LOCAL HIERARCHIES AND INTERNATIONAL NETWORKS : MOLECULAR BIOLOGY AT THE TATA INSTITUTE OF FUNDAMENTAL RESEARCH, 1962-1980

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Introduction

Science has been an integral part of the self-description of modern India. It has been argued from the pre-independence era onwards that science holds the key to progress. It is not surprising then that the years that followed Indian independence in 1947 witnessed an extraordinary proliferation of science institutions in India. Within the current critical literature on the sociology of modern science in India, the history of science institutions has mainly been analysed in terms of underdevelopment[1]. This paper, however, focuses on the beginnings of molecular biology at the Tata Institute of Fundamental Research, Bombay (henceforth, TIFR) and argues that underdevelopment alone is not always an adequate category with which to understand the effort to establish a 'new' scientific discipline in Third World countries like India. This paper also attempts to take such questions of science and underdevelopment into a hitherto unexplored direction, by asking what was the relationship between science institutions in the developed countries and science institutions in the developing world. How far were international networks of knowledge responsible for building and supporting a discipline like molecular biology ? While there have been attempts to trace the growth of biophysics, biochemistry, microbiology and molecular biology in India through personal reminiscences of the key figures involved, there has been no attempt to map the evolution of the molecular biology unit at TIFR and its contributions to the growth of modern biology in India[2]. This paper therefore, attempts to present a neglected episode in the development of molecular biology in India. More specifically, this paper contends that the maintenance of international contacts and the creation of networks were part of the institutional practices of TIFR, and these practices initiated not only the beginnings of the group itself, but also sustained the discipline and steered it towards new research directions. This paper, there-

1. Dhirendra Sharma, " How Indian atomic energy policy thwarted indigenous scientific development ", in Ziauddin Sardar (ed. by), *The revenge of Athena : science, exploitation and the Third World*, London, Mansell Publishing House, 1988, 73-80.

2. D.P. Burma (ed.), *History of development of biochemistry and molecular biology in India : some personal perspectives*, Delhi, Indian National Science Academy, 1996.

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fore, attempts to situate the development of the molecular biology group at TIFR within a larger matrix where the local and the global encounter one another.

World War II, Indian independence and science in India

The period between the Second World War and Indian independence saw a shift in emphasis in the problems of Indian science. Between November 1943 and April 1944, the Nobel Laureate and biophysicist, Sir A.V. Hill, scientific advisor to the British Government in India visited the country to advise the Government about post-war reconstruction and the reorganization of scientific and industrial research. While Hill's arguments for the centralization of scientific research and his clear differentiation between pure research and development and its impact on the organizational structure scientific research has been noted by historians, there has been very little scholarship that has focused on other aspects of the Hill report that had important consequences for science institutions in India[3]. After mentioning the Tatas as an industrial family that had devoted substantial resources for the development of science and medicine in India, Hill went on to cite examples of the American benefactors such as Rockefeller, Guggenheim, Harkness, Carnegie, G.F. Barker, and concluded that science institutions in India offered " great opportunity for the Indian benefactor "[4]. In this context, Hill's wholehearted support for the scheme presented to him by his friend, Homi Bhabha, the cosmic ray physicist, of beginning TIFR with private philanthropy, is therefore, understandable[5].

Founded in 1945 in post-war Bombay, TIFR reflected the buoyant optimism of the years that preceded Indian independence. At TIFR, Bhabha put into operation his idea of 'growing science' – an organic metaphor that was to remain part of his philosophy[6]. However, in post-war India, the growth of science was dependent on several factors : the availability of funds for setting up research programmes, the availability of talent, and on the creation of a suitable atmosphere of intellectual exchange. Unsurprisingly therefore, the success of Bhabha's scheme also depended on the return to India of young scientists who had been trained abroad.

International contacts and the beginnings of molecular biology at TIFR

As George Greenstein, has perceptively pointed out, Bhabha had an international outlook and " people he regarded as his equals were not in India, but in the

3. R.S. Anderson, *Building Scientific Institutions*, Montreal, McGill University Press, 1975, 32-33 ; Shiv Visvanathan, *Organizing for science : the making of an industrial research laboratory*, Delhi, Oxford University Press, 1985, 121-132.

4. A.V. Hill, *A Report to the government of India on scientific research in India*, London, The Royal Society, 1944, 55.

5. A.V. Hill to Homi Bhabha, 22 June 1944, TIFR-ARCH/ D-2004-00345.

6. Homi Bhabha, Presidential address to the National Institute of Sciences in India (now INSA), 1963, cited in B.M. Udgaonkar, " Bhabha on growing science ", *Homi Jehangir Bhabha collected scientific papers*, Bombay, TIFR, 1985, LVI.

West "[7]. It was hardly unusual, therefore, that Bhabha should place such an immense confidence on an international network of scientists he had met during his stay in Europe ; he relied on them for their support and on their recommendations. An analysis of the role of this international network in building the physics and mathematics groups is beyond the scope of this paper. However, the process of creating of a group to work on molecular biology at the Institute reveals the working out of a similar dynamic.

Responding to Bhabha's scheme for a new Institute, Hill suggested that he incorporate biophysics as a discipline within the newly founded Institute : " I think you had better take BioPhysics under its wing, too ! Apart from the Bose Institute at Calcutta, that subject does not practically exist in India. I am sure that many of the most important future applications of Physics will be in Biology "[8].

It is interesting to note that despite this suggestion, the Institute had no place for the life sciences until 1962. The existing groups working on biophysics, biochemistry or microbiology within India did not seem to capture Bhabha's attention. As it is clear from one of the objectives he had spelt out at his address to the Indian National Institute of Sciences in 1963, his idea of 'growing science' was also designed to provide opportunities to Indian scientists returning from abroad[9]. Later, in 1966, Bhabha reflected that he had wanted to wait for a suitable individual to lead a molecular biology programme at his Institute and it was the Hungarian physicist turned biophysicist and peace activist, Leo Szilard who had drawn his attention to Obaid Siddiqi[10]. Bhabha had met Szilard during his years at Cambridge and the warmth of their relationship remained even though the correspondence suggests that they had lost contact for a while. Siddiqi, a molecular biologist trained in Aligarh and Glasgow, had met Szilard at Cold Spring Harbor in 1961. Since Siddiqi had no job in India to return to, his friends Maury Fox (MIT, USA) and Maury's sister Evelyn Fox (later, Evelyn Fox-Keller, historian and philosopher of modern biology, presently at MIT, USA) had suggested to Szilard that he write to Bhabha about Siddiqi[11]. Szilard who had known Bhabha in Cambridge had then written to Bhabha in 1962, enclosing two letters from Guido Pontecorvo (the Italian-born geneticist, with whom Siddiqi had done his PhD at Glasgow) and Alan Garen (with whom Siddiqi had done postdoctoral work at the University of Pennsylvania, USA). The two enclosed letters described Siddiqi's

7. G. Greenstein, " A Gentleman of the old School : Homi Bhabha and the Development of Science in India ", *American Scholar*, 61, 1 (1992), 415.

8. A.V. Hill to Homi Bhabha, 22 June 1944, TIFRARCH/ D-2004-00345.

9. Bhabha, Homi, " Science and the Problems of Development ", Address to the International Council of Scientific Unions, Bombay, 7 January 1966, reprinted in *Science*, 151 (1966), No. 3710, 542.

10. *Ibid*, 544.

11. Interview with biologist, Obaid Siddiqi, 16 January 2003, TIFR-ARCH/OH/MolBio/OS. Unless other wise stated, all oral history interviews have been conducted by Indira Chowdhury on behalf of the TIFR Archives.

work and recommended him ; Szilard had concluded his own letter to Bhabha asking him to take action " which was appropriate in the circumstances "[12].

Institutional identity and local hierarchies

The decision to begin molecular biology was however, initially fraught with a certain degree of institutional resistance. At first, apart from the Deputy Director, MGK Menon, Bhabha's proposal found very few supporters within the Faculty[13]. Questions were raised about laboratory space, lack of library facilities for the subject, lack of research students, and other infrastructural constraints. Most importantly, the members interrogated the compatibility of the discipline itself with the ongoing work at the Institute.

In keeping with its identity as the Advanced Centre for Nuclear Physics and Higher Mathematics, TIFR was primarily a physics institute that had begun with research into elementary particles and cosmic rays, and from the early 1950s, with nuclear physics. Even within the experimental physics groups, cosmic ray physics occupied a prime position at the Institute[14]. Moreover, as my interviews with experimental physicists reveal, very few at that point had appreciated the potential of molecular biology as a discipline[15]. What enabled the establishment of molecular biology despite the reservations of physicists was the uncomplicated access that the Institute enjoyed in the early years to the Atomic Energy Establishment Trombay (AEET, later renamed the Department of Atomic Energy, DAE). This was not surprising because the two establishments not only had a common founder in Homi Bhabha but also had common faculty. It is therefore, hardly surprising that the faculty should recommend that Obaid Siddiqi, the molecular biologist in question could work at the Atomic Energy Establishment where Gopal Ayyangar already had a Biology Unit[16]. However, Bhabha seemed quite clear about the location of the new discipline within TIFR rather than within AEET. In a letter communicating his decision to appoint Siddiqi in July 1962, Bhabha informed Siddiqi that not only would he prefer that Siddiqi joined the Institute (rather than the AEET), but also he would be provided with " all necessary financial assistance " to start building from scratch, a group of workers as well as equipment[17].

12. Leo Szilard to Homi Bhabha, 3 March 1962, TIFR Archives, D-2004-00596.

13. Interview with secretary Physics Faculty, J.V. Kotwal, 8 December 2003, TIFR-ARCH/OH/Admin/JVK.

14. B.V. Thosar, *Growing up with science in India*, Bombay, Nita Printers, n.d., 119.

15. Interview with cosmic ray physicist and former Dean of Physics, Ranjan Roy Daniel, 20 July 2004, TIFR-ARCH/OH/Phy/RRD.

16. 62nd meeting of the physics faculty held in May 1962, molecular biology, TIFR Archives, D-2004-00596.

17. Homi Bhabha to Obaid Siddiqi, 9 July 1962, TIFR-ARCH/OH/OS/Supplement.

Siddiqi, who was at that point working on nonsense mutations with Alan Garen, strongly felt that rather than traditional departments of botany or zoology, laboratories of physical sciences would provide facilities as well as the intellectual environment for developing molecular biology[18]. Siddiqi's earlier experience of working at Aligarh Muslim University in the 1950s where the Board of Studies in Botany was reluctant to introduce modern genetics as a paper at the master's level, characterized the inertia of university departments towards restructuring curriculum[19].

At TIFR, however, the institutional mechanisms for supporting interdisciplinary research were still to be created. Therefore, the practical difficulties of working in a physics institution soon became clear : Siddiqi found that there were 'no fumehoods or real facilities for doing chemistry work or microbiology or chemistry… There was also no pipe for running water in the lab'[20].

Before the space could be allotted and the laboratory made ready, Siddiqi began work with his sole student, B.N. Apte, who joined him in 1963. They worked in the laboratory of Gopal Ayyangar of the Biology Division of the Atomic Energy Establishment, attempting to make medium to grow *Aspergillus nidulans*[21]. The work at Gopal Ayyangar's lab continued for some months. Finally, one room on the first floor of 'B' wing became available, however, not without some misgivings of the plasma physics group, which was displaced[22].

Compared to the allocation of space, the allocation of funds seemed more flexible. Although by the 1960s the DAE provided a substantial budget to the Institute, Bhabha's control over budgetary protocols, as well as the principle of cooperative individualism that allowed groups to 'share' unused funds, were both equally responsible for the quick reallocation of funds for this new group[23]. The Annual budget of the Institute which totalled Rs 80,000 when it was founded in 1945, had by 1966 increased to Rs 1.5 crores. More importantly, the Institute had reserved for itself the freedom to define its own research areas. This was a rare, privileged position in India and it is hardly surprising that the Indian journal *Science and Culture* should have perceived the near simultaneous beginnings of molecular biology and radio astronomy at the Institute as being incongruent : " Since the area of work has not been anywhere defined, we find amongst the latest departments opened is a microbiology wing and another on radioastronomy – two not very congruent subjects to be taken up "[24].

18. Siddiqi to Bhabha, TIFR-ARCH/ D-2004-01230.

19. Interview with biologist, Obaid Siddiqi, 16 January 2003, TIFR-ARCH/OH/MolBio/OS.

20. *Ibid.*

21. Interview with biologist and genetics consultant, B.N. Apte, 15 March 2004, TIFR-ARCH/OH/MolBio/BNA.

22. *Ibid.*

23. Interview with biologist, Obaid Siddiqi, 16 January 2003, TIFR-ARCH/OH/MolBio/OS.

24. Editorial Comment, *Science and Culture*, 3, 7 (1966) 331.

Even though the swift allocation of funds enabled a beginning for molecular biology, the lack of foreign exchange in the 1960s became a serious hindrance to the setting up of new scientific infrastructure. These constraints were overcome when the Wellcome Trust made available a grant of £ 40,000 in 1963. However, when the group applied for a grant to the Wellcome a second time, in 1967, the Trust requested not only a statement on the up-to-date position of the group and current plans but also asked the cosmic ray physicist, MGK Menon, who succeeded Bhabha as the second director of the Institute, to add a brief statement of the underlying philosophy of the institute[25]. The note prepared by Menon entitled " Brief Statement of the Philosophy underlying the Research Activities at the Tata Institute of Fundamental research, in particular the molecular biology Programme ", reflected on Institute's role in the building of scientific capacities of the nation as a whole[26]. The Wellcome Trust also queried the proportional allocation of Institute funds to the group, including foreign exchange before making available a capital grant for the purchase of equipment in 1969[27].

Initially, the poor visibility of the group within the Institute was accounted for by the smallness of its size[28]. In 1963, P.K. Maitra , a PhD in biochemistry from the University of Calcutta, joined the group, after doing post-doctoral work with Britton Chance at the Johnson Foundation in Philadelphia. But by 1968, the group had grown to include seven academic members, of them three left after spending a couple of years. They were : G.K. Notani (from Rockefeller Institute where he had worked with Norton Zender), Siddharth Sarkar (who had earlier worked at MIT with Salvador Luria, the Italian biologist who won the Nobel prize in 1969), and Anil Sadgopal (who had worked with James Bonner at Caltech and left to begin a science movement in schools in India). Among the rest, except of R.N. Singh who came from the Radio Chemistry Division of the Atomic Energy Establishment and U.W. Kenkare, who joined in 1967 from the Veterinary Research Institute, Lucknow, each one of them joined after doing post-doctoral research in the USA. Furthermore, each continued working in the area they had previously worked on. Maitra continued work on the glycolytic pathway using yeast as his model system and went on to discover and describe five enzymes responsible for the metabolism of sugar[29]. M.M. Johri, who joined in 1968, with a PhD from Delhi University and post doctoral experience from the Plant Research Laboratory, Michigan, continued to work on plant enzymes, mainly using *Funaria hygrometrica* as his model system. Few among them went on to collaborate with

25. Sir Michael Perrin to MGK Menon, dated 12 October 1966, TIFR-ARCH/ D-2004-01230, p. 2.

26. *Ibid.*, p. 4.

27. Dr P.O. Williams to MGK Menon, dated 14 June 1968 and Menon's response dated 12 March 1969. D-2004-01230.

28. Interview with physicist and science education expert, B.M. Udgaonkar, 1 November 2003, TIFR-ARCH/OH/Phy/BMU.

29. P.K. Maitra and Z. Lobo, " Control of glycolytic enzyme synthesis in yeast by products of the hexokinase reaction ", *Journal of Biological Chemistry*, 246 (1971). P.K. Maitra and Z. Lobo, " A kinetic study of glycolytic enzyme synthesis in yeast ", *Journal of Biological Chemistry*, 246 (1971a).

other groups within the Institute, and among them was U.W. Kenkare who worked on brain hexokinase, the earliest to collaborate with the NMR group in the Institute for analysing the catalytic reaction of hexokinase. However, despite the impressive diversity of work, the molecular biology group was slow to gain a significant presence within the Institute. Looking back at his years at the Institute, Kenkare felt that the prevalent hierarchy in the sciences which was dominated by physics that shaped perceptions in other groups about the molecular biology group at that time[30].

The predominance of an existing hierarchy within the Institute that made intra-institutional collaboration difficult was, however, not the only reason for the lack of interdisciplinary communication. There was perhaps an epistemological gap between the disciplines similar to what Karin Knorr Cetina, has characterised in another context, as the difference in semiotic culture between high energy physics and molecular biology[31]. Maitra categorized the difference between the physical sciences and the life sciences at a more abstract level, as being the difference between speculative and abstract thinking on the one hand, and the use of simple, unexciting techniques and the complexities of analysis on the other : " When we used to talk to [the computer group] about genetics and so on... they were very surprised to see that such a simple technique could take me to the limit of analysis in some sense "[32].

The computer group, on the other hand, saw this barrier in interaction and exchange as being shaped by the difference between representational technologies and experiential knowledge[33]. The difference between two coexisting epistemic cultures, where each culture exercised a different set of criteria for identifying the meaning of the empirical, as well as molecular biology's preference for experiential knowledge over representational technologies, contributed significantly to the perceived hierarchy between the sciences at the Institute.

International networks and growth of the group

Within the larger ecology of knowledge systems that coexisted within the Department of Atomic Energy, the molecular biology group was acknowledged as an academic biology group that occupied the prestigious area of pure research. This identity was further reinforced when the group started organizing training programmes where members taught alongside distinguished international scientists. In 1967, the winter school in molecular biology was taught by Martin Gellert (National Institutes of Health, USA), George Streisinger (University of

30. Interview with biologist, U.W. Kenkare, 11 October 2004, TIFR-ARCH/OH/MolBio/UWK.
31. Interview with biologist, P.K. Maitra, 27 May 2003,TIFR-ARCH/OH/MolBio/PKM.
32. *Ibid.*
33. Interview with computer scientist, S. Ramani, 7 June 2005, TIFRARCH/OH/CompSc/SR.

Oregon, USA), Maury Fox (MIT, USA), Obaid Siddiqi (TIFR, Mumbai, India) and T. Ramakrishnan (IISc, Bangalore).

The 1969 course in Microbial Physiology and Genetics in molecular biology was taught by Adam Kepes (International Cell Research Organization, UNESCO, Paris), Robert Pritchard (Department of Genetics, University of Leicester, UK), Franklin Stahl (University of Oregon, USA), N.K. Notani (Bhabha Atomic Research Centre, Mumbai, India), and Obaid Siddiqi (TIFR, Mumbai, India). The course on microbial genetics in particular, proved a turning point for a young physicist at TIFR, P. Babu. Babu was trained at the AEET Training School and had a PhD in particle physics ; after the course, he found himself attracted enough to biology to switch over completely to this new discipline[34].

As the group grew, international networks became further strengthened as is evidenced by the list of visiting scientists in the 1970s which included Max Delbrück (California Institute of Technology, USA), Severo Ochoa (Hoffman La Roche Institute, USA), and Hildegard Lamfrom (formerly of Medical Research Council – MRC – Laboratory Cambridge, who was at that point visiting Ahmedabad, India) and Gerald Fink (Cornell University, USA), to name a few[35]. Such networks enabled the molecular group to align itself with institutional practices at the parent institution, i.e., at TIFR. The training programmes or lecture series that most visiting scientists gave, helped in training the students and laboratory assistants in the group[36]. More importantly, such programmes and lectures ensured that students who had joined the group from the traditional university system received adequate orientation. Furthermore, at this stage, university reforms in the life sciences were yet to take place with only a few active groups working in biophysics or biochemistry. By ensuring that students, either from the group or from outside institutions were the core recipients of such training programmes, the molecular biology group created a mechanism for identifying new students. This was especially important given that TIFR had no other access to graduate students as the university departments. The training network contributed to orientation of graduate students to this 'new' field. At the same time, this also created the space for interdisciplinary interaction within the Institute through the participation of Physicists like P. Babu.

The creation and utilization of international networks for training was a practice that was consistent with the institutional norms of TIFR. Since its inception in 1945, Homi Bhabha had been inviting international scientists to give seminar courses at the Institute. Students at the institute would be assigned the task of taking notes at these lectures that were subsequently published as 'Lecture Notes'. This practice was, as several scientists who worked in the Institute during that period have observed, a unique method of training young students, enabling

34. Interview with P. Babu, 17 May 2003, TIFR-ARCH/OH/MolBio/PB.

35. *Report : Molecular biology Unit, 1974-1978*, Bombay, TIFR, 1978, 113-18.

36. Interview with Laboratory Assistant, Biological Sciences, P.N. Bhavsar, 9 January 2004, TIFR-ARCH/OH/MolBio/PNB.

them to focus and discipline their minds on how to think about research problems. By taking up the same practice and adapting it slightly to include experimental techniques and laboratory-based learning, the molecular biology group was pursuing an already established institutional custom that further facilitated and fostered its integration into the Institute.

Moreover, it was through the courses taught by international scientists, that molecular biology at the Institute, shaped an interface that contrasted sharply with the other kinds of international networks that were prevalent in India at that time, where discussions and interactions were more Faculty-oriented. An international network of scientists participated in major conferences that were organized in India in the 1960s. Severo Ochoa and Linus Pauling among others came to 'An International Symposium on Protein Structure and Crystallography' organized in Madras, by G.N. Ramachandran in January 1963[37]. Francis Crick and Seymour Benzer came to the 'Symposium on Nucleic Acids', organized by Pushpa Bhargava in 1964 in Hyderabad[38]. Such conferences while enhancing student exposure were not usually conducive to generating an atmosphere of learning as the courses designed by the molecular biology group were. Moreover, most universities in India persisted with the traditional model of teaching Botany, Zoology or Physiology Honours at the undergraduate and postgraduate levels ; such courses oriented students to the 'new' field of molecular biology. Given the fact, TIFR had to depend mainly on the unreformed university system to find PhD students, such international training networks also created a space for interacting with bright and talented students, and put in place a mechanism for identifying some of them as potential researchers within the group.

Disciplinary freedom and international networks

The self-perception of the molecular biology group and indeed of other groups at the Institute was that they had the freedom to define their own research agendas. This is hardly surprising within an establishment that was abundantly financed by the DAE. However, I would like to raise here a question about the disciplinary consequences of academic freedom. One of outcomes of the freedom that prevailed within the TIFR group was the proliferation of a variety of model organisms that the group worked on in the early years. Broadly though, the focus remained on molecular genetics, macromolecular synthesis and metabolic regulation. Reflecting on the reasons for such a choice, the first decadal report in 1963 said : " To some extent our choices were inevitably determined by the interests of the first members of the group. That, however, is only a part of the truth "[39].

37. G.N. Ramachandran, " An International Symposium on Protein Structure and Crystallography ", *Current Science*, 20 (1963), 126-130.

38. The Francis Crick papers online, National Medical Library, accessed on 10 July 2005 at http ://profiles.nlm.nih.gov/SC/Views/Exhibit/other/visuals.html.

39. *Report of the molecular biology Unit : 1963-1974*, Report, TIFR-ARCH/OH/PNBhav/ Supplement/p. 4.

The justification for choosing molecular genetics seems to have been the 'major advances made in the field in recent years' within the larger international milieu of the discipline. In justifying the group's choice of research areas, the report alludes to several rapid developments in the field. This included the deciphering of the genetic code between 1961 and 1966, by two separate teams : Nirenberg and Matthaei, and Ochoa and his co-workers ; as well as Khorana's chemical method for synthesizing polynucleotides with a given sequence ; and finally, the production of the first recombinant DNA molecule by Berg and Boyer in 1973. The report stated that it was " evident that the concepts developed in these areas will provide a firm basis on which future advances in biology must rest. It seemed natural at that point for the group to choose these as core areas around which to build a long-term programme "[40]. Barely five years later, a very different emphasis emerged marking a shift in research direction from molecular biology to neurobiology.

In 1978, the molecular biology unit, as it had come to be called, brought out a discussion report entitled *Where is the bottle-neck ?,* that included Siddiqi's presentation " Molecular Genetics to Neurobiology : Past, Present and Future " dividing his research into two phases – an earlier phase when he was working on molecular genetics and a later phase from 1973-1978 when he had switched more or less entirely to *Drosophila*[41]. Categorizing the earlier phase of his research as belonging to what Gunther Stent had called the 'romantic phase' of molecular biology[42], he maintained that with solution of the genetic code and an overall understanding of gene action, this paradigm no longer served its purpose. His interests had moved to neurobiology because he became interested in extending genetic analysis to the nervous system. His research questions had therefore shifted on multiple planes : from gene action to questions of behaviour and development, and from microbes to multicellular organisms, more specifically, to the fruit fly *Drosophila melanogaster.* Further, he urged the group to develop a focus around the fruit fly[43].

Undeniably, the world over, the humble fly was re-entering biology labs[44]. This shift within the TIFR group was in tune with changes that were taking place within the international arena. At the MRC laboratory, in Cambridge, Sydney Brenner had articulated the need to move to a new and previously uncharted area in his letter to Max Perutz[45]. Around the same time, at CalTech, Seymour Benzer

40. *Ibid.,* p. 4.

41. *Where is the bottle neck ?* Report, TIFR-ARCH/OH/PNBhav/Supplement/p. 1.

42. J. Cairns *et al* (eds.), *Phage and the origins of molecular biology,* Cold Spring Harbor, N.Y., Cold Spring Harbor Laboratory of Quantitative Biology, 1966.

43. *Where is the bottle neck ?, op. cit.,* 4.

44. M. Brookes, *Fly. The unsung hero of twentieth century science,* London, Phoenix, 2002.

45. Letter from Sydney Brenner to Max Perutz, dated 5 June 1963, http ://elegans.swmed.edu/Sydney.html#LETTER ; Accessed on 6 July 2005.

too spoke of the need to turn towards other problems in biology because of the congestion within molecular biology programmes[46].

Siddiqi's desire to train in neurobiology was therefore, driven by the need to seek out new areas in biology, which in turn, reflected his alignment with a larger international network. At the same time, it also revealed his understanding of local conditions where long-term research goals were necessary for survival. The change in research direction within the group was, therefore, a consequence of the larger international networks that the group had put in place. Siddiqi spent time in Benzer's lab at CalTech where he and Benzer described a set of temperature-sensitive paralytic mutants of *Drosophila* and showed that some of the paralytic mutants exhibit neurophysiological defects in the electrical activity of their nerves and muscles[47]. This discovery has led to the identification of several genes that control nerve conduction and synaptic transmission.

In 1973 Siddiqi applied to the Nuffield Foundation for a grant to start a programme of research in neurobiology. The Nuffield Foundation Grant application proposed " studies of genetic and neurophysiologic studies of behavioural mutants of *Drosophila* and an exploratory survey of locally available invertebrates to assess their suitability as model systems for neurobiological research "[48]. The grant application for *Drosophila* neurobiology was to prove successful. A Winter School in Neurobiology and Developmental Biology – taught by Sydney Brenner (MRC, Cambridge, UK), Graham Mitchison (MRC, Cambridge, UK), John Nicholls (University of Stanford, USA) and Alan Pearlman (Univeristy of Washington, USA) followed in 1974. Brenner's presence once again served to reinforce the idea of developing a strong concentration around *Drosophila*.

Four years later, Siddiqi placed a strategic choice before his group : since there were more chances of breakthroughs in long-term research projects – a prospect that was particularly important for a group working in India if it wanted to compete internationally. However, the lack of collaborative convergence between individual projects proved to be a contentious issue and the discussion at the meeting remained inconclusive[49].

One of the reasons why consensus failed to materialize within the group at that point was the pressures of competing research programmes. M.R. Das who had joined the group in 1971 and worked on tumour virology also submitted a proposal to the Nuffield Foundation to fund his programme in 1973 ; the application was to prove unsuccessful[50]. Holding a view that was in conflict with the

46. Seymour Benzer, Interview with Heidi Aspaturian, Pasadena, California, September 11 1990-February 1991, retrieved 7 July 2005, from www/resolver.Caltech.edu/CaltechOH :OH_Benzer_S.

47. O. Siddiqi and S. Benzer, " Neurophysiological defects in temperature-sensitive paralytic mutants of *Drosophila melanogaster* ", *Proceedings of the National Academy of Sciences USA*, 73 (1976), 3253-3257.

48. O. Siddiqi, 'Genetic Approach to Neurophysiological Basis of Behaviour', grant proposal included with letter of MGK Menon dated 5 February 1973. D-2004-01230.

49. Interview with biologist, Obaid Siddiqi, 6 February 2003, TIFR-ARCH/OH/MolBio/OS.

50. TIFR-ARCH/ D-2004-01235.

research priorities of the group, Das felt that the Institute should create a separate budgetary head for 'tumour virology' since international agencies were no longer interested in funding to cancer research[51]. Similarly, work on development using zebra fish as a model system did not receive the expected fund allocation[52]. Both members left the group in the late 1970s contributing to the depletion in variety. Despite the exit of two members, the work on glycolysis, on brain hexokinase and *fungaria* continued. With the discovery of DNA sequencing, microbial genetics got a fresh lease of life and the idea of building a single focus was revised slightly to include a second group working on yeast genetics headed by P.K. Maitra. Maitra's group, however, despite the breakthroughs the teamwork of Maitra and Zita Lobo had achieved, did not become a pervasive presence within the Unit as the number of researchers in the group remained small. After Maitra's retirement and later Zita Lobo's death in 2000, work on yeast came to a standstill.

By contrast, the success of the *Drosophila* group was based not only on the international support it received. The group focused on building new capacities and reoriented older recruits. Among the Faculty, P. Babu who had worked with Brenner at the MRC Laboratory, Cambridge, using *Caenorhabditis elegans*, had by the mid-1970s turned his attention to the genetics of *Drosophila* persuaded by Brenner[53]. R.N. Singh, who had earlier worked with *Bacillus subtilis*, soon reoriented his skills and trained in electron microscopy at Sydney Brenner's laboratory at MRC, Cambridge, working with John Sulston[54]. By the 1970s, Singh had turned to electronic microscopy studies of nerve connection in wild type *Drosophila* as well as analysis of neural projection using cobalt diffusion through nerve fibres. Soon the techniques of electron microscopy became well established within the group. The effort to train and orient scientists was not confined to its own members. In 1981, the group once again deployed its international contacts to organize a course on light and electron microscopy that was taught by José Campos-Ortega and Nicholas Strausfeld (then based at the European Molecular Biology Laboratory, Heidelberg).

In addition, new members who joined the group in the 1970s worked on genetic analysis of the chemosensory pathways as well as musculature and development in *Drosophila.* One of the major breakthroughs came in 1978 when Siddiqi and his team of researchers isolated one of the genes for olfaction[55]. The following year, in December 1979, the group organized an international conference on development and behaviour of *Drosophila* in Bombay that brought together an

51. TIFR-ARCH/ D-2004-01232.

52. Personal interview with U.N. Singh, Delhi April 2004 by Indira Chowdhury.

53. P. Babu, " Early developmental subdivisions of the wing disk in *Drosophila* ", *Molecular and General Genetics*, 151 (1977). P. Babu and S. Brenner, " Spectrum of ^{32} P induced mutants of *Caenorhabditis elegans* ", *Mutation Research*, 82 (1981).

54. R.N. Singh and J.E. Sulston, " Some observations on moulting of nematode, *Caenorhabditis elegans* ", *Nematologica*, 24 (1978).

55. V. Rodrigues and O. Siddiqi, " Genetic analysis of the chemosensory pathway ", *Proceedings of the Indian Academy of Sciences B*, 87 (1978).

international group of scientists who worked with *Drosophila* – M. Heisenberg (Germany, University of Würzburg), José Campos-Ortega (Spain, University of Freiburg, Germany), P.J. Bryant (Britain, University of California, Irvine), A. Garcia Bellido (Spain, Autonomous University, Madrid, Spain), E. Weischaus (USA, European Biology Laboratory, Heidelberg), Janos Szabad (Hungary, Medical Biology Institute, Szeged University, Hungary) and Yoshimi Hotta (Japan, University of Tokyo Japan) to name a few. Referring to the programme and the conference as 'the most ambitious' that he had ever seen, A. Garcia-Bellido reminded the scientific community present that " If genetics is going to be the Ariadna's thread... I would urge everyone, including myself, to contemplate the beautiful Ariadna, but care even more for her thread "[56].

This conference ensured not only high visibility for the group but also created the space for interdisciplinary interactions at an international level. At a workshop at TIFR held immediately after this conference, José Campos-Ortega elucidated some of the fundamental concepts of nervous system development that would later come to be accepted as a general principle in the field[57]. In many ways this conference marked the transition of the group from one that was marginal to the identity of a physics institution to one that became a significant part of its identity.

Conclusion

The molecular biology group has since been reorganized into the Department of the Biological Sciences ; as well, it has spawned a separate centre called the National Centre for Biological Sciences at Bangalore. Both places now include several other successful research programmes. But work on *Drosophila* olfaction and development occupies a key position. By 1990, the fly stock facility was maintaining 2,500 stocks and was in close contact with international as well as national stock centres in Bloomington, Indiana and Mysore respectively. I have chosen to focus historically on the beginnings of this group and the processes of its setting up in order to examine the interface between the local and global communities of science and how that affects the development of a discipline in a Third World country like India.

The molecular biology unit at TIFR, from its inception, echoed the nationalistic aspirations of Homi Bhabha's vision : to create for Indian scientists trained abroad a space to which they could return, and where they could not only carry out the work they had done abroad, but also train students and help build the capabilities within a newly-independent nation. This vision expected scientists to work at the cutting edge of scientific research, which required continual and con-

56. O. Siddiqi *et al* (eds.), *Development and neurobiology of Drosophila*, New York, Plenum Press, 1980, 2.

57. V. Rodrigues, " Prescience and critical thought : the life and science of José Ortega – Campos ", *Journal of Genetics*, 84 (2005), 220.

sistent exchange, support and collaboration with an international network that would support training as well as enable intellectual exchange. The process of setting up molecular biology within the institutional framework of TIFR therefore involved the creation of such networks. Tracing the evolution of such networks into training networks makes it possible for us to understand the hitherto unexplored relationship between institutions in the developed world and those in developing countries. The establishment of a new discipline like molecular biology within a physics institution in India with its pre-established hierarchies was a complex and complicated task. Obstacles created by local hierarchies, however, were often overcome through the effects of nurturing international networks. Such networks enabled interdisciplinary interactions within the institution as well as facilitated the growth of this new discipline in India by training students and staff, thus creating the conditions for the growth of new biology in India.

Abbreviations used :

AEET : Atomic Energy Establishment, Trombay

DAE : Department of Atomic Energy

IISc : Indian Institute of Science

TIFR : Tata Institute of Fundamental Research

TIFR-ARCH : Tata Institute of Fundamental Research Archives

TIFR-ARCH/OH : Tata Institute of Fundamental Research Archives, Oral History.

References

Anderson (Robert S.), *Building Scientific Institutions in India : Saha and Bhabha*, Montreal, McGill University Press, 1975.

Babu (P.), " Early developmental subdivisions of the wing disk in *Drosophila* ", *Molecular and General Genetics*, 151 (1977), 289-294.

Babu (P.) and Brenner (Sidney), " Spectrum of ^{32} P induced mutants of *Caenorhabditis elegans* ", *Mutation Research*, 82 (1981), 269-273.

Bhabha (Homi), " Science and development ", *Science*, 151 (1966), 541-548.

Burma (Debi Prasad) (ed.), *History of development of biochemistry and molecular biology in India : some personal perspectives*, Delhi, Indian National Science Academy, 1996.

Brookes (Martin), *Fly. The unsung hero of twentieth century science*, London, Phoenix, 2002. First Edition 2001.

Cairns (John), Stent (Gunther) and Watson (James D.) (eds.), *Phage and the origins of molecular biology*, Cold Spring Harbor, N.Y., Cold Spring Harbor Laboratory of Quantitative Biology, 1966.

'Editorial', *Science and Culture : A monthly journal of Natural and Cultural Sciences*, 32 (1966), 331-332, Calcutta, Indian Scientific New Association.

Greenstein (George), " A Gentleman of the old School : Homi Bhabha and the Development of Science in India ", *American Scholar*, 61, 1 (1992), 415.

Hill (Archibald V.), *A Report to the government of India on scientific research in India*, London, The Royal Society, 1944.

Maitra (Pabitra K.) and Lobo (Zita), " Control of glycolytic enzyme synthesis in yeast by products of the hexokinase reaction ", *Journal of Biological Chemistry*, 246 (1971), 489-499.

Maitra (Pabitra K.) and Lobo (Zita), " A kinetic study of glycolytic enzyme synthesis in yeast ", *Journal of Biological Chemistry*, 246 (1971a), 475-488.

Ramachandran (G.N.), " An International Symposium on Protein Structure and Crystallography ", *Current Science*, 20 (1963), 126-130.

Rodrigues (Veronica), " Prescience and critical thought : the life and science of José Ortega – Campos ", *Journal of Genetics*, 84 (2005), 217-222.

Rodrigues (Veronica) and Siddiqi (Obaid), " Genetic analysis of the chemosensory pathway ", *Proceedings of the Indian Academy of Sciences B*, 87 (1978), 147-160.

Sharma (Dhirendra), " How Indian atomic energy policy thwarted indigenous scientific development ", in Ziauddin Sardar (ed. by), *The revenge of Athena : science, exploitation and the Third World*, London, Mansell Publishing House, 1988.

Siddiqi (Obaid), Babu (P.), Hall (Linda M.) and Hall (Jeffrey C.) (eds.), *Development and neurobiology of Drosophila*, New York, Plenum Press, 1980.

Siddiqi (Obaid) and Benzer (Seymour), " Neurophysiological defects in temperature-sensitive paralytic mutants of *Drosphila melanogaster* ", *Proceedings of the National Academy of Sciences USA*, 73 (1976), 3253-3257.

Singh (Ram N.) and Sulston (John E.), " Some observations on moulting of nematode, *Caenorhabditis elegans* ", *Nematologica*, 24 (1978) 63-71.

Thosar (B.V.), *Growing up with science in India*, Bombay, Nita Printers, n.d.

Udgaonkar (B.M.), " Bhabha on growing science ", *Homi Jehangir Bhabha collected scientific papers*, Bombay, TIFR, 1985, XLIX-LXXIX.

Visvanathan (Shiv), *Organizing for science : the making of an industrial research laboratory*, Delhi, Oxford University Press, 1985.