

The four stages of speech as described in the Lalita Sahasranamam, one of the most ancient and sacred of Hindu scriptures, the 1,000 names of the Goddess Lalita Tripurasundari, which is set out in 183 verses.

परा, प्रत्यक्चिती रूपा, पश्यन्ती, परदेवता ।
मध्यमा, वैखरीरूपा, भक्तमानस हंसिका ॥ 81 ॥

Para, Pratyakchiti Rupa, Pashyanti, Paradevata
Madhyama, Vaikhari Rupa, Bhaktamanasa Hamsika ॥ 81 ॥

She is Para or the Transcendent Word, above the other lower stages
of speech known as Pashyanti, Madhyama, and Vaikhari.

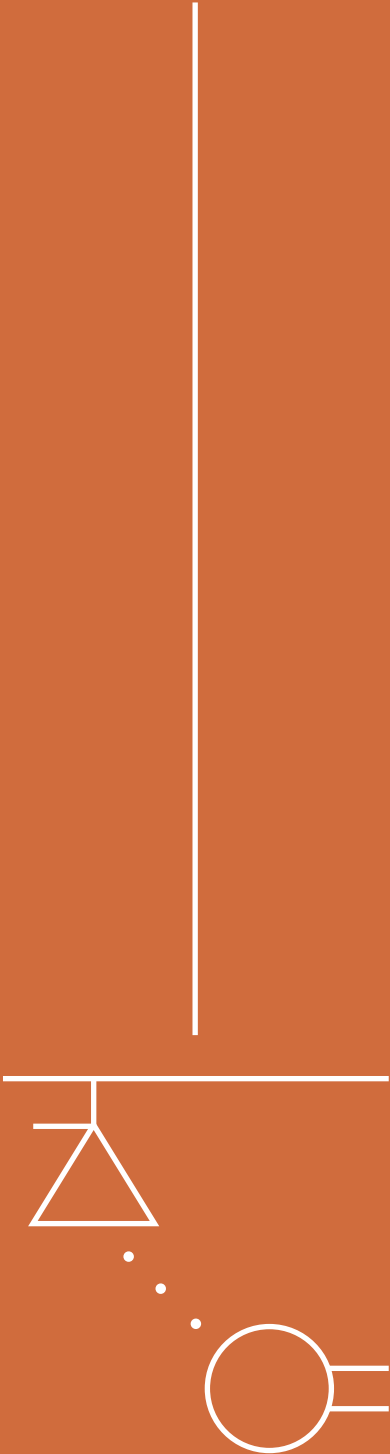
She is Pashyanti, or speech in the inaudible stage.

She is Madhyama, or speech in the middle stage of its external expressions.

She is Vaikhari Rupa, or the form of uttered, audible speech.

These four stages of speech have mostly impelled my feeling, thinking, conceptualization, and articulation of my scientific ideas. The entire process is discrete and not a continuum. In some cases, one can linger at a stage for a longer period of time.

PROLOGUE



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When the German Chemical Society (GDCh) asked me several years ago to write about myself and my scientific work, my initial reaction was to say that scientists don't write autobiographies. I felt and still do feel that the work of a scientist ought to speak for itself. Whatever they do appears in publications, books, talks, and even in conversations and interviews. I, as an individual, have been curiously detached from the corpus of my work—now something like 475 papers, three authored books, three edited books, and several general articles. I look at some of my papers dating from, say, the 1980s or 1990s, or even later, and ask myself, "Why did he even write this nonsense?" And when I do this, I invariably refer to the scientist Desiraju in the third person singular.

Another reason I hesitated to pen a few words about my scientific life, which spans close to 50 years now, is that I did not want too many details of my family and personal life to slip in: this was deeply worrisome to me. On the other hand, no one—I am not special in this regard—can separate one's personal and professional life into non-intersecting silos. One is the entire integrated individual; personal life will almost always influence professional life. I don't think the reverse was necessarily true, at least not in my case, or not to any notable degree. My

professional life has rarely influenced the important events in my personal life, suggesting that my inherent personality—whether determined by nature, nurture, or a combination of both—has uniquely shaped my work as a research chemist, independently of the various things that have happened to me professionally. The paternal and maternal families into which one is born and brought up within shape the entire personality of an individual in my view. Some, like me, are additionally lucky in the family they create as they go along, in my case, an enduring, non-linear combination of the above genetic and circumstantial factors, synergized by a loving spouse and loyal children. And once again, keeping with what I have just written above, one also creates an academic family, a subtle and minor variant of the family theme, but a family nevertheless, where the members do not have the rights, duties, and privileges of the personal family, but a family nonetheless in terms of the long-term associations that are formed and the feeling of academic oneness that includes those within the family and excludes those outside.

All of these things are deeply interconnected, and when Peter Gölitcz, who I have known well for 30 years, kept persisting in telling me that other chemists would like to learn more about how I arrived at my research problems—yes, it's almost always more important to ask the correct questions than to get the correct answers, for the simple reason that one never knows what the correct answer is—I kept telling him that even I sometimes didn't know why I kept going

after a certain problem. Something wasn't right, or something was nagging me, usually a crystal structure that didn't look as it was supposed to. But it was these strange crystal structures, the exceptions that defined new rules—the hallmark of chemistry—that led me on a deeply satisfying journey where chemistry and crystallography were intertwined so implicitly that they were hardly two distinct subjects anymore. I think I realized pretty early that what was happening to me was that I was travelling on a new road that few had taken before—a new subject called “crystal engineering.” Combining the accuracy of crystallography with its uselessness, to paraphrase Goethe, and coupling it to the delicious inaccuracy of chemistry—a subject of extreme utility—provided an excellent stomping ground for my academic excesses.

Another reason I held off for so long on writing anything remotely autobiographical was that the operational aspects of doing my science were determined in large measure by the fact that I was working in a country still finding its feet in the world of international science. I was an Indian who was well trained in America and was returning home voluntarily, unlike many of my compatriots, resigning a well-paying job, surrendering a difficult-to-obtain immigrant visa acquired at the age of 25, and, at the grand age of 26, going back to India without a job. I was unique. I was coming back to a wretchedly poor country that had innumerable problems that affected one and all. I knew I would have problems. I didn't realize at the time how bad these problems would be. There was a great

deal of difference between working in a so-called prestigious institution in India and the University of Hyderabad, newly founded when I joined it in 1979. The place was in the back of beyond, 25 km outside the metropolitan area, literally in the middle of hostile, arid terrain, and lacked even a reliable telephone connection for the first ten years I worked there—I need say no more. The locational isolation was unbelievable, and before long, I feared it would lead to scientific isolation. Needing to live close to the workplace, basically my laboratory, compelled us to reside on a campus that was bereft of basic requirements like schools, shops, offices, parks, and playgrounds. This difficulty placed a nearly intolerable burden on my young family.

Even more disturbingly, I found that I was deeply enmeshed in a regressive scientific system that, for socio-economic reasons that had overwhelmed India at the time, was controlled by gatekeepers who held complete control over most aspects of one's professional life and could make or break careers at will. Doing science without money is a non-starter. I had to pick and choose problems that didn't require too much funding because I wasn't going to get any. The way to arrive at a research problem was to avoid all that was impossible to do. My department in Hyderabad did not have a single crystal X-ray diffractometer, the workhorse of my subject, till 2002, long after I had clearly established myself as a scientist of the first international rank. I will say more about this in the book. Right from 1979, when I joined the university, I was very clear that, once back home, I would

not try to reproduce the scientific conditions I had experienced during my stay abroad—like some of my colleagues had, alas, attempted without success. It would have to be my own science, done in my own way, in my own country.

These were not fashionable positions to take, not in a country that was limping out of 250 years of debilitating colonial oppression and where the complete intellectual edifice of a great civilization had been destroyed to be replaced by a servile and docile mentality that excluded two of the most important attributes required in a successful scientist—curiosity and courage. And, at the end of the day, one is judged in international science on the absolute basis of one's work, not given latitude because one is working in a repressive system or in a backward country. What saddened me, however, is that most of my scientific colleagues abroad did not realize the very adverse conditions that an honest scientist in India had to labor under, especially at the high noon of the Indian gatekeepers—say the years 1980 to 2000—the years during which my major scientific contributions were made.

During the course of my career, and more so after the 1995 review in *Angewandte Chemie* that really made me as a scientist, I encountered a new kind of problem and one which, curiously, made me aware of my philosophical identity as an Indian, or more specifically, as a Bhāratīya, for now many of us in this country prefer to call it Bhārat, its 5,000-year-old name, rather than India, which is a superficial epithet concocted by our colonial masters of yesteryear. This

matter concerns the cognitive processes that influence the deeper movements of thought, and I feel it satisfies the suggestion made to me that readers of this book would like to know how I arrive at my research problems. Thinking can be either or both intellectual and intuitive. Intellectual thinking is logical, but this does not mean that intuitive thinking is illogical: it is supralogical. It follows from intellectual thinking, but there is a non-linear break where communicability suddenly vaults into certainty—it is so because it has to be so. I found I tended to unconsciously slide from the intellectual to the intuitive more often than not.

I could suddenly understand why Mendeleev did what he did and what Lothar Meyer did not, or should I say could not, when he predicted the existence of gallium and its properties before it was isolated. He was so sure of his model of the periodic table that he was certain gallium existed—it had to. Going further into Indian philosophy, our scriptures say that the nature of reality is *Tat tvam asi*, meaning “that thou art”. In the context of Mendeleev, one could reasonably say that the utter reality of his table created a situation in which there was no difference between Mendeleev the man and gallium the element. Mendeleev was gallium, and gallium was Mendeleev. This is non-duality or *advaita* as defined in our philosophy in Bhārat. In a popular podcast a few years ago, I specifically discussed Mendeleev and the fact that fault lines in human thought processes probably lie somewhere between Königsberg and Petersburg. Mendeleev was

thinking Eurasian rather than European, and Bhārat is part of the great Eurasian land mass, which is now emerging as one of the two power centers of the modern world. In making such a suggestion, I am only taking from Halford Mackinder's seminal 1906 proposal on the geographical pivot of history. Today, a century later, we see a situation in which Western Europe might well drift towards Eurasia, although not for the reasons Mackinder envisaged; this century may see a dipolar world with Eurasian and American spheres of influence, but these spheres need not be mutually adversarial.

All this and more became a reality to me when I realized that, for many Western scientists, especially those in my own field of crystal engineering and crystal structure prediction, it mattered more *how* you got a correct result than that the result itself *was correct*. I've often wondered whether Mendeleev was denied the Nobel Prize because the European establishment disliked the intuitive thinking that underpinned his model. Might they have been happier had Lothar Meyer waited patiently till all the chemical elements had been discovered and Moseley came along and identified atomic number rather than atomic weight as the true basis of chemical identity, and then proceeded to award him (Meyer) a Nobel Prize for logically deriving the table? Was Mendeleev guilty of scientific impropriety by reversing the tellurium–iodine order in his proposal?

Hydrogen bonding, another area of my research, saw more confrontations because I came up fair and square against the mighty

Linus Pauling, who had said hydrogen bonds were strong. I will not say that Pauling was wrong—he was just partly right. It seemed to me, as a chemist, that carbon could not have zero electronegativity to the extent that any C–H group in an organic molecule was just a waxy extension. In the area of weak hydrogen bonding, *I was just plain lucky—I was born at precisely the right time, not too early, so that the concept of weak hydrogen bonds was not useful enough, and not so late as that everyone knew most things about these feeble interactions*. Even here, I was involved in protracted and essentially useless discussions on the C–H...F–C beast and on whether it was a hydrogen bond, a dispersive interaction, or nothing at all—fluorine hating all elements when it was covalently bonded to carbon, it being so hard. I was pleased to have participated in an International Union of Pure and Applied Chemistry (IUPAC) committee that redefined the hydrogen bond, and our 2008 recommendation cleared many cobwebs, allowing the chemistry community to move beyond Pauling happily.

Coming back to this book, I was still not sure, even in early 2025, whether or not I wanted to write it. I was working for much of 2024 on a volume entitled “Fixing Science in India,” which is a survey of the scientific enterprise in my country since 1835, when the British switched our educational pattern from the original time-tested Indian Knowledge Systems, now popularly referred to as IKS, to an examination-oriented system designed to produce individuals who could fill lower-level clerical positions in the British

Raj. This book appeared in late 2025, and the basis for much of what I had to say there was, of course, my own experiences as a teacher-researcher in Bhārat, and, in this sense, it was indirectly autobiographical.

However, as I neared the end of “Fixing Science in India,” I became more comfortable writing a proper scientific autobiography for an international audience. This book, I felt, should be strictly about my scientific work and how I dealt with it, both as a player in the international chemical community and also as a member of the Indian scientific community. Above all, I was thinking about Peter Gölitz’s belief that readers might be interested in how I arrived at some of the (counterintuitive) ideas that went on to become established precepts in the areas of crystal engineering and weak hydrogen bonding. It is here that my Indian (more specifically, Hindu) roots come into play. I recollect a conversation with Jean-Marie Lehn in Strasbourg; I think it was sometime in the late 1990s when he remarked that only an Indian (read Hindu) could appreciate molecular sociology the way I did when I proposed the idea of the supramolecular synthon.

The subtitle of this book is “Designing Complexity.” *A crystal, I feel, is a wonderful example of a complex system in that its whole is greater than the sum of its parts, whether one speaks of its components, its formation, its evolution into a lump of macromatter, or its description either in terms of structure or properties.* Research is not something a scientist does in isolation. He interacts with students, coworkers, collaborators, colleagues, and other

individuals. All these interactions form part of a complex system, and very few important interpersonal interactions are truly binary: they tend to be multibody even when they are formally two-body. The final reason I have used the subtitle I have is that almost my entire body of research has been done in India, or should I say Bhārat, which, in my view, is the ultimate example of a complex system on this planet. My professional life has been a journey of unravelling crystal complexity in the many ways it manifested itself to me, and of trying to simplify it for the benefit of others.

Keeping within the scope of a scientific autobiography, this book consists of chapters that describe my early days, studies, scientific and extrascientific work, and a round-up of my professional life so far. My research has been based on two themes: crystal engineering and weak hydrogen bonding. Bhārat is now rapidly ascending in the comity of nations and is projected, along with America, China, and Russia, to become one of the new empires in the emerging new world order. No one today seriously believes that India is a land of snake charmers, tigers, and the Taj Mahal. Bhārat will soon become the third-largest economy in the world in purchase power parity terms (PPP). In such a scenario, it may also be relevant for the international chemical community to observe, perhaps through this book, how a scientific system and a scientific career evolve in a rapidly changing world, at least as visualized by the author.

This book is a spontaneous and informal account of my scientific life and about my

See page 10.

Fig. 0.1.
Campus of the
Indian Institute
of Technology
(IIT) Mandi
located in the
Kamand Valley
of Himachal
Pradesh in
northern India.

understanding of chemistry, a subject that had me hooked when I was 14 years old and has fascinated me ever since. It does not attempt to be a comprehensive chronology of events. Like most of my books, scientific or non-scientific, the actual writing of “Crystal Engineering” was performed relatively rapidly, and at least a third of the textual matter was written during a week’s sojourn on the beautiful campus of the Indian Institute of Technology (IIT) Mandi, nestled in the Himalayan foothills of Himachal Pradesh in northern India (**Fig. 0.1**). I was there for the international “Mind, Brain, and Consciousness Conference” in the context of IKS in June 2025, and as I keep travelling along my life’s journey, I see less and less contradiction between my activities in various spheres—personal and professional, scientific and non-scientific, domestic and international, Indian and Bhāratīya—that I had hitherto felt to be distinct. The only common factor in all these spheres is the individual himself, and this is why this book attempts to call itself an autobiography.

