T. V. Ramakrishnan

Tiruppattur Venkatachalamurti Ramakrishnan (TVR) was born on 14 August 1941 in Tamil Nadu. He completed his B Sc (with honours in Physics and Mathematics) and M Sc (in Physics) degrees from Banaras Hindu University (BHU), Varanasi, going on to finish his Ph D (in Physics) from Columbia University, New York. He has worked in BHU, Indian Institute of Technology Kanpur, Indian Institute of Science (IISc), University of California, Princeton University, AT&T Bell Laboratories, Jawaharlal Nehru Centre for Advanced Scientific Research (JNCASR) and Cambridge University in various capacities.

He is a theoretical condensed matter physicist interested broadly in the physics of electrons, disordered systems and biological physics; specifically the physics of strong electron correlation (e.g. high-temperature superconductivity in the cuprates, colossal magnetoresistance manganites and general approach to strongly correlated electronic systems), superconductivity in disordered systems and noise in gene expression. He collaborates with other scientists in IISc, BHU, JNCASR and the National Centre for Biological Sciences in pursuit of his novel ideas and interests. TVR has 150 research publications to his credit, in addition to three books and 15 review articles.

In recognition of his contributions to the field of physics he has received, among others, the Shanti Swarup Bhatnagar Award for Physical Sciences from the Council of Scientific and Industrial Research, India (1982), Padma Shri from the President of India (2001) and Distinguished Materials Scientist of the Year from the Materials Research Society of India (2004). He is also internationally recognized for his work; he is a Fellow of the Royal Society, London, and is a Foreign Associate of the Academie des Sciences, Paris. TVR is a member of various editorial boards and scientific councils, including the Scientific Advisory Council to the Prime Minister.

T. V. Ramakrishnan spoke to *Current Science* on 23 January 2012.

TVR in his office at the new Physical Sciences building, Indian Institute of Science, Bangalore.

Can you tell us something about your current research work?

My current research actually focuses on two or three areas of condensed matter physics, having to do essentially with the behaviour of electrons in condensed matter, which has been my concern for much of my working life. In the last few years, I have been particularly interested in a theory for high-temperature superconductors, in which there has been an enormous amount of effort, both experimentally and theoretically, since it was discovered 25 years ago. There have been almost 200,000 papers published on it and it is considered one of the major outstanding problems in physics because the materials in which high-temperature superconductivity is found – namely the ternary and quaternary copper oxides (cuprates) – are metals which become superconductors at unprecedentedly high temperatures, but they are also metals qualitatively unlike any metals we know.

Though there are many approaches and many strongly held views, we (Chandan Dasgupta, who is a colleague in IISc, his Ph D student Sumilan Banerjee and I) felt that a new phenomenological approach is needed. This is the approach which was taken by Ginsburg and Landau in relation to conventional superconductivity which was discovered in 1911, but for which the microscopic theory was given 46 years later in 1957. In the intervening period, almost all the great names in physics tried to develop a theory but were not successful. In 1950, Ginsburg and Landau developed a phenomenological theory, which addresses and explains a wide variety of phenomena in superconductivity on the basis that the superconductor is described by macroscopic coherence which can be mathematically symbolized by a complex function. They showed that the free energy of the system can be expressed as a function of this quantity. From this idea, they developed a large number of consequences. Then seven years later, Bardeen, Cooper and Schrieffer proposed a microscopic theory. They proposed a mechanism for the occurrence of this macroscopic coherence from atomic-level interactions.

When high-temperature superconductivity was first discovered experimentally about 25 years ago, it was felt that one should aim for a microscopic theory, that is, a first principles theory. However, that has still eluded us; there is, at present, no such theory in which you can actually calculate physical quantities and which can do justice to the wide range of phenomena which are observed in systems. So what we thought was that we would go one step back in order to progress, and we developed a phenomenological theory in the spirit of Ginsburg and Landau. This theory has a simple form, deceptively like that of Ginsburg and Landau; but in actual fact very different. We have been able to show that its consequences agree very closely, both qualitatively and quantitatively, with a wide range of experiments. I feel that there are large number of applications of this idea, and I am trying my hand at some of them.

The other thing that I am doing is developing a new approach to strongly correlated electronic systems in solids, that is, in those solids in which the atoms have an unfilled shell. The physical properties at low temperatures are determined by the electrons which are in the outermost unfilled shell. More than a 100 years ago, Drude developed a simple theory for such electrons by making the breath-taking hypothesis that these electrons can be assumed to be completely free – to form a gas of independent particles which roam around the entire solid, having completely forgotten their atomic origin. This Drude free particle theory has been exceptionally successful; it has been improved upon and has assumed so many sophisticated forms, which take into account the effect of interactions between electrons – these interactions do exist, but there have been clever ways of taking them into account.

However, for the last 30–40 years, people have increasingly come upon systems in which the forces between electrons are so strong that this approach seems to be inappropriate. The forces between electrons are particularly strong when they are very close to each other; the electrons tend to avoid each other strongly. These are called strongly correlated electronic systems. In addition, these electrons are also quantum mechanical objects, that is, they are actually waves. Furthermore, no two electrons can be in a single quantum mechanical state; that is the Pauli exclusion principle. So, how do I describe the behaviour of a very large number of electrons which avoid each other strongly, which are like waves, and which obey the exclusion principle? We believe that we have made progress in developing a new paradigm similar to the Drude paradigm.

The third kind of work I am doing is essentially on superconductors in which there is a large amount of disorder, such that fluctuations in the phase of the pairs of electrons affect their properties quite seriously. This is an interesting problem which may have been swept under the rug because of the great success of the Bardeen, Cooper and Schrieffer theory for such superconductors.

What do you learn from the study of disorder in systems?

Suppose there is a system, in this particular specialized case – a system which is disordered. This disorder consists of atoms which are missing or are in the wrong place and so on, and electrons are moving in this medium. Now, the electrons have a tendency to form pairs; these pairs – when they condense coherently – the system becomes a superconductor. So the question is 'what is the effect of disorder on pairing and the phase coherence of these pairs?' This has its own intrinsic interest to physicists like me; it may also have practical interests in the sense that most superconductors that one makes for practical use have some disorder or the other. If the properties of the superconductor are not affected by this disorder, then it is so much the better for those applications.

There was one article[1] in the 1994 special section on condensed matter science in Current Science, in which Professor Leggett enquired whether we are asking the right kind of questions in condensed matter physics? Are we doing that?

I do not know whether we are asking the right kind of questions. The reason I say this is: condensed matter physics is, I think, predominantly an experiment-based part of physics. Of course, all science is experiment-based in my view; it is a study of natural phenomena. But in condensed matter physics, one is able to first make a large number of new kinds of systems. Secondly, one is able to study them experimentally. And thirdly, by doing this, one finds that many of them have very unexpected kinds of behaviour. Now, if such kinds of behaviour occur, our first reaction – if we want to understand them – would be to see if they fit some known mental pattern. If they don't, then we would like to make slight modifications, so that they fit this pattern again. In this process, it is possible that we might miss something which is quite distinct; even if it is distinct, our mental inertia may force us to think of it again in familiar terms, and that is a serious danger.

However, this is masked by the constant discovery of unexpected phenomena or systems. In the last 10 years, completely new families of systems have been discovered, and completely new regimes of physical parameters like temperature, density and so on under which these systems can be studied have been made possible. This is a great development, because we are now able to concentrate on new things like graphene, topological insulators and cold atoms. But it also carries the danger that some of the older phenomena which were not fully understood are shelved. So, often we may not be asking the right questions.

Is there an alternate way?

To me, the only way seems to be to respect experimental facts, and to learn and develop ways of describing those facts theoretically.

How would the approach of a theoretical condensed matter physicist to understand fermions, for example, differ from that of an experimentalist? Would they collaborate?

Collaboration might be useful in two ways: one is – suppose the experimentalist is doing some experiments and finds some features, and may be the theorist has some experience and knowledge which is of use. The other is that the theorist has some ideas and predictions, and he may be able to persuade someone to do the appropriate experiments. So, these are standard ways in which people learn to do things together. However, whether such physical collaboration is possible or not depends very much on the two kinds of people being roughly in the same bandwidth; they may have to be roughly in the same place, think similarly, have similar interests and so on.

What is your opinion about Occam's razor[2]? Are we oversimplifying nature?

Occam's razor is humanly unavoidable because that is how our mind works; I mean, we would like things to be as simple as they can be.

But it may not be the actual case …

It may not be. There is a very famous statement by Einstein which says that one should try to make things as simple as they can be, but no simpler. So, you can oversimplify and miss the essence.

But on the other hand, you may make it very ornate. It is a tricky thing.

But most of the systems which condensed matter physicists deal with are complex!

They are complex, and as a result one very interesting thing happens which is not generally appreciated – completely new kinds of behaviour emerge. So it is not just a question of complexity, of putting a lot of mess together and something happens. But the fact is that when you put things together, something completely different does happen. For instance, you can take atoms of lead; it doesn't make sense to ask whether an atom of lead is superconducting or not. But if you take lots of atoms of lead together, then you make lead metal; then it does superconduct. So, some property has arrived, which is a consequence of having lots of these things. It is very naïve to say that complexity is at the bottom; I mean it is at the bottom for sure, but that doesn't say almost anything.

You have said before that electronic degrees of freedom can be ignored for many purposes. How do you justify such assumptions and approximations?

Whenever you look at the behaviour of something, like this piece of wood, where you are interested in its behaviour at ordinary temperatures, pressures and so on – the question is under these conditions, what are the degrees of freedom or things in this piece of wood which are relevant in giving rise to that behaviour? It may be that this piece of wood consists of lots of molecules which are put together in such a way that the electrons are tightly bound to the nuclei; they form atoms; these atoms form molecules via covalent bonding, ionic bonding or they form a chain by some other process. Freeing an electron may take an enormous amount of effort or energy. So electrons are there, but they are not relevant. You can still think of them as atoms and molecules; you can crudely describe them by saying that this atom is at this place, which means that the nucleus of the atom is there. That is enough; you don't have to worry about the electronic degrees of freedom.

So you can make these approximations?

You have to; that is the whole point of this enterprise – this enterprise is to have an accurate and economical description of a set of interconnected properties. If it is not accurate, it will show up; if it is not economical, then also it will show up. In making an economical description, often you have to make some approximations. There are a few cases in which you can do everything exactly; that is very rare.

Is this true in all fields?

I think it is true in all fields. In some fields people have gone so far as to believe that you don't know the underlying dynamics; only on the basis of symmetry and so on, you can describe the entire happening. Now, that may or may not be true. But in this field, it is believed that – of course, the general symmetries are relevant, important, and are present – in addition, the dynamics is known. We know how electrons move. But then, what form does that take and what are the consequences? There are so many varieties of consequences, and many of these emerge only under certain conditions. That is what makes this field interesting, because there are completely unexpected happenings. Some people feel that 'this is a very complicated system, what else do you expect?' – I feel that this is a vague and naïve statement. The question is: 'in all this complexity, is there some emergent pattern? Is there some simplicity? Is there some characteristic behaviour? And can that be now put in concrete terms?'

What are the practical applications of studies done in condensed matter physics?

Almost everything is a practical application. But unfortunately the distance between understanding and application maybe two weeks, sometimes it may be 20 years. So, some people like me are not interested that much in focusing on the practical applications. But, take for example this recorder here – it has a lot of semiconductors, it has chips. What is a chip? A chip is an integrated circuit. What is an integrated circuit? It consists of tens of thousands of transistors, rectifiers and other things. How can you put all of them in one little thing?. The discovery of the transistor in 1947–48 was preceded by about 15, 20, 30 years of failed efforts. People tried various semiconductors; they were either not pure enough or they could not make right junctions or they did not have the right semiconductors. For a long time, people were not even aware that such a thing was possible. In 1947, Shockley, Bardeen and Brattain discovered transistor action. They actually made the first transistor amplifier. For many years, it remained like this. Integration, microminiaturization – all this took many years. Finally now, we are seeing this recorder.

Jagadish Chandra Bose discovered that crystal of galena (lead sulphide) is a good detector of microwaves. The response time of this detector is quite short, because microwaves have a very high frequency. Bose did not understand what was going on, that this is a semiconductor rectifier. But he found that it was very useful. Now, how he hit upon it – I do not know. That is another peculiarity in science – serendipity. Somehow, somebody finds something. Sometimes, people find things which they don't quite recognize. Very often, people find things they do recognize; the J. C. Bose detector is a good detector and he made good use of it. He made this discovery in 1896 or 1897. I think it was maybe in the 1940s or so that its nature was first understood. So, often it may happen that you do something, and understand why it is happening much later. You can't wait, though.

What is the role of other disciplines like mathematics in this field?

Their role is important. Condensed matter physics is a broad spectrum field. There are some people like me who are connected more closely, maybe temperamentally or otherwise, more inspired by or more interested in phenomena and who do theory. There are other people who are much more mathematically inclined. They are interested in exact results and proving whether some limit exists or not, whether some spectral function is Herglotz or not, that is, mathematical properties of a number of quantities or objects which are relevant to condensed matter physics. But the fraction of those people is relatively small in this area. In other areas of physics, it is different. A fair amount of sophisticated mathematics is obviously needed in condensed matter physics. But it is an experiment-dominated field.

It is also connected in some way to chemistry, materials science…

That's right. It is not only connected, but I think that unless you have good chemistry and materials science, you cannot have good condensed matter science. Condensed matter physicists have a somewhat slightly different emphasis on the very same materials and systems, but you need to have materials and systems. For new materials and new systems, you need to have chemists who are interested and materials scientists who are inventive.

In an article[3] you wrote for the 1994 special section, you said: 'There is a very great need to focus on non-fashionable areas with scientific depth and possible or actual local strength (i.e. to do your own thing)' and 'An advantage (!) of not being at the centre of the scientific world is that one can try harder problems.' Can you elaborate on this?

First, we are not at the centre of science. This, I think, we should realize. Of course, there is a flow of scientists from different parts of the world to this place, and a flow of scientists from here to there. But still, we are not at the centre. Even within the country, there is an enormous inhomogeneity. Most people couldn't care two hoots about science. Even at IISc – a very special place – we are not at the centre of science. But for some reason, the country has supported scientific activity – not extravagantly, but consistently. This means that there is a certain security to scientific activity in this kind of climate. It also means that you are not a creature of fashion. You can focus on national needs, societal needs, intellectual needs, much more seriously over a period of time than you would be able to if you were completely at the mercy of changing values or importance. So in that sense, yes, we are actually privileged. And if you are privileged, then what you can do is that you can look at things which cannot be done in two weeks or two months.

You can think of things which may be harder, which may be long term. This fact is actually well understood by mathematicians. For instance even in the centre of the mathematics world, in Princeton, there was the famous proof of Fermat's last theorem. Fermat scribbled his theorem in the margin of some book more than 300 years ago. This theorem was supposed to have been proved by a Princeton mathematician about 10–15 years ago. So their timescales are very large. But in almost every other science, the timescales become short. What I am trying to say is that in a country like ours where support is reasonable, where you are not at the centre of scientific activity, where you are not buffeted about so much by it, you can afford to and you should do your own thing.

What problems can future scientists take up in condensed matter physics?

Keep your eyes open.

There must be some crucial unanswered questions…

Yes, there are many unanswered questions. You may want to work on them or you may want to solve them, but you may end up doing something quite different. For example, in particle physics, in high energy physics, in cosmology – there are so many fundamental questions. I do not know whether we can answer them or not. And secondly, it may be a kind of romantic fantasy to say that we can answer them in one shot. I think we should not indulge in it too much because like those major problems, if you suggest any solutions, they have to conform to a large number of existing networks of facts. So suppose somebody comes up tomorrow and says that 'I have proved that Einstein's theory of relativity is wrong and I have something else'. Now he is welcome, he should say it if he believes so, and you should listen to him. However, the fact also is that there are hundreds of facts which are explained by or which necessitate this theory of relativity. So now if you start something new, you have to explain those also. There are such issues in mature sciences.

Do we have enough research facilities in India?

Well, I think not. In this experiment-driven field, the experimental facilities of world quality are very few. This is because it is small-scale science, that is, it is science which experimentally can be done in a room about this big (4 m × 3 m). If somebody has a laboratory, he can maybe measure some angle-resolved photoemission spectroscopy from a pnictide or a cuprate; maybe that is an important experiment. But in order to do that, he has to have world-class equipment, which may be small and cost a large amount. But then, it wouldn't be a mega project. It is not big science. Big science means we have a great detector, accelerator, telescope, something which may be five miles in extent and so on.

But this field requires very high quality equipment even to make crystals. Suppose you want to make single crystals of something, then you need an infrared furnace; if you want to characterize it, you need a synchrotron. In order to be able to do world-class work even in small science, you need a collection of facilities. These exist in very few places – if at all – in this country. So the result is that experimental work in physics, chemistry and parts of materials science which use such facilities is not of very high quality; it is not possible.

Facilities are needed and for them to actually reach us, as a society, we need an appreciation that you actually do need these facilities in a large number of places. Suppose you have Rs 500 crores to spend, you can spend it on one very large major thing or you can divide it into 50 small things, and you make 50 world-class laboratories of Rs 10 crore each. Maybe out of them 30 don't do anything; maybe 20 do something. That is one way.

There are many discoveries which essentially have gone past us. In this country where Raman worked, light scattering was a very big thing, well before lasers. Now, the discovery of lasers revolutionized the study of light scattering. But the laser age essentially passed us by. Why? Because that is expensive, needs equipment, maybe you need 20 people who have lasers, maybe you need 10 people who make new lasers, things like that. So it went away. So, in order to be prepared for such eventualities and to take advantage of such things, you need to appreciate this requirement.

Do we have enough manpower?

We don't have enough manpower. But manpower can be created. After all, if you had the facilities you could have students and staff. Suppose you had 50 groups and each group has 4–5 students, then you have 250 students; out of them maybe 50 will enjoy it, take it up seriously and develop into something. It is a matter of time; for instance, in 10 years, you will see the results. So, if there is an appreciation for this, then everything is possible. In the meantime, we can get manpower from somewhere; we can support people who are already there with the equipment. In many ways manpower can be developed, because I think the potential is there in this country. It is a huge country with great people. But somebody has to take it up.

Even for students passing out with a PhD, are the job opportunities adequate?

It depends on what you think is worth doing. They need not all be in the academic world. For example, we do great things in software in India. Many big companies like IBM, AT&T and so on – they have come here, they have established units here, and used the training and intelligence of people here in order to make products. However, we make very little hardware and we make very little large-scale software by ourselves. So, there are opportunities, but you must feel that this kind of thing is worth doing. If we feel that the need of the nation or the need of the world is that people should be employed in some way and the country should be of help to some global village – fine.

Just after the Second World War, Sony Corporation started making radios. When I was a young man, in the sixties, they were making tape recorders; then they began to make TVs, Sony Walkman, and other things. They made all kinds of hardware and so they employed lots of people. So, it is possible if you feel it. If you feel that some other thing is what you should do, you will keep on doing that.

What are your future plans?

I don't know anything except what I am doing. So, I'll just keep on doing this so long as I can do it.

Additionally, I am also interested in science textbooks. I have actually written part of the NCERT textbook for plus-two classes; I was the Chief Editor of those textbooks; this ran for about ten years. From that, I got interested in textbook writing. So, one of the things I would like to do is to write textbooks at a slightly higher level like M Sc and B Sc (honours).

I am also interested in philosophical questions. My view is that the reason we take any branch of knowledge seriously is that it is experimentally or experientially confirmed. Why do we take science seriously? A hundred years back, there were very few scientists. Today, everybody wants to be a scientist. Everybody says it is scientifically proven; I have a scientific method for this. There, the argument stops. If you say something is scientific, nobody argues. This is dogmatic; it is itself unscientific. But it means that being scientific is given a lot of importance. So then you can ask why it is given importance. I think it is because it is experimentally verifiable.

People could say all kinds of things. Aristotle, the great Greek philosopher, said women have fewer teeth than men. But it is a simple matter for experimental verification. He could have just looked at the teeth of his wife. Why didn't he do that instead of making theories? He said that bodies are attracted either to the ground or to the moon. This is again experimentally provable or disprovable. So, that was the state of affairs for a very long time. Then came this idea that things have to be proved by experiment. That made it larger and larger into an open system, and many people are adding to it.

In the same way, there are other branches of knowledge in which people have experience. From this experiential background, you can make various kinds of mental structures. But if you have only mental structures without any experiential backing, then to me it looks sometimes like an intelligent fairy tale, sometimes like a foolish fairy tale. This is my view. So, I would like to work on it, write something about it, talk about it and so on.

1. Leggett, A. J., *Curr. Sci.*, 1994, **67**, 785–795.
2. Occam's razor; http://en.wikipedia.org/wiki/Occam%27s_razor: Occam's razor is the 'law of parsimony, economy or succinctness. It is a principle urging one to select from among competing hypotheses that which makes the fewest assumptions'.
3. Ramakrishnan, T. V., *Curr. Sci.*, 1994, **67**, 871–873.

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