

The Vinod Wadhawan Blog

Celebrating the Spirit of Science and the Scientific Method

Home

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DOING SCIENCE IN INDIA. A PERSONAL NARRATIVE (Part 2 of 3)

7. AT THE UNIVERSITY OF OXFORD (1979-1980)

I stayed at a Guest House in the Oxford University campus till Mike could find me a house on rent. And he had a tough time doing that. He rang up a number of people, and many of them agreed initially. But when they were told that I am an Indian, they declined, giving some excuse or the other ('Smell of Indian curry', etc.). Mike was very angry, and told me that these same people enjoyed going to Indian restaurants for the curry. Finally, he arranged that the college where he was a teacher (Jesus College) would give me an apartment from its married-students quota.



Winter came, and Mike was very amused when I told him that I was seeing snowfall for the first time.

There were some other firsts too for me. One was the quaint British sense of humour. Prime Minister Thatcher was rather tough in dealing with the trade unions. But on one occasion she relented, and the opposition described it as a

climb down. She said something which I cannot recall, but the response from an opposition leader went something like this: 'When one is climbing down, the view from below is not particularly appetizing'!

Once Mike took me to some social gathering, where lots of cream and strawberries were being consumed. He pointed out to a very old person who tended to keep his mouth open, and had no teeth left. Mike said: 'Look Vinod, that old chap looks as if he is already dead'!

My apartment was on the ground floor, and there lived an American couple on the first floor (no idea whether they were married or not). The way apartments are built in many Western countries is not what we normally have in India. Ours are RCC structures all through, with thick slabs separating the floors. But I noticed that in Oxford (and also in some other cities abroad I have lived in since then) the ceiling of one apartment, which is naturally the floor for the apartment above, is not very thick (it is a wooden structure), and far from being sound-proof. My problem was that when I tried to sleep sometime before midnight, I was woken up by a certain hammering cum battering cum screeching cum grazing sound, coming through the ceiling. I thought it strange that somebody upstairs indulged in some carpentry-like activity, so late in the day. But soon it dawned on me that it was something else altogether. The couple regularly made love at this hour, before falling asleep, and the sounds which reached me were the protestations and the creaking of the bed under them. Very unprecedented situation indeed! I could get sleep only when they were asleep after the daily chore. Anyway, they left the place after a few weeks and normalcy was restored in my routine.

On joining Mike's group I made a courtesy call to the Director of the Clarendon Laboratory. He also had a senior Chinese visitor at the same time, who had brought him a gift. The gift reflected the Chinese value system regarding science and technology. It was a large and very transparent disc of the electroceramic PLZT. I was very impressed by the contrast with India. Indian scientists carry (or used to carry in those days) gifts like a silk scarf or a necktie, or the Bhagwad Gita, or some piece of handicraft. Here was a Chinese scientist taking pride in a technological achievement of his laboratory, and showing it off!

The atmosphere in Mike's group at the Clarendon was pervaded with crystallography, and I soaked in a lot of it. Mike himself is a crystallographer par excellence. Among many other things crystallographic, he served a term (or perhaps two) as President of the BCA (British Crystallographic Association). Once they had a visitor, Helen Megaw. I told her that I had read her book

Crystal Structures, A Working Approach, and found it very instructive. She was pleased, but also said that she was disappointed that it did not go into a second edition. Unless your book goes into a second edition, you have probably not done a very good job writing it.

At the Clarendon I also came across Prof. H. M. Rosenberg, whose name was familiar because of his book on low-temperature physics. He used to come to the lab on a bicycle. We ran into each other often, but the remarkable coincidence was that on at least 90% of the occasions we saw each other in the men's room only!

People in Mike's group were nice, except for one obnoxious character. Among other things, he was peeved by the fact that the Nuffield Foundation was paying me such a decent amount as Fellowship. He made his perceived British sense of superiority a bit too obvious. Once, in a heated discussion, I ended up asking him: 'What is colonialism if not international banditry'?



[With Mike's group at the Clarendon (1979)]

During my tenure at the Clarendon I learnt about the 'rotating-analyser method' developed by Mike and coworkers for making ultra-sensitive measurements of the birefringence of crystals. We used this technique for investigating the ferroelastic phase transition in the solid-solution series lead phosphate-vanadate (Wood, Wadhawan & Glazer 1980; Wadhawan & Glazer 1981).

I met Bob Newnham for the first time when he came to the Clarendon as a visitor. It turned out that he was the referee for my paper on the ferroelastic effect in orthoboric acid (Wadhawan 1978). He told me that, after reading my manuscript, he had put one of his graduate students on the task of preparing a polycrystalline ceramic of orthoboric acid, though that effort turned out to be unsuccessful. I told him that I was looking for a post-doctoral position after my tenure at the Clarendon ended. He agreed readily, and offered me a three-

year position at Penn State. I accepted, but I had to return to my job at BARC after only one year at Penn State because my absence from the job could not be for more than two years in all, and I had already spent a year at Oxford.

So, in mid-1980 I and my family moved to the Pennsylvania State University. But before that, while still at the Clarendon, I went to Sicily for a Summer School on Ferroelectrics.

The Summer School at Erice in Sicily gave me a lot of material for my book on ferroic materials (Wadhawan 2000). I also got to meet many of the prominent names in the field of ferroelectrics: A. M. Glass, J.-C. Toledano, Jan Fousek, L. E. Cross, and many others.



[At the Summer School on Ferroelectrics, Sicily (1980).]

The lectures by these people gave me insights into the stuff ferroic behaviour is really made of.

8. THE PENN STATE DAYS (1980-1981)

Life at the Materials Research Laboratory (MRL) of the Penn State University was an enriching experience. The Director at that time was Dr. Rustam Roy. I learnt a thing or two from him about how to give lectures such that the audience feels totally involved. His approach was to make the whole thing very very interactive.

The group led by Leslie Cross and Bob Newnham at the MRL carried out great research work on ferroelectric ceramics and composites. Cross, Newnham, Barsch (a theoretician), and a couple of other professors (each with a different specialization), collectively led a bunch of ~25 juniors: post-docs, Ph.D. students, and graduate students. There was a weekly seminar, and the best thing about it was that the seniors shared with us the details of the research programme for the next few years. The goals set were explained very clearly

to the whole group, something I had never experienced at BARC. In fact, when I returned to BARC in 1981 I remarked that I am better informed about what the Penn-State team is going to do in the next few years than what we are supposed to be doing at our Crystallography Group of BARC.

In those days there was considerable debate in India about excellent research *versus* relevant research. Bob Newnham showed me how one can do research which is both excellent *and* relevant. His group did '*targeted* basic research'. For example, they promised to the funding agencies that they would develop miniaturised capacitors and transducers. And their approach for doing this was to develop new configurations of composites based on proper and improper ferroelectric materials. And *basic research* on ferroelectrics was geared to meet the same objective. They ended up doing the best and the most innovative research in this area. Of course, they went all the way, making devices which actually worked. The best thing was that a part of the research was purely academic, with no immediate applications in mind. It was thrilling to watch this at close quarters.

I saw another interesting thing during my Penn-State days, namely a visit by American car manufactures to the MRL, the purpose being to tell the scientists about some pressing problems in the auto sector which called for some innovative solutions. At the other end of the spectrum, there was also a visit by a hard-core group-theory expert, Dan Litvin. He had come looking for some common area of interest between him and the work being done in our ferroelectrics and materials science group. This visit led to a lasting friendship between him and me. He listened with great interest to my description of what ferroic materials and ferroic phase transitions are all about. He went back and started a programme focussed on the symmetry aspects of ferroic phase transitions (Litvin 2010). We have also done some joint work since then (on latent symmetry), but more on that later.

Like at the Clarendon laboratory in Oxford, here also there was an obnoxious character in the group I worked in, but this time a very senior person. He was an American whose ancestors had migrated rather recently from England. I found his haughty attitude quite offensive, and had some difficulty keeping my cool. But Bob Newnham made up for all this, just by being what he was.

Bob was an **expert** in many areas of condensed-matter physics, the common thread being his passion for structure-property relations in materials, and his keenness to exploit the knowledge of structure-property relations for the benefit of mankind. He once said to me: 'Vinod, the challenge is to make those little atoms and molecules work to our advantage'.

An immensely innovative scientist, he was also a great teacher and a marvellous speaker. I happened to be present at a couple of lectures he gave at major conferences. Of course he did use Powerpoint, but had the self-imposed restriction that he prepared each lecture afresh, and not by just rearranging the already existing slides in his stock. That brought freshness, topicality, and spontaneity to the lectures. Plus there was the awe he inspired in the audience by presenting so many new results and ideas. When a lectures ended, there was the usual applause, but with one difference. People not only got up and clapped, but went on clapping for surprisingly long times. On one occasion I timed it casually; it was more than four minutes. And this happened after just about every public lecture he gave. To get a feel for the contents of such lectures, see Newnham (1999).



[This picture with Bob Newnham was taken, not at Penn State, but at the 6th International Meeting on Ferroelectricity (1985) at Kobe, Japan.]

Bob Newnham was full of new ideas, and had a keen innate sense of what technology is going to bring for us in the future. At a personal level, he also gave me a glimpse into what the life of a dedicated scientist should be like. At work he felt no need to compete with others for the top job and become a boss and get all the appurtenances that come from being one. He just enjoyed being a scientist. He enjoyed teaching also. He had some interesting hobbies, and I got a glimpse of them when he invited us home for dinner. There was a permanent and genuine smile on his face which comes when you are completely at peace with yourself and are completely happy with life. Here was a role model for me, and not only from the vantage point of science as a profession.

During my stay at Penn State I worked on two research problems: Demonstration of the ferrobielastic effect in quartz crystals (Laughner, Wadhawan & Newnham 1981); and demonstration of the shape-memory effect in PLZT ceramic (Wadhawan, Kernion, Kimura & Newnham 1981).

Every three months (or was it six months?), some senior person from the group went to Washington to present to the funding agency the progress report. Once he carried a report which included the work I had done on the ferrobielastic effect in quartz. I had designed and fabricated a press small enough to sit on the stage of a polarizing microscope. With it I could use a tiny crystal of quartz for the experiment, and the requisite uniaxial stress could be generated just by turning a screw by hand. Earlier somebody had used for this purpose a huge press normally employed for high-pressure studies. The advantage of my device was that, since it sat on a polarizing microscope stage, one could easily obtain microphotographs of the ferrobielastic twinning introduced in the quartz crystal. The person at the monitoring agency in Washington was very impressed, and sent a message that he wanted to meet me. The person who brought the message gave enough hints that the reason for the invitation was to offer me a permanent or semi-permanent stay in the USA. I was just not interested, and did not go to meet him.

Near the end of my year-long stay at Penn State I and Bob Newnham drove to Ottawa to attend the XII IUCr Congress (1981). I gave an invited talk here. The title was 'Ferroelastic Phase Transitions'. This was the first invited talk I gave in my career. The irony and the tragedy was that it was given in an *international* conference, rather than in a national conference (as is usually the case). And it was a near-disaster. I had prepared very hard for it. I even bought a tape recorder, and rehearsed and recorded the talk several times, so that I would know how much I could fit into the 30 minutes allotted to me. The content of the talk was nice, but what went wrong was that, in my nervousness, I spoke too fast, so the talk ended well before the time I had at my disposal. In view of the fact that several parallel sessions were being held at the conference, the Chairman of my session had to announce a five-minute break before inviting the next speaker. And the irony is that the big boss from India, who was responsible for this lack of experience of mine, was sitting in the audience.

9. BACK TO B.A.R.C. (1981-1990)

I was back at BARC in mid-1981, full of confidence and new ideas. But nothing much had changed at BARC, so there was bound to be trouble for me. Anyway, the first major thing I did on return was to write a major review article entitled 'Ferroelasticity and related properties of crystals' (Wadhawan 1982). This turned out to be a game changer for my career. All those endless hours spent in the BARC library lapping up papers by the reclusive K. Aizu (originator of the concepts of ferroelasticity and ferroicity) were duly rewarded. My

article was the first comprehensive review of the subject, and I think it helped in attracting many physics people to this field. My name became known internationally among people working in this field.

Once the big boss returned from a conference abroad, and he was talking to us about it. He said, 'Wadhawan, they were talking about your review article'. I was thrilled to hear that, and responded in some incoherent manner not typical of me. He said 'Well, they were not abusing you!' There was pin-drop silence for a while. I said nothing, but could not help wondering what had made this man, who had mastered the art of carrying a frozen smile on his face all the time, feel so flustered? Frustration, that here was a young upstart who could stand on his feet without his consent and bolstering?

There was pressure on me to devote my time to technique-based research, whereas I was determined, more than ever before, to first choose a research problem and then apply whatever techniques were needed to make progress. Another major cause of friction with the big boss was that, influenced by Bob Newnham's ideas about doing 'targeted basic research', I questioned the wisdom of doing, say, only neutron-scattering based research simply because the large number of instruments set up in the reactor hall had to be kept busy. I was snubbed and insulted in meetings if I dared raise the issue of relevance of a piece of research carried out by anybody in BARC. I think this value system was not confined to BARC alone, and was a national problem, at least in those days. No wonder our system has not been producing Bob Newnhams in large enough numbers.

Acoustical ferrogyrotropy, or ferroacoustogyrotropy

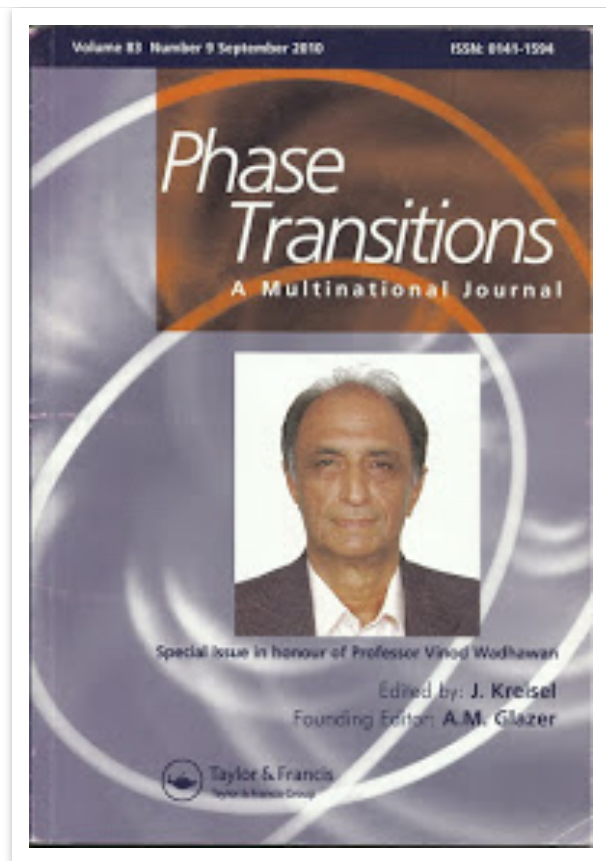
Anyway, I tried to do research on problems which interested me, but by factoring-in the constraint that no worthwhile equipment would be available to me. For example, I introduced the notion of ferroacoustogyrotropy (Wadhawan 1982), by analogy with optical ferrogyrotropy mentioned earlier in this narrative. Acoustical activity arises from the spatial dispersion of the elastic-stiffness tensor, and is described by a fifth-rank tensor. I collaborated with my friend Keshav Bhagwat, a brilliant mathematician, for an analysis of this tensor property. It has a certain intrinsic symmetry, which we used for introducing a lower-rank (fourth-rank) tensor which is completely equivalent to the fifth-rank tensor for describing the acoustical activity of crystals. A similar tensor of rank 4 had been introduced earlier in the literature, and our detailed analysis showed that there were serious errors in the formulation

and analysis of that tensor (Bhagwat, Subramanian and Wadhawan 1983; Bhagwat, Wadhawan and Subramanian 1986).

The concept of pure-mode axes exists in crystal physics in the context of propagation of acoustic waves in crystals. For a direction to be a pure-mode axis, the longitudinal-polarization component of the acoustic wave must be independent of the transverse-polarization component, and vice versa. An acoustic axis is a degenerate pure-mode axis. In our work we introduced the term 'pure acoustical activity' for describing the acoustical activity of a crystal along an acoustic axis. The concept of 'pure ferroacoustogyrotropic state shifts' was also introduced and analysed, and a total of 29 ferroic species were identified in which such state shifts can possibly occur.

We also showed that, like optical ferrogyrotropy, acoustical ferrogyrotropy also is only an implicit form of ferroicity, meaning that state shifts involving change of acoustical activity can only be effected via a concomitant explicit ferroic property. For example, such state shifts in quartz can only be effected through ferroelastic or ferroelastoelectric state shifts.

Mike Glazer was the founding editor of the journal *Phase Transitions*, begun in 1978. In 1985 he invited me to be the Regional Editor for India for this journal. I was happy to accept the invitation. Thus started an association with this journal which lasted for the next 25 years. In 2006 I took voluntary retirement from the task of Associate Editor of the journal, so as to concentrate entirely on book-writing work. I was asked to continue for some more time so as to ensure a smooth transition. They brought out a special issue of the journal in my honour (cf. Glazer & Rolder 2010).



In Indian science, Editorship of journals is supposed to be done by big bosses, who have due secretarial support for this kind of work. I was too young, and nowhere near being a big boss, so my task was harder. Anyway, this was one more challenge I accepted, and if the remarks by the then Editor-in-Chief are any indication (cf. Glazer & Rolder 2010), I did a good job of it.

Lack of adequate secretarial support was not the only difficulty I faced. There were other problems too. For one, this journal is a purely commercial venture, with no support from some academy or society (unlike, say, Physical Review which is run by the American Physical Society). Another difficulty was regarding the availability of sufficiently motivated referees. If some big boss is the Editor, referees would oblige easily, for obvious reasons. Not so in my case. I solved this problem by myself becoming one of the referees for most of the papers. This entailed doing an adequate amount of reading in the field of the paper I was processing. Liquid crystals was a good example of that, as I received many manuscripts from this field. As time passed, I acquired a good working knowledge of what phase transitions in liquid crystals are all about, even though I had never intended to do research on phase transitions in liquid crystals. The same became true about many other fields. As a result, 25 years of editorship work has given me a lot of breadth (though not a matching amount of depth also) in various aspects of phase transitions and physics. Moreover, this personal touch from the Editor is possible only in theme journals like mine, and I noticed its absence, for example, when I submitted papers to, say, Physical Review. I have published three papers in that journal,

and was not at all impressed by the choice of referees or the quality of refereeing. It is clear that its editors choose from a large data base of referees, and then depend excessively on what the referees pronounce about a manuscript. The whole process is a bit too mechanical. In the case of my journal I often overruled the referees, simply because of my better familiarity with phase transitions.

Most of the papers submitted to me were from India (till I was upgraded to the Associate Editor position), so I got a good ringside view of the strengths and the weaknesses of some aspects of Indian science. I was rather ruthless in rejecting bad papers, but often I felt that the basic work is not bad, but has been presented badly. Use of bad English by the authors was of course a constant headache for me, and in quite a few cases I ended up rewriting substantial portions of a paper. I also felt that the blame in such cases lay with the senior author.

In 1987 I went to Perth to give an invited talk at the XIV IUCr Congress.

In 1987 I also started working on the high- T_c superconductor Y-Ba-Cu-O. I and my colleagues demonstrated that it is a ferroelastic (Wadhawan 1988; Somayazulu, Rao & Wadhawan 1989), and exhibits the shape-memory effect (Tiwari & Wadhawan 1991).

Latent symmetry

One book that I found absolutely fascinating was Shubnikov & Koptsik's (1974) 'Symmetry in Science and Art'. I read portions of it several times. Obsession again? Yes, and it paid off again. I ended up discovering something significant, and I called it 'latent symmetry' (Wadhawan 1987).

There is this well known Curie principle of symmetry. It says that when several phenomena of different origin are superimposed in one and the same system, the symmetry elements which survive in the composite system are only those which are common to each phenomenon taken separately. Naturally, the symmetry of the composite system cannot be higher than the symmetry of each phenomenon taken separately (but see below). So there is a lowering of symmetry, and Shubnikov & Koptsik (SK) called it a process of 'dissymmetrization'.

Can the composite symmetry ever be higher than this lowest common

symmetry? The answer is yes, and the term 'symmetrization' was used by SK for describing such a situation. If the conditions are just right, such a possibility can occur in those cases wherein the phenomena or objects superimposed have equal or equivalent symmetry.

SK just left the matter there and moved on. But I was stuck. I kept wondering where this extra symmetry came from, after we had taken due note of the individual symmetry of each component and of the symmetry introduced by us for making the composite system in a given special way. It turned out that, in certain cases, the final symmetry was more than what could be accounted for like this, and I called it latent symmetry. A simple example (my favourite) is that of two equal right-angled isosceles triangles which when juxtaposed (in two dimensions) so that they share their long side end up becoming a square. This composite object (square) has, for example, a 4-fold axis of symmetry which cannot be accounted for by all the book-keeping you can do, so I said that it was lying as a latent symmetry in the two triangles and became manifest when the two triangles were juxtaposed in a certain special way.

Soon after my paper on latent symmetry was accepted for publication (Wadhawan 1987), I got a note from Bob Newnham, thanking me for introducing him to the idea of symmetrization. This meant that Bob was the anonymous referee for my paper. I was impressed. He was under no obligation to make it known to me that till he read my manuscript he did not know about the possible existence of symmetrization (and had thought that the Curie principle entailed dissymmetrization always). But he did. This is the kind of intellectual honesty and intellectual humility every person in this noblest of professions called science must have.

Bicrystals

One reason why I was fascinated by the discussion of symmetrization in SK's book was my interest those days in bicrystals. Any two crystals sharing an interface constitute a bicrystal. For a group-theoretical analysis of the symmetry of bicrystals it is convenient to take the interface as planar, and a bicrystal is formally defined as two semi-infinite crystals sharing a planar interface. Pond & Vlachavas (1983) wrote a great and exhaustive paper entitled 'Bicrystallography', and I read it again and again. Later on, when I was to formulate a comprehensive classification of twinning in crystals, I took due note of the ideas from bicrystallography, something not done in any of the earlier classification approaches.

Soon after the paper by Pond & Vlachavas, there was a paper by Vlachavas

(1984) in which two theorems about bicrystallography were proved. One said that, given a two-component composite $\{A, gA\}$, where the component A is of point-group symmetry F , the order of the point-group symmetry of the composite is $2/k$ times the order of the group F , where k is a positive integer. The second theorem (a corollary of the first theorem) said that the lowest order of the composite point-group symmetry is 2 times the order of the group F . To me this felt wrong. An obvious counter-example I thought of was that of the square (mentioned above) constituted by juxtaposing two equal right-angled isosceles triangles. For it, $F = \{1, m_{\text{sub}x}\}$, a group of order 2. The composite (i.e. the square) constructed from it by the isometries $\{1, m_{\text{sub}y}\}$ has the symmetry $G = 4_{\text{sub}z} m_{\text{sub}x} m_{\text{sub}y}$, a group of order 8. But the theorem by Vlachavas wrongly predicts this order to be either $(2/1) \times 2 = 4$, or $(2/2) \times 2 = 2$. And this error occurred because Vlachavas was not aware of something called latent symmetry.

Another example of the non-validity of the theorem was given later in Litvin, Wadhawan & Hatch (2003).

Enunciation of the symmetry composition principle

The recognition by me that latent symmetry exists has enabled me to enunciate a comprehensive 'symmetry composition principle' (Wadhawan 2011). I have stated this principle as follows: The existence of symmetry generally implies the coexistence of two or more equal or equivalent components or building blocks, and the overall symmetry group is then EITHER the product of the symmetry group of the building block and the placement-symmetry group which describes the mutual placement of the building blocks; OR it is a larger group because of the presence of latent symmetry.

The series of symposia called International Symposium on Ferroic Domains (ISFD) was started in 1989, and I was one of the founding members of its International Advisory Board. The first symposium in this series, ISFD-1 was held at Volgograd in 1989, and I was invited to give a talk in it, apart from chairing a session.

On my way back from Volgograd, I halted for a month as a Visiting Scientist at the Institute of Physics, Prague. Here I worked with Vaclav Janovec. I got a chance to have a close look at the work done by him on the symmetry aspects of ferroic domains and domain walls, and covered it in substantial detail in by

book on ferroic materials (Wadhawan 2000). Another dear friend I met at Prague was Vojtek Kopsky, mentioned above in the context of optical gyrotropy.

Fed up with the lack of funds and other support needed for doing anything substantial, I took one of those snap decisions I am well-known for. I decided to leave BARC, and ask for transfer to another unit of the DAE, namely CAT (Centre for Advanced Technology), Indore. This entailed great personal sacrifice, particularly regarding the education prospects of my daughter and son.

The big boss's reaction was remarkable. He kept saying that if I want to go, he is not going to stop me. And he also made, on the side, what he thought was a truly magnanimous offer. He said he would put me in charge of an X-ray diffractometer (which I was to run after it was purchased and set up), and that I would be in 'control' of that facility! Very revealing indeed. His way of punishing me for having a mind of my own had been to make sure that I remained an unimportant person, not in 'control' of anything substantial.

After a few days a senior person close to the big boss came to my room and offered to make me head of a certain Section under his Division, after the present incumbent retired. Anyway, since the big boss had kept saying that he is not stopping me if I want to go, I latched on to that statement at its face value, and thanked him profusely for that, and started packing. I was so happy that I could escape.

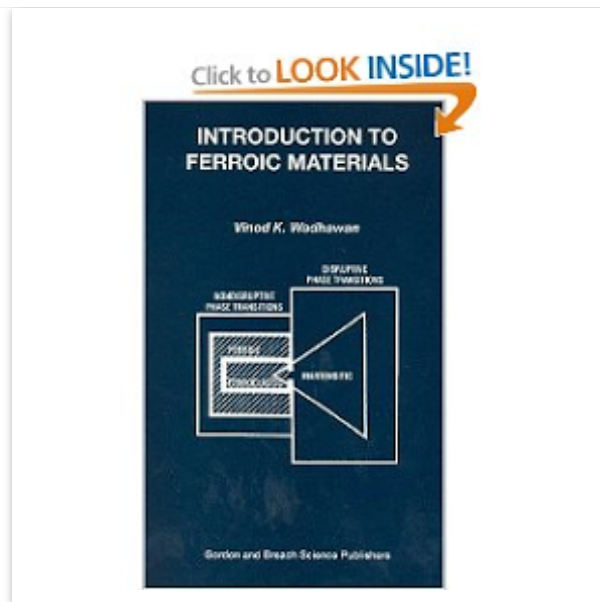
10. AT THE CENTRE FOR ADVANCED TECHNOLOGY (CAT), INDORE (1991-2004)

CAT has two major research programmes: development of accelerator technology and development of laser technology. My initial mandate was to set up a crystal-growth laboratory for work on crystals of interest to the laser programme. In view of my interest in ferroic materials the Director also let me initiate research on polycrystalline ferroelectric ceramics. In due course this part of CAT under my charge was named the Laser Materials Division (LMD). Another important activity I added later was setting up of a major facility for fabricating high-optical-quality polycrystalline domes of ZnS by CVD (chemical vapour deposition), for use in target-seeking missiles.

Ours was a new research centre, and mine was an even newer laboratory in it, which meant that one had to start from scratch for anything one wanted to do. No laboratory building, no equipment, nothing, to start with. At some stage funds were sanctioned for the LMD to have a building of its own. I can say that I had to build everything, brick by brick.

In 1991 I received an invitation from Prof. Claude Boulesteix to spend a couple of months as a Visiting Professor at his laboratory at Aix-Marseille, France. This was a very interesting visit for me. French science and technology is, of course, very impressive. The French are also very good at not being nosey about what people around them are doing, so long as no nuisance is created. Their appreciation and love for the female form is legendary. I still remember a hoarding I saw there, which showed nothing more than a bare female leg, the bottom two-thirds of it, in an extended horizontal posture. I have seldom seen anything so sensuous as that.

Around 1992 I started work on my massive book on ferroic materials (740 pages), and took almost seven years for completing it (Wadhawan 2000). Much of this work was done in the evenings and on weekends. It was, and still is, the only comprehensive book on the subject. It was one of the most fulfilling activities I ever undertook. Just imagine. Seeing so many gaps in the new subject, particularly regarding formal definitions and classifications, and providing these definitions and classifications. Giving credit where it was due. Covering everything from basics to theory to applications. It was fun, so I never felt that I was working hard.



The extensive library work I did on ferroic materials made me aware of another exciting field of research, namely *smart structures*, one reason for this being that ferroic materials find extensive applications in smart structures. After ferroic materials, smart structures became my next *obsession*. I ended up writing a book on them, with the title: ‘*Smart Structures: Blurring the Distinction between the Living and the Nonliving*’ (Wadhawan 2007). In this book I introduced, among other things, the properties of ‘superelectrostriction’ and ‘superpiezoelectricity’, by analogy with superelasticity in physical metallurgy.

For both ferroic materials and smart structures, I was ahead of my times in Indian science.

In the book on ferroic materials I had not covered what became later a very important field of investigation, namely multiferroics. I made up for that by discussing them extensively in the book on smart structures, particularly because of their great potential for use in smart structures. One of the multiferroics we investigated at CAT was the relaxor ferroelectric PMN-PT(70/30) (Wadhawan, Pandit & Gupta 2005; Pandit, Gupta & Wadhawan 2006).

In 1996 I visited Vienna to attend ISFD-4. I was an invited speaker there, as also a session chairman.

My NSBT (or tensor) classification for twinning in crystals

As a crystallographer I was naturally exposed to the occurrence of twinning in crystals. I also learnt that there were two major works of classification of twinning, one by Friedel and the other by Donnay & Donnay (see Wadhawan (2000) for references). I felt that these classifications were not comprehensive and informative enough, and kept thinking about formulating a better classification scheme. Finally I came up with the NSBT (or tensor) classification scheme, which is all-inclusive and informative (Wadhawan 1997; also see Wadhawan 2000). The information part is contained in the unique symbol I introduced for labeling the twinning in any crystal.

I argued that twinning is determined by the crystal structure, and the crystal structure is properly described by the space group of the crystal, so a good classification of twinning must work at the space-group level. Actually, one has to work with two space groups, one the actual space group of the component crystals of a twin, the other a real or hypothetical prototype space group. The latter has at least one additional symmetry operator, which maps one component of a twin to another. Without going into too many details here, I come to the end result of such considerations:

All twins must be one of two types: T-twins (T for translational) and rotational twins. Components of a T-twin do not differ in any tensor-property coefficient, and in rotational twins at least one tensor property coefficient is different across the interface.

Rotational twins can be of two types: B-twins and Aizu twins. For B-twins a prototype structure cannot be defined. For Aizu twins such a prototype is always definable.

Finally, Aizu twins can be either N-twins or S-twins. Components of an N-twin have zero 'relative spontaneous strain', and it is nonzero for S-twins.

Thus all twins can be divided into four fundamentally different classes: S-twins, N-twins, B-twins, and T-twins. Hence the name 'NSBT classification of twinning'. It covers everything: transformation twins, growth twins, mechanical twins, bicrystals.

The symbol I introduced for twinning consists of one of the four letter S, N, B or T, followed by one or more lower-case letters in brackets which represent the tensor properties in which the twin components differ.

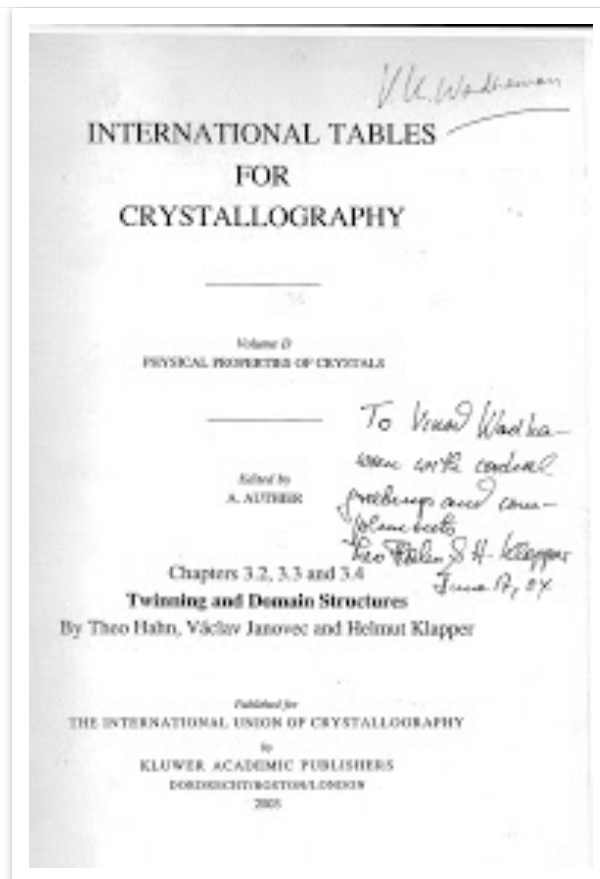
Take the case of Dauphine twins in alpha quartz. The crystal is ferrobielastic as well as ferroelastoelectric, meaning that the twin components differ in at least coefficient of the compliance tensor and the piezoelectric tensor. Accordingly the symbol I assign to this twinning is N(d,s); here d denotes the fact that the two components of the twin differ in at least one piezoelectric coefficient, and s is the corresponding representation of the difference in the compliance-tensor coefficient(s).

In contrast to this, the Brazil twins of quartz are growth twins, with a mirror operation parallel to the optic axis as the twinning operation. Unlike the case of Dauphine twins, this type of twinning does not disappear on transition to the beta phase on heating. My symbol for this twinning is B(g), with g denoting the fact that the twin components differ in the optical gyration tensor (optical activity).

Unlike my description, the classification scheme of Donnay & Donnay is not able to make a distinction between Dauphine twins and Brazil twins of quartz. Both come under the same category, namely 'twinning by TLS' (TLS = twin lattice symmetry).

Similarly the twin individuals in ammonium chloride crystals differ in the sign of a piezoelectric coefficient. But neither the description 'twinning by merohedry' (Friedel), nor 'twinning by TLS' (Donnay & Donnay) conveys any information about this fact. In the SNBT classification the symbol in this case is N(d).

Volume D of the International Tables for Crystallography (2003) deals with the physical properties of crystals. I counted 23 citations to my work in this volume (referring to my work on twinning, ferroic domains, ferroelastic properties, etc.). No other Indian crystallographer has his work cited such a large number of times in these Tables. I am mentioning this (as well as many other trivia in this narrative) because later on I shall comment on what it takes to be a Fellow of the Indian science academies.



In 1999 I organized and chaired a 'Microsymposium on Ferroic Structures' at the XVIII Congress of the IUCr at Glasgow.

In my book on ferroic materials (Wadhawan 2000) I had introduced the new notion of *latent symmetry*. When I told my friend Dan Litvin about it, he not only recognized it as something new and important, but also gave it a formal group-theoretical footing (Litvin & Wadhawan 2001, 2002).

In 2001 I received an invitation from Guillermo Castellanos-Guzman to spend some time in his laboratory at Guadalajara, and give lectures in his laboratory and also at Mexico City. Guadalajara is at the other end of the Earth from Indore, so I wangled an invitation from Prof. Dorian Hatch of the Brigham Young University, Provo, Utah, to spend a week in his Department, on my way to Mexico. Dan Litvin thought this would be a good opportunity to do some more brain-storming on latent symmetry, so he flew down from Pennsylvania to Utah, and the three of us made some more progress in understanding latent symmetry (Litvin, Wadhawan & Hatch 2003).



[With Dan Litvin.]

In 2002 I attended ISFD7 at Toulon, France, and gave an invited talk, apart from chairing a session.

2004 was my final year of service before retirement on superannuation. So I decided to catch up on all the pending visits to foreign laboratories, particularly because of my plans to do book-writing work after retirement. In March 2004 I visited the Condensed Matter Section of the Abdus Salam International Centre for Theoretical Physics (ICTP), Trieste, as a Senior Guest Scientist. I gave two lectures there: ‘Smart Structures’ and ‘Symmetries and Broken Symmetries in Condensed Matter Physics’.

The ICTP has an excellent library, and I used it every day, gathering material for my book on smart structures. I found that the library stocked a copy of my book on ferroic materials (Wadhawan 2000). One day I happened to mention this to the staff there. Soon I got a call from the head of the library. She told me that they had the tradition that whenever the author of a book in the library was a visitor, he/she was invited to autograph the book and write a few words. I was happy to oblige. Look at the thought behind this simple practice. How many libraries worth their salt do something similar?

From Trieste I went to Montreal. The occasion was the March 2004 Meeting of the American Physical Society (APS). It was a mind-blowing experience. Some

5000 participants. More than 25 parallel sessions on practically all the five days, apart from a few poster sessions. No convener. No inaugural function. No conference bags. No free lunches! No operator in any of the lecture halls for helping with the projectors etc. And clockwork precision in the timing and running of the various parallel sessions, so that the participants could easily go from one session to another.

In one of the sessions the Chairman came four minutes late. Without waiting for him, the audience exhorted the first speaker to start on time, and he did. When the Chairman came in, running, the speaker introduced him, saying: 'Here in the Session Chair'. The Session Chair quietly settled down, and set the clock for timing the already speaking speaker.

My 36-minute invited talk at this APS Meeting was in the focus session on multiferroics. Its title was 'An Overview of Multiferroic Materials and Modelling'. It was the only invited talk of the session; in fact it was the opening talk.

At the ICTP my host was Prof. V. Kravtsov. He attended my lectures on broken symmetries and on smart structures. In the discussion that followed I mentioned to him that I have been working on a book on smart structures, and that it would be a great help if I could come back to the ICTP specifically for the purpose of doing library work for this book. He agreed immediately, so I was able to spend another two months at the ICTP during 2004.

In 2004 I also spent a week at the laboratory of Prof. K. Kitamura, the great crystal-growth expert. This visit to Tsukuba was timed to overlap with the dates of ISFD8, so I could attend that conference also.

After retirement I was given the prestigious Raja Ramanna Fellowship (2005-2010) by the DAE, thanks to Dr. Sahni (the then Director of CAT), Dr. Banerjee, and Dr. Kakodkar.

11. TEACHING ACTIVITIES

I have been involved in teaching work all through my career. I joined BARC in 1968, and from 1969 onwards I have been teaching off and on, first at the BARC Training School, and then at the CAT Training School. Even now, in these

post-‘retirement’ years, I have only enlarged my audience by writing extensively on my blog (*‘The Vinod Wadhawan Blog: Celebrating the Spirit of Science and the Scientific Method’*).

<http://vinodwadhawan.blogspot.in/>

At BARC the lectures I gave were on crystallography, crystal physics, phase transitions, and materials science. At CAT I also covered smart structures. At some stage, as I rose up in seniority, the entire training programme at CAT was put under my charge. I made it a point to establish direct contact with the trainees, and always remembered the dictum that ‘the secret of education lies in having respect for the pupil’. The love and affection I got in return was gratifying. This experience also told me what I was going to do after ‘retirement’: Mentoring and science popularization at a global level (made possible by the Internet).



[Continued in Part 3. The references cited are listed at the end of Part 3.]

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I am a condensed-matter physicist. For the last few years I have been active spreading **the message of science and the scientific method**. In this context I have written over 30 articles at Nirmukta.com. I am listed as an '**Enthusiastic Bright**' at the Brights Net.

Books written

1. **Introduction to Ferroic Materials** (2000)
2. **Smart Structures** (2007)
3. **Complexity Science** (2010)
- 3a. **Nauka złożoności. Trudne pytania, które zadajemy o sobie i o naszym Wszechświecie** (2010)
(Polish version of Book No. 3. Translation by Małgorzata Koraszewska.)
4. **Latent, Manifest, and Broken Symmetry** (2011)

I did editing work for the journal *Phase Transitions* (Taylor & Francis, U.K.) for 25 years, and took voluntary retirement from the task of **Associate Editor** in 2010.

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