripheral researchers such as Bose or Raman are thus part of a fragile loop in their collaboration with metropolitan science, tracking and (sometimes) solving the problems raised by their metropolitan communities. Yet, the research programs that they propose later in life do not always gain consensus or adherents. Viewed from this angle, what we obtain is a complex picture of peripheral science— while the research in peripheral science displays a high degree of creativity and productivity, the process of validation of their knowledge often shows a one-way dependence, especially in matters of consensus and intellectual authority. Thus, the peripheral science phase in India from 1910-1930 can be interpreted as a complex and difficult practice mostly in the early stages of a SEE network in state 2 (in Raman’s case, sometimes state 3), where such colonial researchers could only display a limited amount of tenacity in developing their more mature research programs. Asymmetries of this type remain a persistent feature in most peripheral situations, and a nascent scientific community, obliged to take its first steps in scientific research under this kind of asymmetry, faces a complicated, uphill task before gaining any stable intellectual authority and establishing themselves in science.
FINAL REFLECTIONS

It is often assumed that while good innovative science takes place at the (different) centers of US or Europe, the so-called peripheries develop only dependent varieties of the major metropolitan paradigms. In his book, *Aborted Discovery*, Susantha Goontilake had indeed argued for such a view. This is an assumption that often stands behind the arguments supporting positions like ethnosciences or alternative science, and contributes greatly to their psychological force. Since non-Western scientific communities can produce only minor themes in science, it is better therefore that such communities shift altogether to some other form of alternative science, which may be organically better suited to their own indigenous traditions. In the two case studies that I have presented in the above, I argue that this is not the case. The two studies show clearly that it is indeed possible for a small and a very peripheral scientific community—such as the physics group in Calcutta—to make significant contributions in scientific knowledge that went on to have a future—first, leading to the development of a new foundational concept such as the Bose-Einstein statistics, and secondly, to a new standardized technique such as the Raman spectroscopy. Thus, there is no reason to think that peripheral scientists will only fill in the roles of minor epistemic players. A peripheral scientist playing the role of an ambitious maverick can certainly achieve new breakthroughs in science and begin a (potentially) successful trading zone with his or her metropolitan community. By incorporating different such trading zones among its different communities, the wider international community of science gradually becomes more diverse, thereby integrating a multiplicity of perspectives within its practice.

And yet, this process is not without its manifest share of difficulties, at least not on the peripheral side. In each case in the above we saw that there was a very complex pattern at work: the paradigms, techniques and practices developed by such scientists do not always gain wide circulation and are often set aside and forgotten. This is especially true about their more ambitious later work which often does not gain wide acceptance in the form of productive research programs. Thus, evidence strongly suggests that peripheral science suffers from some kind of epistemic asymmetry. Whether in the hands of minor or major epistemic players in scientific practice, peripheral science remains, always, a most difficult endeavor. Between the creativity of the individual scientists and the consensus on their fruits of labor, there often lies an enormous yawning gap. Since their ideas do not always gain wide acceptance in the metropolitan centers, those ideas often either die down, or are frequently replaced by something emerging from a metropolis, thus leaving the peripheral scientific communities often with less than significant

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intellectual authority (in spite of their large and crucial contribution). This result shows that the social models of science that intuitively allocate all scientific communities the same (or similar) levels of epistemic authority require some modifications. Differential contribution and differential participation are commonplace in scientific practice, and this is a manifest consequence of the social nature of science. As I have argued above in Chapter 1, not only are such processes operative in the contexts when a new community joins the practice of science from a different social and cultural context, such processes can also become operative within a central community. In the contexts of peripheral science, scientific knowledge remains tightly threaded always to its overall situation: to understand a peripheral scientist, one must understand from which locations such a scientist carries out his or her efforts, what specific conceptual trajectories he/she follows, with whom he/she collaborates, and how those results are received (or rejected) by a metropolis.

The complexity of those processes observed in peripheral science leads us to some interesting speculations about the processes of science in general and how knowledge-making from such peripheral contexts can be grasped in the form of a philosophical account. In the pages above, I have argued for a network model in peripheral science, describing such science in the form of a trading zone between two communities with some restrictive and adversarial properties. The case studies that I used above show how scientific knowledge made within such trading zones can be traded among the metropolitan centers of research and their smaller counterparts at the periphery, and how such interactions can be conceptualized in the form of a general account. In spite of manifest difficulties in forming such a network—and more importantly in sustaining it—the possibility of the development of these zones suggests an open future for peripheral science. To the best of my knowledge, this complex relationship has not been articulated before in the form of a philosophical account. But once conceptualized and articulated, such accounts can help us gaining important insights into how scientific knowledge is produced from such difficult, peripheral sites. The aim of this dissertation is to break new ground in this direction. More specifically, in this dissertation I seek to articulate a conceptual scheme and a theoretical language for exploring peripheral scientific situations by means of which we can understand how the peripheral scientists function as productive cognitive agents. This allows us to understand the peripheral communities (as well as their creativity) especially when such communities are in their formative pioneering stages, and thus grasp how new national traditions in science come to existence.

The scheme of peripheral science that I have sketched above does not claim to be an exhaustive analysis of science outside of its Western homeground. Still, it is not intended to be simply a historical exercise either, but hopefully can also serve as a conceptual basis of some
kind, bearing the weight of some explanatory work in peripheral science. Seen properly, peripheral science is a concept that tells us about the relative distance between the creation of a scientific concept and its reception and validation by another scientific community. This is primarily an epistemic relationship that holds between two knowledge-producing communities, and as such, can show up in other locations under various guises—whether they lie in the metropolitan areas of science or are in those areas which are non-Western in a geographical sense. Not only is it the case that a scientific community with a different ethnic origin or in a distant geographical location may hold a peripheral status, the same might be true about an individual scientist or a small group of scientists who operate with a significantly different research approach from their mainstream members but yet reside in close proximity with them. Or, they may be newcomers in an area of practice that until then had been dominated by a different group of scientists. Peripheries thus can exist outside as well as inside a metropolitan scientific community, and an understanding of how peripheral processes work in science may illuminate the processes of science closer at home.

What I have argued above is that peripheral science presents a very complex structure and it is more than a just a few remote scientists doing science imitatively with their scarce resources. Analyzed properly, it shows us how two scientific communities, who hold differential knowledge resources, still manage to enter into a collaboration with one another—each intent on its goal of making new knowledge—and how the dynamic that comes to exist between them shapes the movement and the circulation of knowledge in general. Since the history of science show us that the centers of scientific knowledge shift over time, it is important to understand the different mechanisms that can drive such shifts. This dissertation shows us a way of conceptualizing such shifting centers over time, showing how a peripheral community can slowly attain the status of a more resource-rich community after having gone through an extended period of collaboration with another community during which time they (gradually) gain the status of being (more) important contributors in scientific knowledge. I have focused in this dissertation only on India as the locus of my case studies, but more location-specific studies of peripheral science can now be undertaken to see if this model holds good in other peripheral locations, or, if, in the light of more information from other sites, it will require substantial modification and revision. In future, I plan to do some of this work myself, hoping that others soon would join me in the same task.
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Appendix 1: S.N. Bose and A. Einstein (1924-1925)

Physics Department
Dacca University
Dated, the 4th June, 1924

Respected Sir,

I have ventured to send you the accompanying article for your perusal and opinion. I am anxious to know what you think of it. You will see that I have tried to derive the coefficient \( \frac{8\pi \frac{v^2}{e^2}}{c^3} \) in Planck’s law independent of the classical electrodynamics, only assuming that the ultimate elementary regions in the Phase space have the content \( h^3 \). I do not know sufficient German to translate the paper. If you think the paper worth publication, I shall be grateful if you arrange for its publication in Zeitschrift für Physik. Though a complete stranger to you, I do not feel any hesitation in making such a request. Because we are all your pupils though profiting only by your teaching through your writings. I do not know whether you still remember that somebody from Calcutta asked your permission to translate your papers on Relativity in English. You acceded to the request. The book has since been published. I was the one who translated your paper on Generalised Relativity.

Yours faithfully,
S.N. Bose

Physics Laboratory
Dacca University
Dacca, India
15th June, 1924

Respected Master,

I send herewith another paper of mine for your kind perusal and opinion. I hope my first paper has reached your hands. The result to which I have arrived seems rather important (to me at any rate). You will see that I have dealt with the problem of thermal equilibrium between Radiation and Matter in a different way, and have arrived at a different law for the probability for elementary processes, which seems to have simplicity in its favour. I have ventured to send you the type-written paper in English. It being beyond me to express myself in German (which will be intelligible to you), I shall be glad if it publication in Zeitschrift für Physik or any other German journal can be managed. I myself know not how to manage it. In any case, I shall be grateful if you express your opinion on the papers and send it to me at the above address.

Yours truly,
S.N. Bose
17 Rue de Sommerard
Paris
26-10-1924

Dear Master,

My heartfelt gratitude for taking the trouble of translating the paper yourself and publishing it. I just saw it in print before I left India. I have also sent you about the middle of June a second paper entitled “Thermal Equilibrium in the Radiation Field in the Presence of Matter.”

I am rather anxious to know your opinion about it, as I think it to be rather important. I don’t know whether it will be possible also to have this paper published in Zeitschrift für Physik.

I have been granted leave by my university for 2 years. I have arrived just a week ago in Paris. I don’t know whether it will be possible for me to work under you in Germany. I shall be glad, however, if you will grant me the permission to work under you, for it will mean for me the realization of a long-cherished hope.

I shall wait for your decision as well as your opinion of my second paper here in Paris. If the second paper has not reached you by any chance, please let me know. I shall send you the copy I have with me.

With respects,

Yours sincerely,

S.N. Bose

Dr. S. Bose
Berlin W. 30
17, Rue De Sommerard
Paris

3 November 1924

Dear Colleague:

Thank you sincerely for your letter of 26th October. I am glad that I shall have the opportunity soon of making your personal acquaintance. Your papers have already appeared sometime ago. Unfortunately the reprints have been sent to me instead of you. You may have them at any time. I am not in agreement with your basic principle concerning the probability of interaction between radiation and matter and have given the reason which has appeared together with your paper. Your principle is not compatible with the following two conditions:

1) The absorption coefficient is independent of the radiation density.
2) The behaviour of a resonator in a radiation field should follow from the statistical laws as a limiting case.

We may discuss this together in detail when you come here.

With Kind regards,

Yours

A. Einstein
Respected Master,

I received your kind note of 3rd November in which you mentioned your objections against the elementary law of probability. I have been thinking about your objections all along and so did not answer immediately. It seems to me there is a way out of this difficulty, and I have written down my ideas in the form of a paper which I send under a separate cover. It seems that the hypothesis of negative Einstrahlung stands, which, as you have yourself expressed, reflects the classical behavior of a resonator in a fluctuating field. But the additional idea of a spontaneous change, independent of the state of the field, seems to me not necessary. I have tried to look at the radiation field from a new standpoint and have sought to separate the propagation of Quantum of energy from the propagation of electro-magnetic influence. I seem to feel vaguely that some such separation is necessary if Quantum theory is to be brought in line with Generalised Relativity theory.

The views about the radiation-field, which I have ventured to put forward, seem to be very much like what Bohr has recently expressed in May Phil. Mag 1924. But it is only a guess, as I cannot say honestly to have exactly understood all he means to say about virtual fields and virtual oscillators.

I am rather anxious to know your opinion about it. I have shown it to Professor Langevin here and he seems to think it interesting and worth publishing.

I cannot exactly express how grateful I feel for your encouragement and the interest you have taken in my papers. Your first post card came at a critical moment and it has more than any other made this sojourn to Europe possible for me. I am thinking of going to Berlin at the end of this winter, where I hope to have your inestimable help and guidance.

Yours sincerely,
S.N. Bose