The Bangalore–Birkbeck Phosphazene Project 1971–1981 – a retrospect on this and other international and interdisciplinary scientific research collaborations*

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Early acquaintance with the literature on India was followed by service with the British army in the World War II in India and South East Asia Command, where I acquired university entrance qualifications by correspondence course. My invitation in 1969 to become the first university visitor to India under the Commonwealth Education Co-operation Scheme led to a collaborative 10-year (1971–1981) joint research project sponsored by the Indian and British governments with a team headed by A. R. Vasudeva Murthy at the Indian Institute of Science in Bangalore. The outcome of this collaboration, in joint scientific publications and the subsequent careers of the students trained under it, is now assessed. Brief references to other successful international and interdisciplinary collaborative projects are made and some generalizations for such successful interactions are proposed.

My memory of India goes back to my earliest childhood. The first book I remember my mother reading to me was Rudyard Kipling's *The Jungle Book*. When I was able to read by myself, I had several books (which still grace my library shelves today) by the Indian author Dhan Gopal Mukerji (1890–1936), including *Ghond, the Hunter* (1928) and *The Chief of the Herd* (1929). I was also a keen stamp collector and learned quite a lot from my stamp album, probably printed in the 1920s. To this day I still recall the then quoted populations: India 350 million, China 480 million.

In early adulthood, India was again destined to play a part in my life. During World War II I was most keen to fight against Hitler and the Nazis and volunteered for the British Army. Some health problems delayed my training and in 1945, the army in their wisdom decided to send me to India in preparation for service with the South East Asia Command (SEAC). I recall our troopship sailing through the Mediterranean, when the surrender of Germany was announced on 8 May 1945. I stepped on Indian soil for the first time, when my troopship landed in Bombay. My first posting was to Deolali (close to Nashik city with its historic association with Mahatma Gandhi). Later there was jungle training in Bihar; first to Ranchi, then to Ramgarh. I then travelled via Calcutta to Rangoon and from there to Bangkok. A later posting to 2nd Echelon at Jhansi brought me back to India. From Jhansi small detours led to Datia, Orchha and a brief visit to Agra for my first sight of the Taj Mahal. I then left India via Madras for Singapore, where I completed my army service.

In the army I had decided to become a chemist. What triggered this decision has puzzled me to this day, as I had not had any chemistry lessons in school. I had had to leave school at the tender age of 13. Now, a soldier in my early twenties, I had decided to study for my London Matriculation examination to enable me to enter university to study chemistry. This was done by a correspondence course. I served in an infantry battalion and was considered somewhat of an eccentric, the odd chap, who had his nose in books at every spare opportunity, always studying, even in the middle of the jungle. I eventually passed the London Matriculation examination in Singapore at the age of 23. It was the hardest examination in my life; my brain had become so rusty.

Following my return to the UK and having completed my B Sc and Ph D studies, my academic career started in 1953, when I was appointed as an Assistant Lecturer at Birkbeck College, University of London. Birkbeck College will be known to many readers. Patrick M. S. Blackett (1897–1974; Nobel Prize for Physics 1948) was Professor of Physics there, as was in my days John D. Bernal (1901–1971). My friend and colleague, Derek H. R. Barton (1918–1998), who won the Nobel Prize for Chemistry (1969), was on the staff when I joined. The Birkbeck connection with India has been discussed in an Indian journal[1] in 1976.

My Ph D topic had been on organosilicon compounds. My supervisor, however, had allowed me to change the topic within this field, from the one he had originally given to me, to one which I had thought up myself and found to be more exciting. While preparing my lecture notes for undergraduates at Birkbeck, I came across the field of phosphonitrilic chlorides (as it was then called) and my interest was greatly aroused. It soon became a passion, which has continued to dominate my research life. The subject was born at the hands of Justus von Liebig (1803–1873). Its first compound, $N_3P_3Cl_6$, was analysed, albeit incorrectly, by Friedrich Wöhler (1800–1882). It received its first detailed attention in the 1890s by H. N. Stokes (1859–1942). The field then lay largely fallow for many years[2].

In the mid-1950s, I began to experiment myself with these compounds and applied the techniques of organic chemistry, such as column chromatography. I initially worked by myself on the bench. Then in September 1957, I was joined by my first research students: Cedric Stratton (Ph D 1963), an industry-based part-timer and, a month later, by my first full-time student from Calcutta, Sanjoy Kumar Ray (Ph D 1963). At that time, I had not yet raised a single penny for my research.

Then a year later the size of my research group rocketed. Using a term from the big bang theory of the universe, a period of hyperinflation set in. By the summer of 1960, when we started taking

*Dedicated to the memory of my dear friend and co-Director of the Bangalore–Birkbeck Project, the late Professor A. R. Vasudeva Murthy.

photographs of the inorganic chemistry research group, it numbered 23, all but one, working with me and all except one, full time! And virtually all were supported by the grants, which I had raised!

The transformation was spectacular. How did this come about? In 1957 there were no funds for my research; a couple of years later I was the biggest research fund raiser in the college. In the Cold War period the space race (1955–1972) between the USSR and USA (and its allies) attracted much attention. The launching of the Sputnik in 1957 by the Soviet Union attracted enormous news coverage. Many Western agencies began to fund innovative research, which might be of benefit in this technological competitive race. This was later reinforced by speeches by the US President John F. Kennedy in 1962, 'We choose to go to the moon' and the Prime Minister of UK Harold Wilson on the 'The white heat of the technological revolution' in 1963.

My type of chemistry, based on phosphorus–nitrogen and similar non-carbon, non-metal backbones, aroused a great deal of interest and attracted research funding. Virtually every research proposal I wrote at that time got funded, sometimes very generously indeed. It probably did no harm to my applications that in the 1890s, H. N. Stokes had discovered that my type of starting material could be converted to an inorganic polymeric elastomer/rubber[2], $(NPCl_2)_n$.

Some far-sighted, eminent scientists had spotted the potential of my research even earlier. Sir Robert Robinson (1886–1975), Nobel laureate for Chemistry (1947) and Emeritus Professor at Oxford University invited me to come and see him in the late 1950s. He had a rather forbidding reputation, as somebody who did not suffer fools gladly. However, he was charming to me and called my compounds 'Organic Chemistry without a carbon backbone'. It was a welcome experience to talk to a great scientist, who understood what I was trying to achieve and he quizzed me very knowledgeable about my work. He was at that time a Director of the Shell Chemical Company and I duly became a very young consultant to the Shell Research Company. I directed a research group there and had to defend my ideas to a most formidable committee consisting of Sir Robert Robinson, Lord Rothschild (an eminent scientist, then Chairman of Shell Research) and S. A. Barratt, the global research coordinator for Shell Chemicals. It should be remembered that I was then still a very junior Lecturer, having only been promoted in 1956 from Assistant Lecturer (an academic grade so low, that it no longer exists). A few years later BP also approached me with a similar proposition; my type of chemistry was then very much in vogue.

John Bernal also took an interest in my work and encouraged a young crystallographer on his staff, Graham Bullen, to take an interest in my compounds. Bernal also introduced me to the potential of the Reichert–Kofler polarizing microscope with a micro-heating stage. Together we watched the solid-state phase transformation of a compound which Cedric Stratton had prepared. It was my first experience of polymorphism.

Our first preliminary publications began to appear in the journal *Chemistry and Industry* in 1959–60. One particular publication brought an unexpected request for samples for crystallographic studies. This approach was to lead to events, which impacted my thinking most profoundly. The request came from an Indian Ph D, N. V. Mani (1934–1972), shortly after the appearance of the preliminary publication by Bernard Wells and myself: 'Phenyl derivatives of cyclotriphosphazatriene (Phenyltriphosphonitriles)'[3]. We had reported three compounds in a series, obtained by a Friedel–Craft's reaction: $N_3P_3Cl_4Ph_2$, $N_3P_3Cl_2Ph_4$ and $N_3P_3Ph_6$. We had shown what type of compound could be isolated from this reaction and, by a process of chemical degradation, had demonstrated by the isolation and characterization of diphenylphosphinic acid, $Ph_2P(O)OH$, that the phenyl groups were attached in pairs to the same phosphorus atom, a reaction which we called geminal substitution. We thus believed that we had all the information, which we had set out to discover. In a scientifically unbecoming manner, I questioned the value of a crystallographic investigation.

Nevertheless, in spite of my reservations, I sent Mani these three compounds and he proceeded to do a postdoctoral attachment with Farid Ahmed at the NRC (National Research Council) in Ottawa. Ahmed had been trained by the eminent crystallographer Durward W. J. Cruikshank (1924–2007) at Leeds University and thus had an outstanding scientific pedigree. Our three compounds were duly studied with great accuracy in three separate papers[4–6]. When I began to appreciate the data they had provided, I was ashamed of my scientific arrogance. Yes, the attachments of the phenyl groups (Ph) were, as we had deduced chemically. What we had not anticipated was the considerable effect on the bond lengths and angles they had produced and this knowledge, much extended by further very accurate crystallographic studies, became a most important component in my intellectual armoury.

Ahmed and I collaborated further. Another important series consisted of the three isomers, $N_3P_3Cl_3(NMe_2)_3$, geminal, *cis*- and *trans*-non-geminal (geminal, a pair attached to the same atom; non-geminal, a pair attached to different atoms). Again we had previously established by chemical and spectroscopic methods, their overall structures, but again their detailed architecture was most enlightening. Although Ahmed, Mani and I never collaborated formally on joint publications, their detailed crystallographic work on my compounds greatly affected my thinking process about them. Quite recently, I discovered that Mani and K. Venkatesan (see later) were both students in 1953 of S. Ramaseshan (1923–2003), a distinguished crystallographer in the Department of Physics at the Indian Institute of Science (IISc), Bangalore. Thus here, was my first indirect contact with IISc. The year 1953 was also a personal landmark for me – I was appointed as an Assistant Lecturer at Birkbeck.

More or less at the same time, as this intellectual impact from crystallography, a most important collaboration started with David Feakins, a colleague of mine, in the Chemistry Department at Birkbeck. Once again a chance observation led to important insights. When I started my phosphazene research, I realized that some very basic questions about its chemistry had not been asked and thus the answers were unknown. Although at first the approach may have seemed trivial, important information was eventually obtained from seemingly simple questions. Could all the six chlorine atoms (Cl) in $N_3P_3Cl_6$ be stepwise replaced and, if so, by what reagents? We had already seen that by the Friedel–Crafts route only derivatives with 2, 4 or 6 phenyl groups could be isolated and this had important reaction mechanistic implications. This was an example of an electrophilic Friedel–Crafts reaction.

But what about nucleophilic reactions with a simple secondary amine, dimethylamine, $HNMe_2$? Again, surprisingly, nobody had asked this question before and thus the answer was unknown. I had given this task to Ray, my above-mentioned research student. For the generalized structure $N_3P_3Cl_{(6-n)}(NMe_2)_n$, he had isolated derivatives with $n = 1$, 2 (three isomers), 3 (three isomers), 4 and 6. Hard as he tried, Ray could not find a derivative with $n = 5$. Dimethylamine was a secondary amine, HNR_2. He tried other amines; most secondary amines behaved similarly. Primary ones, H_2NR, differed. Things were becoming more complicated now. With tertiary butylamine, H_2NBu^t, for example, Ray obtained only derivatives $N_3P_3Cl_{(6-n)}(NHBu^t)_n$ with $n = 1$, 2 (no isomers), 4 and 6 (ref. 7). However, the overriding message was that 'no amino derivatives with $n = 5$' were isolated.

Eventually after many experiments, Ray came to me and announced that he believed that at last he had isolated the long searched for derivative with $n = 5$! He had isolated it from a primary amine reaction and had obtained what he thought was $N_3P_3Cl(NHR)_5$. Alas, when the elemental analysis came back, the data were close, but did not quite fit to the calculated values. The analytical values however fitted well the formula for $N_3P_3(NHR)_6$,HCl. This could be chemically proven by the removal of HCl, leaving $N_3P_3(NHR)_6$. But this turned all the then accepted chemical wisdom on its head. Hitherto, all nitrogen atoms attached to phosphorus atoms had been found to be weakly basic, analogous to the nitrogen in organic acid amides, $RC(O)(NH_2)$ and not as basic as that in organic amines, H_2NR, HNR_2, NR_3. Yet here, in our reactions in the presence of an excess of primary organic amines, our Phosphazenes had competed favourably for the acid! They were obviously quite strong bases, but how strong?

Here I must interject some comments on nomenclature. It had become apparent to me that the phosphonitrilic nomenclature was quite unsuitable for the rapidly increasing complex chemistry, which I had foreseen for some time. I thus proposed a new one, based on the name 'phosphazenes'. For this [for which I had consulted the then Master of Birkbeck, Sir John Lockwood (1903–1965), a distinguished classicist], I received the wholehearted support of the long-time and distinguished editor of the prestigious journal *Chemical Reviews*, Ralph Shriner (1899–1994), when I visited his department at the University of Iowa in 1959 and gave a seminar there. We published a major review article in his journal[2] in 1962. Our new nomenclature was readily accepted worldwide.

Now back to the unexpected, very strong basicity of our aminophosphazenes. Here I was out of my depth in physical chemistry. Luckily for me, Birkbeck was quite strong in electrochemistry and, as I have said, one of my colleagues, David Feakins, was interested in collaboration. I transferred one of my Ph D students, Allan Last, with his enthusiastic consent from preparative phosphazene chemistry to this collaboration on basicity studies and we struck an enormously productive seam of scientific gold. Over the next 8–10 years, in frequent brain-storming sessions, Feakins and I, with our co-workers produced ten joint papers which transformed the phosphazene landscape. The timing of this research was fortunate. I had a large synthetic research group, which provided literally hundreds of samples for basicity measurements. We were able to deduce for all the different chemical substituents on our phosphazenes, substituent constants, which enabled us to evaluate the basicity at the different nitrogen atoms of the six-membered phosphazene rings. Agreements between calculated and measured basicities were gratifyingly close. We deduced the different substitution patterns for different amines (some non-geminal, some geminal, some a mixture of both patterns) and other incoming reagent groups, which were born out of subsequent spectroscopic and/or crystal structure determinations. This fruitful collaboration[8–10] came to an end when David was appointed to a professorship of chemistry in Dublin.

Mani now made contact again and requested another sample for crystallographic studies. He was now spending time at the University of Groningen, an institution with a history of interest in our type of compound. I sent him a sample of $N_3P_3Cl_2(NHPr^i)_4$,HCl. The crystal structure by him and A. J. Wagner[11] in 1971, confirmed the site of protonation, which had been correctly predicted from our basicity studies. Additionally, it showed the very large changes in ring architecture produced by the positive charge on the ring nitrogen atom. Thus Mani must be considered one of the flag bearers and proselytisers for phosphazene structures amongst the crystallographic community. On writing this in 2014, I searched for more information about him. I discovered his association with IISc. Thus some sort of connection had existed for almost a decade between IISc and me, before I physically set first foot there in 1969. From other IISc friends, H. Manohar and K. Venkatesan, I learned that after Groningen, Mani had gone to USA to work with Muttaiya Sundaralingam at Madison, Wisconsin and sadly died there at a young age – a rather unfulfilled potential. Stan Cameron managed to discover his death certificate!

Chemical research is an intellectual and experimental endeavour based on the interactions of a number of different individuals; in other words teamwork. How are such teams organized? In universities and similar institutions the team leader is usually a member of the academic staff, perhaps a lecturer or a professor, who is usually called the research supervisor, research director or director of studies. To my mind the Germans have a most appropriate nickname; they call him *Doktorvater* (the father to the aspiring doctoral candidate). (Some may object to this as sexist, but chemistry has regrettably been until recently, in the UK, the USA and Germany, at least, a rather male-dominated subject. Perhaps we shall now begin to see *Doktormutter* putting in an appearance, especially as Angela Merkel, the German Chancellor (a graduate in physics, who has researched in quantum chemistry), is referred to affectionately as *Mutti*.) The indication of a parental relationship is apt and I shall return to it later on. The other members of the team are typically in their early to mid-twenties, graduate students working for a higher degree such as the Ph D (Doctor of Philosophy) or recent 'doctors', so-called postdoctoral research fellows ('postdocs' for short), who desire more advanced research training.

To train them adequately in their research discipline is an obvious and responsible task for the research supervisor. Less obvious at first, but equally important in the long run, is for the *Doktorvater* to extend good pastoral care. I have had plenty of experience of that. Over the years, I found that I had to devote more and more of my time to pastoral

care. This could have been due to working conditions in particular and life in general, becoming more stressful, particularly for students. People with great intellectual abilities suffer the same stresses and strains as everyone else. Perhaps, being often less street-wise, they are frequently less well equipped to deal with these ups and downs of life. If a member of my team had worries, be they financial, health, accommodation, emotional or a whole host of others, his performance will inevitably suffer. If the stresses became too great, they might even decide to leave.

What has been my response to these stress problems amongst members of my team? It was pastoral care in the widest sense. Anybody plagued by worries cannot give of their best. If they lack the experience to cope with them, the problems become exacerbated. One hopes that the team leader being older and more experienced is more worldly-wise and can give help and advice in such situations. For this to become feasible, an aura of trust must exist in the *Doktorvater*. Trust in, and respect for, a team leader does not, however, come automatically; it must be earned!

One would be surprised by the number and variety of problems I was presented with over the years. They ranged from an overseas member's child with a hole in the heart problem, spouses wishing to terminate my co-worker's participation in a project for short-term advantage at the expense of a longer-term gain, marital problems or lovers' tiffs, loneliness and perhaps even depression in a strange environment, to more straightforward financial worries. As an interesting aside, a great friend of mine, an eminent professor in an Arts subject, commented to me on reading my views on the above subject – I quote him: 'What you say about the whole *Doktorvater* concept interested me, because only very rarely can that relationship develop in Arts subjects'.

Over the years, my reputation as a caring and pro-active *Doktorvater* became more widely known. I expected hard and dedicated work from my team. But, in turn, I felt responsible not only for their scientific education and training, but also for their wider cultural and educational nourishment, as well as their health, financial and emotional welfare. I recall a visit to Oxford University, where a fellow chemist, who had never met me before, greeted me with 'I hear that you are a bit of a slave driver,' but he then continued '…but I also hear that you look after your students very well, in every way, and try hard to find them good and suitable jobs.' I was pleased with his comments, as they summed up accurately my approach!

Pastoral care was clearly something which students' parents looked for and valued. Sanjoy Ray's father (a retired judge) wrote to me a charming letter of introduction, entrusting me to act like a father to his son. I always had tried to live up to this hope!

Amongst the rapidly increasing intake of new research workers, in the hyperinflation period, were a number of keen young chemists from India. These, like all my other co-workers, had the usual student problems, which I tried to sort out satisfactorily. This involved frequent contact with the Indian High Commission in London. In July 1966, we had a visit to my research group from Jamal Kidwai, the Minister responsible for Education at the High Commission and his cousin, Akhlak R. Kidwai, then a chemistry professor at the Aligarh Muslim University. All this activity, scientific and pastoral, seemed to have played a part in Jamal Kidwai inviting me to become the first university visitor under the Commonwealth Education Co-operation Scheme to come to India. This took place in early1969. It was a fascinating experience, which space does not allow me to enlarge on here. My friend Akhlak Kidwai had gone to great lengths to make this trip, and the subsequent ones, most memorable. He amply succeeded in this task. On the last day before my return flight from India in 1969, I was sitting with Indian friends in my hotel room in New Delhi. There was a knock on the door, and on opening it, we found a splendidly apparelled messenger in the presidential livery, who handed me a package. On opening it, I found a large photograph of the President of the Republic of India, Dr Zakir Husain (1897–1969), inscribed to me personally (Figure 1). Unfortunately, 45 years later, the ink has faded and it can be read only with great difficulty.

For
Professor Robert A Shaw
with warmest regards
Zakir Husain
3.3.1969.

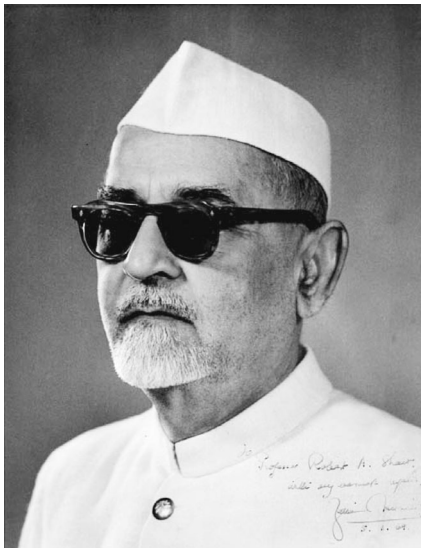

Figure 1. Portrait of the President of India, Zakir Husain inscribed to Robert A. Shaw, New Delhi, 1969.

I had had the privilege of meeting him a few days earlier and having an interesting and stimulating conversation with him. This photograph, together with others, similarly inscribed, from distinguished chemists I knew (Sir Robert Robinson, Lord Alexander Todd, Derek Barton, Georg Wittig,…,) was on display behind my desk in my Birkbeck study.

At the end of my 1969 visit, the Minister of Education, V. K. R. V. Rao, invited me to his private residence in New Delhi and asked me to write a report of my impressions of this visit. I told him that I would be pleased to do so. When some months later he visited London, Lord Fulton, the Chairman of the British Council, gave a lunch for him, where a number of senior British Council officials were present. After the lunch, I requested Rao's permission to circulate my report also to other interested parties, which he gladly gave.

I had been frequently asked to examine Indian Ph D theses and had noted the acute shortage of instrumental facilities in the 1960s. This situation was quite familiar to me; my own work too had suffered from similar shortages and I had learned how to overcome these by suitable collaborations. While in New Delhi, I had met Sir Christopher Cox, a senior educational adviser to the Overseas Development Administration (ODA) of the Foreign and Commonwealth Office (FCO). He encouraged me to follow-up my ideas with a request for action. From

personal experience I already knew about the hard work and dedication of Indian research students. An idea had begun to crystallize in my mind: Would it be possible to combine the resources of highly dedicated, able and keen Indian researchers with instrumental facilities (my own and those of collaborating colleagues in other laboratories) in the UK and elsewhere? I had already collaborated successfully with colleagues in the UK and, through Mani, with Canada. What about a similar collaboration with India? The timing seemed auspicious!

By this time some 'golden rules' about successful collaborations had been forming in my mind: (1) The total output must be bigger than the sum of its part. (2) Each party must take out more than they have put in. (3) There must be complete confidence in the competence and integrity of the collaborating parties. (4) Personal chemistry between the groups, especially the group leaders, helps enormously. (5) Avoid hierarchical order of authors for publications; use an alphabetical order. (6) Collaborations should be initiated and structured from the bottom-up, not top-down. Authorities can authorize overall project policies and funds, but they should not interfere in partner selections.

I was therefore intellectually and emotionally ready for further extensions in collaboration. I was convinced that by combining the instrumental resources, which were at my disposal with skilled and enthusiastic manpower elsewhere, we would create a situation beneficial to all parties involved. I do not exactly know when I conceived this idea, but it certainly matured during my trip to India. I discussed it there and had found a suitable partner and intellectual soul mate in A. R. Vasudeva Murthy at IISc.

Murthy proved to be the most stimulating and important scientific contact I had made on the entire tour. Both on a personal and professional level we hit it off immediately. We shared deep interests in history and he had a wide knowledge of Indian history. He proved a most knowledgeable guide to the famous 13th century Hoysala temples at Belur, Halebidu and Somnathpur with their superb stone carvings. Together we also visited Tipu Sultan's palace at Seringapatam, the famous 5-m high Nandi, the Bull, sculpted out of rock, on Chamundi Hill, outside Mysore and finally the Maharaja's palace. Murthy was also an enterprising chemist, who had started a number of new lines of research both in his department and also with local industry[12]. I gave two lectures in IISc, one of India's most eminent scientific institutions, which had been founded by the Tata family.

My initial discussions with the two Governments received an enthusiastic response. The time seemed ripe for such a project. Then we ran into some problems, which we had not anticipated. Having agreed that the idea was sound and worthy of support, both Governments wanted me to implement this project with the institutions which *they* currently favoured. I felt obliged to take a firm line. I pointed out that I would stake my reputation on getting this project to work and I was unwilling to do so, unless my 'golden rules' were met. Murthy and I had the common interest and the personal chemistry. The project was to be with him or, as far as I was concerned, it was off. I eventually got agreement and the project started officially in 1971. It had taken two years to get it officially off the ground!

The then Indian High Commissioner in London, Apa B. Pant (1912–1992), gave a reception to launch our project and amongst the numerous distinguished attendees with interests in India were *inter alia* Blackett and Bernal.

While officialdom was taking its time to deliver formal approval, Vasudeva Murthy and I were not content to sit around twiddling our thumbs and waiting for an eventual official approval. We started an unofficial collaboration on other topics, which involved in Bangalore D. K. Padma, later a professor at this institution. Six publications arose from this; all publications linked to Bangalore–Birkbeck collaborations (Figures 2 and 3) are listed in Appendix 1.

Once we had the official approval from both Governments, S. S. Krishnamurthy was recruited as an Assistant Professor and spent one year, 1971–72, with me as postdoc at Birkbeck getting acquainted with the experimental techniques (which resembled those of organic chemistry), with which we practised phosphazene chemistry in my group.

One could well ask the very pertinent question, 'Was the topic, phosphazene chemistry, which was my privilege to introduce to chemists at Bangalore, a worthy one?' I leave others to judge that. I can only say that scientists of the calibre of Robert Robinson, John Bernal, Alexander Todd, Georg Wittig and Derek Barton found it interesting and worthy of

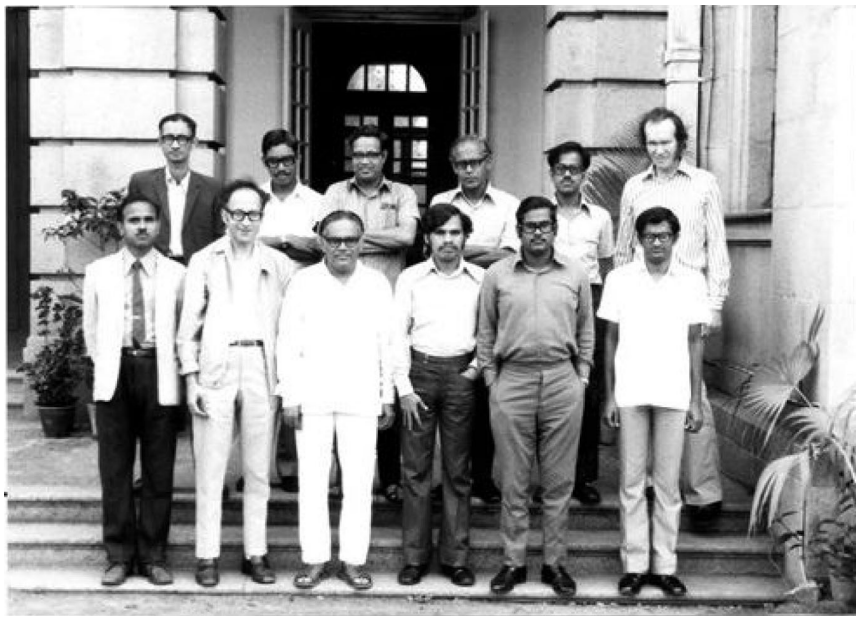

Figure 2. Some members of the Bangalore–Birkbeck team, Bangalore, 1975. Back row (left to right): Dr (now Prof.) H. Manohar, Mr (now Prof.) Y. Sudhakar Babu (Ph D student of Manohar), Dr (now Prof.) S. S. Krishnamurthy, Dr (now Prof.) K. Venkatesan, Mr (now Prof.) M. N. Sudheendra Rao and Dr R. Keat. Front row (left to right): Dr (now Prof.) J. Ramakrishna, Prof. R. A. Shaw, Prof. A. R. Vasudeva Murthy, Mr (now Dr) K. Ramachandran, Mr (now Dr) A. C. Sau and Mr (now Dr) R. Sridharan (Ph D student of J. Ramakrishna).

discussion. I can update you to a more recent view of this subject. In 2005, I was awarded my second Honorary Doctorate at the Gebze Institute of Technology in Turkey. Here too I was responsible for introducing this research topic, in this case to Turkey. My family was with me and my son had just told me that at his chemistry studies at Cambridge University, inorganic ring systems with a special emphasis on cyclophosphazenes was a specialist subject taught there. Just then Cambridge had been ranked second after Harvard University in the world listing. I could thus safely tell my audience that the research topic I had introduced to them was now taught in one of the world's leading universities. They had thus no need to be coy about their research subject.

When the collaborative project was first conceived and then presented to both the Indian and British Governments, we had two goals in mind:

(1) To help train Indian scientists in India under Indian conditions to produce published work of world-class status.
(2) To produce future leaders of Indian science.

The eventual outcome of these recommendations was the Bangalore–Birkbeck Phosphazene Project, which had financial support from both Governments in the period 1971–1981.

I would like to highlight, by means of one example, what can, sometimes unexpectedly, be achieved by a skilled researcher. In Bangalore, as earlier by other research workers in USA, Germany, the Soviet Union and in my own laboratory in London, the reactions of the eight-membered ring compound, the tetramer, $N_4P_4Cl_8$, with amines had been investigated. Here, I will only mention the results achieved in the presence of a large excess of amines leading to the fully aminolysed derivatives, $N_4P_4(NHR)_8$, $N_4P_4(NR_2)_8$ or $N_4P_4(NHR)_{(8-n)}(NR_2)_n$. Then a young Indian Ph D student, Arjun C. Sau, also studied this reaction at IISc. Like the other researchers before him, he too isolated fully aminolysed octa-amino derivatives, but he also noted that while crystallizing successive crops of products, that although they *almost* looked the same, that they were *not* the same; they contained no chlorine atoms, but only 7, not 8, amino residues. When this new product was purified and its structure established (by a three-laboratory collaboration), it was the first time a bicyclic phosphazene, $N_4P_4(NMe_2)_5(NHEt)(NEt)$ had been prepared.[13] A new branch of phosphazene chemistry had been opened up, all due to the acute observations of a young Indian chemist. Previously, all the researchers who had studied this reaction in the USA, Germany, the Soviet Union and, dare I say it, in my own laboratory at Birkbeck in London, had overlooked this most exciting product. If I was a lawyer, I would now say 'I rest my case'!

Figure 3. The two co-Directors of the project, Bangalore, 1975 – Vasudeva Murthy and Shaw.

Other collaborations in Bangalore sprang up. With the crystallographer, H. Manohar another four joint papers arose, two of which were also co-authored with Stan Cameron. With another Bangalore crystallographer, K. Venkatesan and again Stan Cameron we had a further two joint papers (see Appendix 1).

There were other more immediate effects from the success of the project. The Bangalore Department received extra funding from the University Grants Committee (UGC) for equipment and this was followed by help from UNESCO. Obviously success bred success. The project was financially supported in India by the UGC and in the UK by the ODA of the FCO (later the ODM). The British Council was involved with arrangements once we (my assistant or I) had arrived in India and the return visits of our Indian colleagues to the UK.

Over the years, ever since my first visit to IISc in 1969, I remained in contact with my friends and colleagues there. In January 2014, I received two e-mails from Bangalore. The first was the sad news that my dear friend and co-director of the Bangalore–Birkbeck Phosphazene Project, Vasudeva Murthy had passed away, following a long illness[14]. I sent my condolences to his family via his then deputy, Krishnamurthy, with whom I have remained in constant contact. Murthy's scientific achievements were honoured by several awards (Figures 4 and 5).

The second e-mail was of a more joyous nature. Krishna, as I have known him for over 40 years (or to give him his full present title Emeritus Professor S. S. Krishnamurthy), had recently been honoured by being asked to deliver in February 2014 the 'Lifetime Achievement Award' lecture at Mumbai (Figure 6). Krishna is probably the most outstandingly successful scientist amongst my former co-workers; I was delighted to hear of this honor for him.

I shall be 90 years old at the end of 2014. In the current writing of my memoirs and looking back on a long and eventful life, I noted that India occupies one of its longest chapters, with Bangalore at its epicentre. With the death of my co-director, Vasudeva Murthy, an era has passed and it seemed appropriate

Figure 4. Vasudeva Murthy's retirement, Bangalore, July 1986. T. R. Kasturi, then Dean of Science Faculty is presenting the award. Seated, another retiree, S. Soundararajan.

Figure 5. Vasudeva Murthy receiving the Sir M. Visveshvarayya award from the Karnataka State Council for Science and Technology, Bangalore, 1999. Behind the recipient are R. Narasimha and M. N. Srinivasan, both from IISc, Bangalore.

Figure 6. S. Chandrasekharan, then President of the Chemical Research Society of India, presenting the Lifetime Achievement Award to S. S. Krishnamurthy, Mumbai, 2014.

Box 1. Subsequent careers of former Bangalore co-workers

S. S. Krishnamurthy: Professor, IISc, Bangalore
D. K. Padma: Professor, IISc, Bangalore
A. C. Sau: Scientist, Hercules Inc, Delaware, USA (postdoc U Mass, Amherst)
M. N. Sudheendra Rao: Professor, IIT Madras (postdoc Goettingen and Calgary)
K. S. Dhathathreyan: Scientist, Associate Director, Center for Fuel Cell Technology, International Advance Research Centre for Powder Metallurgy and New Materials, Hyderabad (postdoc in Goettingen and Calgary)
K. Ramachandran: scientist. 3M Dallas (postdoc, Univ Vermont)
P. Ramabrahmam: scientist, Oil and Natural Gas Commission, India
P. M. Sundaram: Professor, American College, Madurai (superannuated)
V. Chandrasekhar: Professor, IIT Kanpur and currently Director (on secondment), National Institute of Science Education and Research, Bhubaneswar (Postdoc, U Mass, Amherst, Visiting Scientist, Goettingen)
K. C. Kumara Swamy: Professor, Central University of Hyderabad (postdoc, U Mass, Amherst)

now, about 40 years later, to assess the long-term results of this 10-yr project. An international project then was a novelty. Now with a rapidly changing world and technological advances, science has become and continues to be more and more international.

I had already listed above the two main aims for this collaboration. For item (1) we can point to more than 50 joint publications in prestigious international journals (see Appendix 1). For item (2), there needed to be the opportunities for able and ambitious young scientists and it appears this too succeeded, though it needed the passage of time to evaluate the sustained success of these young scientists. Below is a list of the career paths of the young chemists (kindly provided by Krishnamurthy), who were jointly trained during the ten official years of our project.

I think that the above impressive list speaks for itself. And there were also undoubtedly other indirect effects. Following the official end of our Project, Murthy and Krishnamurthy have undoubtedly trained other able Indian scientists and these, as well as the distinguished cohort mentioned above, will have had a trickle-down effect on Indian science. I learned that research on aspects of phosphorus–nitrogen chemistry (which I had been privileged to introduce to India) now flourishes in several leading institutions in the country.

Now, some 40 years later, since the conception of the project, we can see and judge its outcome and long-term effects. I feel that both Governments can now see that their money was well spent.

Finally, before I leave India in this essay, I must point out that while the above focus has been on Bangalore, most of the Indian Ph D students we had at Birkbeck, including those in my own group, came through word-of-mouth recommendations, one must presume, predominately from Bihar and the Calcutta area. Thus my trips to India always included visits to Bihar and Calcutta. Bihar, where at the University of Patna, an old friend, a former Ph D student, Sudhin Das (1924–2011), of my former late colleague David Ives (1906–1983) at Birkbeck, had become a Professor and Vice-Chancellor. My young guide, Srini, in 1969, became Professor K. V. Srinivasan at the same university. One of my all-time favourite students, Chandramauleshwar P. Thakur (Ph D 1970), known as Chandra, who has become a great friend and frequent visitor in London also hails from that region. Calcutta too became a standard port of call for me in India, because of my former students: Sanjoy Kumar Ray (Ph D 1963) and Sunil Kumar Das (Ph D 1967).

Throughout my academic career I have been convinced of the value of interdisciplinary research collaboration, which for me, as an almost automatic corollary, means International collaboration. The result of this conviction were numerous joint publications in prestigious refereed journals, involving initially universities in the UK (Oxford, Glasgow, Essex, Cardiff, Colerain, Southampton, etc.) and later on also abroad in India, Turkey, France, Poland and some others.

I will, more briefly, try to illustrate this general point with partners from countries other than India.

My 'French connection'

Not very long after my visit to India in 1969, I received a letter from Toulouse, from a group of researchers headed by Jean-François Labarre. They requested samples of our phosphazenes to measure on their very specialized instrument for the Faraday effect. They had been studying this effect in benzene derivatives and our six-membered phosphorus–nitrogen rings seemed an obvious inorganic extension. I wrote back saying that we could not provide the large quantities they had requested, but that I would be happy to train one or more of their researchers in our techniques. Sometime in 1970, one member of Labarre's group, Jean-Paul Faucher, a pleasant young French chemist came to Birkbeck and we trained him in phosphazene chemistry. Following this, I was invited as a Visiting Professor to Toulouse, where I spent about one month during an Easter vacation. Very soon a firm personal friendship was established between Labarre, Faucher, and other members of his team and myself. Labarre's group was based at the Paul Sabatier University. He had an enormous and infectious enthusiasm and this was reflected in his group. He also had a keen interest in theoretical chemistry and the subject of bonding in the phosphazenes intrigued him. Thus having seeded the interest in phosphazene chemistry in Toulouse, it took root there and flourished. Joint work on the Faraday effect and on bonding in phosphazenes was published. I got involved with Faucher's doctoral thesis and was virtually acknowledged as co-advisor of his doctorate. On the back of his thesis was a beehive representation, with myself in the centre as the queen bee. Chemistry aside, my life was also culturally enriched by what I had learned about

the local French history. My command of French also increased by leaps and bounds. In 1978, the Paul Sabatier University conferred an Honorary Doctorate on me, an honour which I appreciated even more, being the first chemist to have received this award since the great Linus Pauling (1901–1994) (Nobel Prize in Chemistry 1954; Nobel Prize Peace 1962).

Other collaborative projects

In 1973 I was approached by a Hans Rose, a young German chemist from the *Ruhr-Universität Bochum*, who had heard me speak at a symposium in Prague. This approach resulted in collaboration from 1973 to 1978. Rose also subsequently visited Bangalore.

Sometime around 1968, the Oxford University crystallographer, Keith Prout contacted me. This too became an innovative collaboration. I had collaborated with crystallographers before, but it had always been, up to then at arms lengths. Either they had approached me for compounds or I had approached them to do structural studies. Until Prout appeared on the scene, publishing-wise, we chemists and crystallographers went our separate ways. With Prout we began to integrate our work and publish together, which proved to be much more productive and also much more fun. The collaboration lasted from 1968 to 1976. One of Prout's Ph D students, Stan Cameron, was involved in most of the above joint work, but got, once academically independent, even more involved with us. This lasted during 1968–1995 and resulted in 15 joint publications. As with Prout, there was a personal chemistry with Cameron. We kept in touch through his postdoc in Sweden, his return as a postdoc to Oxford, his lectureship at the New University of Ulster in Colerain and finally his professorship at Dalhousie University in Canada. Cameron also got involved with Rodney Keat, Labarre and, very importantly, IISc. Keat was one of my best research students at Birkbeck. After his appointment to a lectureship at Glasgow University, we kept in touch. Although he was beginning to carve out an independent career in another branch of phosphorus–nitrogen chemistry, we continued to collaborate on joint researches and publications.

Fortunately, I had been able to arrange for both Keat and Cameron to visit Bangalore under the Younger Scientists exchange scheme and this had catalysed joint work with or without me with the Bangalore group of scientists. Through Keat, I was introduced to Andrew L. Porte (1931–2005), a Glasgow colleague of his. Porte's very considerable expertise was in nuclear quadrupole resonance (NQR) spectroscopy. This proved enormously useful in our joint work as chlorine atoms were often present in the compounds we were studying. Porte's knowledge of ^{35}Cl NQR spectroscopy was of profound interest. Our collaboration lasted from the early 1970s until the late 1980s and led to eight joint publications. In addition to learning about the electronic environment around the chlorine nuclei and relating this to earlier collaboration with crystallographers regarding the lengths of the phosphorus–chlorine bonds, his studies also observed the effect of temperature and drew attention to solid-state phase changes which might occur in the temperature range he studied from liquid nitrogen upwards. This clarified a problem which my wife, Leyla, had observed in her crystallographic studies and on which I will elaborate later on. I noticed with pleasure that a similar collaboration between chemists and physicists (led by J. Ramakrishna) at IISc had also led to further NQR studies[15,16].

Through being married to an enthusiastic and able crystallographer, I became vastly more involved with this technique. Where previously crystallography was a useful adjunct, it now became an integral part of our research. We started a series of crystallographic studies on 'Structural investigations of phosphorus–nitrogen compounds', which were designed to relate crystallographic parameters to chemical reactivities as well as to physical properties such as basicity and NQR spectra. One of these structures, $N_3P_3[O(CH_2)_3O]Cl_4$, puzzled us greatly[17]. Leyla was always keen to get ultra-high quality crystallographic data. She obtained it for the other two compounds in the paper, but failed to do so with this one. A clue to this was obtained from our collaboration with Porte from his temperature-dependent ^{35}Cl NQR studies. The puzzling compound showed a solid-state phase transition not far below room temperature, where our crystallographic studies were at that time carried out[18]. The other two compounds did not have such solid-state phase transitions. So we now knew the cause for the reduced accuracy, but not its exact details. This would have to wait for many more years before its eventual clarification. Leyla was a highly valued co-author on 25 publications, before she chose a different career path and took up risk management and capital management in investment banking, though she remained intellectually involved in our crystallographic work.

This brings me to another crystallographic collaborator Michael B. Hursthouse, who appears first on our horizon in 1976, when he, in conjunction with another crystallographer from our earlier days, Graham Bullen (then at Essex University), collaborated on an interesting structure of ours. He then had offered Leyla in the 1980s postdoc facilities at Queen Mary College, London. I turned to him again for crystallographic support for my post-retirement research activities (see later).

The Turkish (Ankara) connection

In July 1967, two Docents from Ankara University appeared at Birkbeck. The husband and wife team were Turgut and Neçla Gündüz. Both subsequently became professors at Ankara University, Neçla becoming the Head of the Department, and I received an excellent Ph D student Türsen Demir and later three postdocs on their recommendations. In the 1980s, Leyla and I had a joint project with both the Gündüz's and their team. Eventually we published a total of ten papers with them. Demir's work involved at different stages collaboration with crystallographers Prout, Cameron and Venkatesan.

On the Birkbeck College Open Day in 1973, celebrating the 150th anniversary of its foundation, Margaret Thatcher (a chemistry graduate), M.P., then Secretary of State for Education, was the most important visitor and I had made a map indicating my various international collaborative research activities as the main motif of my display, with the Bangalore project as its centre piece. Very sharp eyes will recognize Vasudeva Murthy and Krishnamurthy in one of the photographs in Figure 7.

Post-retirement collaborations

The winner of the Sternberg award for 2011, Colin Murray Parkes, an eminent

psychiatrist, then 83 years old, was quoted as saying 'Life is too short for retirement'![19]. I whole-heartedly concur with this philosophy of life!

The Polish connection

A few years after my retirement from Birkbeck in 1990, I received an approach for collaboration from Poland. The writer was Krystina Brandt from the Polish Academy of Sciences in Zabrze. Brandt had heard me speak at several symposia, some in Czechoslovakia. I managed to obtain some European Union funding and a productive collaboration on phosphazene research developed (nine joint publications). I paid a visit with my family to their laboratory in 1996, Brandt and her all-female team visited us in London and everything was going swimmingly well until cruel fate intervened. Krystina died unexpectedly of a heart attack. Even more tragically, she had attained her life-time ambition in becoming a research professor only a few days before her untimely death.

The Turkish (Gebze) connection

This too reached its full flowering after my so-called 'retirement'. One of my former postdocs from the Gündüz group, Adem Kilic, had kept in touch and in the late 1990s, a collaboration sprung up, which greatly flourished over almost a decade. Kilic had become Head of the Department at a new Institute of Technology in Gebze and later on its first professor. More than 30 publications arose from this post-retirement activity of mine. I recall that at one stage Gebze reached the second place in the research rankings amongst Turkish universities. A second collaboration with another former postdoc, Zeynel Kiliç, started somewhat later at Ankara University, where he too had become a professor, ably supported by Selen Bilge.

Figure 7. Margaret Thatcher (M.P., Secretary of State for Education), with Shaw and Phillip O'Grady (Chemistry Technician) in the background. Photograph of the Bangalore project, London, 1973.

As a firm believer in gender equality [the late, great Rosalind Franklin (1920–1958) was a friend of mine at Birkbeck and Dorothy Crowfoot Hodgkin's (1910–1994, Nobel Prize 1964) signature graces my visitors' book], I am delighted to report that the whole of the collaborating team in Poland was female, as was the majority of the Turkish group. Gratifyingly, women generally are now taking their rightful places in science. As I read in my newspaper recently, it was announced that the three women science presidents in the UK (Lesley Yellowlees, President of the Royal Society of Chemistry; Frances Saunders, President of the Institute of Physics; Dame Nancy Rothwell, President of the Society of Biology) have stated that they will work together to help women reach the top in science.

For both projects, with Poland and Turkey, we again had crystallographic collaboration with Hursthouse and Simon Coles of Southampton University. Hursthouse's name on our joint publications appeared at least 49 times. I paid tribute to this very productive and long-lasting (more than 30 years) joint enterprise at his retirement celebrations at the crystallographic meeting at the University of Lancaster in 2006. It had started at Queen Mary College and involved him successively moving to Cardiff University and eventually Southampton University, from where he retired.

My final crystallographic collaborator, John Rutherford, also spanned quite a long timescale. He first appeared on my radar in 1971, when as a postdoc with Graham Bullen at Essex University, he co-authored a multi-authored and multi-centred paper[20]. Rutherford began to concentrate on theoretical problems, using his formidable mathematical skills to tackle and solve tasks such as disordered crystals, mixed crystals, etc. I learned about this when he paid me a visit at Birkbeck in May 1987. With his help and that of our Southampton crystallographers, we managed to get a satisfactory investigation and explanation[21] in 2007 of the observation which Leyla had made[17] in 1985 on $N_3P_3[O(CH_2)_3O]Cl_4$. It had taken us 22 years to get to this point of understanding!

Now we were at last in a position to investigate numerous other problems, probably arising from solid-state phase transitions. Alas, fate intervened again. Rutherford died and that together with other mishaps and manpower shortages (history repeating itself, we had experienced this before), put a stop, at least temporarily, to this promising line of research. Promising, interesting and possibly also *very useful* research. Why? Because in pharmaceutical formulation, knowing reliably which polymorph is being produced is essential, as different polymorphs may have different rates of becoming biologically available. Polymorphism may well be of potential interest in other spheres such as, for example, the food industry.

When medical researchers publish their results, they are usually expected to reveal any financial interests they have received from their sponsors. Having recently had articles (although of a historical nature) published in a medical journal, I like this custom and happily extend it to my field. I wish to declare that I have received no personal financial benefits (not a single pound, rupee, franc or Turkish lira) from my Bangalore, Toulouse or Gebze projects. However, my life has been immeasurably enriched culturally and I have made many friends. There were, however, some quite unexpected other benefits, a couple of which I will mention here.

In 1969 I met in Jaipur, Robert Heilig, a former Austrian refugee, a highly regarded professor of medicine there[22]. In 2010, I discovered that Heilig had been in the 1920s an assistant in Vienna to the famous Dutch cardiologist, Karel Wenckebach[23]. This stimulated my getting involved, together with my daughter, a young physician, in the history of medicine.

I had, however, totally unknown to me, set the wheels in motion for the greatest unexpected benefit! How did this come about? When in 1975, the first 5-year period of our grant was due to expire, both the Indian and British authorities were favourably disposed to its extension for a further 5-year period. The Secretary of the Indian UGC, Shankar Narayan, was keen to discuss some details with me. As he was due to visit UNESCO in Paris and had heard that I was invited as a Plenary Lecturer to a symposium in Besançon, he suggested for us to meet on my return journey via Paris

Figure 8. Reunion of 'The Lords of the Inorganic Rings', London, 2012. Standing (left to right): Dr Lily Shaw, Dr Sorab Contractor (Ph D, 1983), Mr John Gee (brother of late Wilfred Gee, Ph D, 1963), Dr Chandra Thakur (Ph D, 1970), Dr Leyla Shaw (Chandra and Jyoti Thakur's family), daughter-in-law Neha, grandson Arnav and son Saket, and Mr Michael Grayeff. Seated left to right: Mrs Nalini Contractor, Mrs Jyoti Thakur, Prof. Robert A. Shaw, Mrs Susan Gee, Mrs Gail Grayeff (M Phil, 1992).

at UNESCO. He must have spoken favourably about our project, as the UNESCO officials promptly invited me to become a UNESCO consultant. I accepted.

In the summer of 1977, I accepted an invitation to act as a UNESCO consultant to the Turkish Government to advise on the founding of new faculties of science and engineering. I arrived in August 1977 and after a short stay in Ankara was sent to Çukurova University at Adana. There I met the great love of my life, Leyla, a young physicist, who became a crystallographer. We married in 1980 and recently celebrated one-third century of a wonderful marriage, a true partnership in every respect!

Recently I was approached by a former co-worker about a joint research project. I told him that I had plenty of new ideas and I knew his scientific strengths and weaknesses. For an adequate modern research team in our field, we needed, however, experts with the necessary equipment and experience for separation science, NMR spectroscopy and X-ray crystallography. Without the necessary expertise and an enthusiastic partnership of experts, such a project would be, as far as I was concerned, a non-starter.

Our not so little group of international researchers has managed to keep in touch over the years. We have mini-reunions from time to time. My very first research student Stratton (now Emeritus Professor Cedric Stratton in Savannah, Georgia, USA) has coined a phrase for us; 'The Lords of the Inorganic Rings' (with apologies to J. R. R. Tolkien). The reunion of 2012 was held in London (Figure 8).

When I decided to write this essay I had three aims in mind. The main one was that it should be a tribute to my late dear friend, A. R. Vasudeva Murthy. I also wanted to put on record my many contacts with India and my regard and affection for the country and its people. Finally, I wanted to demonstrate to my fellow scientists the value of interdisciplinary and international collaboration. In my lifetime we have made enormous strides in all branches of learning. Specialization has become greater and greater, a trend which is particularly noticeable in medicine and the physical sciences. To tackle a problem adequately, increasingly we need a team of specialists, who must learn to collaborate and communicate effectively. I am applying this principle constantly to my own activities. Different teaching methods and different personalities will all produce slightly different seekers of knowledge. This variation of knowledge will help us to obtain a better, more complete and more rounded picture of our universe and the problems facing us. If we can unite our knowledge and skills, we have a better chance of finding satisfactory solutions and benefits for all of us!

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