INTERVIEW WITH MICHAEL E. FISHER

■ José M. Ortiz de Zárate - Complutense University and Royal Spanish Physical Society - DOI: 10.1051/epn/2011101

In June 2010 Professor Michael E. Fisher, from the University of Maryland, visited Madrid for the ceremony of the "Frontiers of Knowledge Prizes" that are annually awarded by the Foundation of the Banco Bilbao Vizcaya Argentaria (FBBVA[1]). Prof. Fisher shared the 2010 prize in the category of "Basic Sciences" (400 k€) with Prof. Richard N. Zare from Stanford University.

Prof. Fisher is considered, together with Kenneth Wilson, Leo Kadanoff and Ben Widom, one of the fathers of the Renormalization Group (RG) Theory, probably the most important theoretical development in Statistical Physics in the last quarter of the 20th century, that deserved a Nobel Prize in Physics (1982, to Ken Wilson). For his work in this field, Prof. Fisher was awarded with the Wolf Prize (1980, shared with Ken Wilson and Leo Kadanoff), the Boltzmann Medal (1983) and the Onsager Prize (1995). During his visit to Madrid, Prof. Fisher talked at length with the Royal Spanish Physical Society (RSEF), and in what follows we reproduce part of that meeting as an interview. Most of Prof. Fisher's comments are of general interest. Although some refer more specifically to the situation in Spain, we think they may also be interesting for the wider audience of Europhysics News.

You are well known mostly for your contributions to what nowadays is known as the Renormalization Group Theory of critical phenomena. You may want to share with our readers recollections of those years (1970-1971) at Cornell, and about the importance of collaboration and multi-disciplinarity in the birth of this theory.

Well, how should I answer this question? As a practicing scientist one should know that one's memory in such matters is seldom reliable! Secondly, one should be concerned to give proper credit to one's friends, teachers, collaborators and competitors. For these reasons, some years ago I published a paper [1] where you can find my recollections of that time, and where I endeavored to give appropriate credit to everyone involved.

But perhaps some of the background to the events may be of interest?

I did my thesis in King's College London on analogue computers. After finishing my Ph.D. I found there were few prospects in that field in Britain. I became interested in critical phenomena through Cyril Domb who became Professor in Theoretical Physics at King's College at that time, and took an interest in me.

Initially, however, through his contacts with Aharon Katchalski at the Weizmann[2] Institute I was introduced to polymer physics. In that connection, especially from a paper by Robert Rubin (at what was then NBS[3]) I learned that four spatial dimensions is a special, borderline, case. This knowledge later proved valuable when applied to critical phenomena.

At that time the famous 1944 paper by Onsager where he solved the two-dimensional Ising model exactly [2] was still fresh. Many people were trying to solve it in three dimensions. But I had no such ambitions. Rather, as often when I read a new paper, instead of trying to follow in detail each step, I think about the solution, asking what it tells me and how it might be generalized. Then, combining what I learned from Onsager's paper with what I gathered from Cyril Domb, I arrived to some ideas –essentially scaling concepts– that proved similar to what, independently and over the same period, Ben Widom at Cornell[4] and Leo Kadanoff (then in Illinois[5]) were developing.

Widom read my papers and invited me to visit him. And that is how I eventually ended up in the Chemistry Department in Cornell (with a courtesy appointment in Mathematics). At Cornell Ben and I ran an interdisciplinary seminar where many people came from other fields. Among them was Ken Wilson, who arrived in the Cornell Physics Department at more or less the same time I arrived in Chemistry. Ken Wilson had been a very bright student of Murray Gell-Mann at Caltech[6], and was a very open-minded person. He attended and spoke at our seminar and both Ben Widom and I discussed critical phenomena and scaling with him. In some sense, Wilson learned about critical phenomena from us.

And this was, more or less, the cradle in which Renormalization Group (RG) theory was born! Perhaps, I can add, to my personal credit, my contribution to the epsilon-expansion. I knew that four dimensions (and above) was a special situation where, in some sense, the Ising model can be solved simply. Thus, it occurred to me to suggest an expansion of the theory for dimension d in powers of $\varepsilon=4-d$. I recall very well discussing this issue with Ken Wilson; and this later turned out to be an important ingredient for many (but by no means all) explicit applications of the RG.

In your opinion, the theory of critical phenomena has to be considered nowadays as essentially "closed", or is there room where relevant contributions are still possible?

If you had asked me this question say six or more years ago I may well have answered: "Yes, the theory of critical phenomena is now rather well understood and so may be regarded as closed". However, during the last decade, some intriguing new developments have arisen.

One is related to multicritical points, especially those associated with quantum phase transitions as one sees at low temperatures. Thus the appropriate description of the many phenomena seen in high T_c-superconductors, still seems to raise open questions. Then there is the issue, of long interest to me, concerning the possible existence of "supersolids", exhibiting both long-range crystalline order together with intrinsically quantum-mechanical ODLRO, or "off-diagonal long-range order". The path-breaking experiments of Moses Chan [3] in Penn State University[7] are the prime stimulus. And the books by Subir Sachdev [4] and by Xiao-Gang Wen [5] pose many open theoretical questions: First there is the issue of new types of phases of matter, second of the phase transitions between them and, then, of the associated critical phenomena including the dynamical aspects.

But even in what may be regarded as the "classical theory of critical phenomena" new issues have arisen. One is the so-called "complete-scaling theory" formulated with Makis Orkoulas and Yougchan Kim (two of my former postdocs), which has been taken up and applied to fluid mixtures, colloids, *etc.*, by my colleagues at Maryland, Jan Sengers and Mikhail Anisimov. And related to this there are a host of what we are calling "compressible cell models" –many exactly soluble– developed with Makis and with Claudio Cerdeiriña from the University of Vigo, Ourense Campus (where, sadly, they are planning to close the programme in Physics).

So, in summary, there are still interesting and rewarding issues for theoretical research.

In your opinion: What are the most important developments in Statistical Physics during the last decade?

Well, this is a tricky question for me to answer, in particular for the same issue of giving proper credit that we ▸

Notes

[1] BBVA is the second largest bank in Spain, with an important business in Latin America too.
[2] Weizmann Institute of Science, Rehovot, Israel.
[3] National Bureau of Standards, USA Government.
[4] Cornell University, Ithaca, New York, USA
[5] University of Illinois at Urbana-Champaign, Illinois, USA
[6] California Institute of Technology, Pasadena, Los Angeles, USA
[7] Pennsylvania State University, University Park, Pennsylvania, USA

talked about before. And partly because I do not normally think in such terms! Indeed, many interesting things are discovered and new ideas arise; but what will prove of lasting significance on a scale of a decade is often not so clear.

But, anyway, if I have to give an answer I would say that the Jarzynski Relation [6] and subsequent Non-equilibrium Work Theorems are prime candidates. Indeed, a distinguished Russian theorist has remarked that Jarzynski's relation is the only known true formula that does not appear in the volumes of Landau and Lifshitz! [7]. Indeed, Chris Jarzynski has effectively opened up a new field in Statistical Physics. Now, with precision, one can apply Statistical Mechanics not only to equilibrium states, but also to finite rate processes that carry a system from one state to another. It also has provided a new way of looking at the Second Law, which in classical Thermodynamics was always formulated as an *inequality*, but Jarzynski showed that for some processes it can be formulated as an *equality*.

We know that, because of family reasons, you are greatly interested in what happens in Spain. What is your opinion about Physics in Spain?

So, there is Flamenco in Spain and cante hondo; there are bullfights and fiestas; there is the Semana Santa in Seville and the Alhambra in Granada; there is the mosque in Cordoba and the cathedral in Santiago de Compostela; there is pelota in San Sebastián and the jota in Aragón and, here in Madrid, there is the Puerta del Sol and the Plaza Mayor, the Prado, the Retiro, the Rastro and Chamartín de la Rosa where my wife, the youngest daughter of José Castillejo, was born.

And –jokingly– there is Physics too?...

Now seriously, it has been a pleasure and a privilege to give plenary talks at the meetings of the Royal Spanish Physical Society in Jaca in 1993 and, more recently, in Granada in 2007. And I have spoken on science at other venues in Madrid and elsewhere in Spain.

But it is true that in earlier years, after the Second World War, I was sometimes a bit sad at the seeming paucity of Spanish physicists in International meetings. In the fields I know, Britain, France, Germany and the Netherlands were countries with representatives much in evidence. Later came Italy and Scandinavia. In truth, the period after World War II was hard in Europe. But, initially from the United States and later in the aftermath, from European nations came strong public support for Physics. For Spanish scientists, support doubtless grew more slowly.

But now the situation is becoming not so good for most. We may well be back to what it used to be before the War, when money for science, especially for basic science, was provided mainly by wealthy individuals or their Foundations, primarily because they liked the results the scientists achieved with their grants, and believed that the basic sciences, like the Arts, are fundamental for a good society.

What about Physics education in Spain?

I like to think that I know what I don't know! And, also, how to keep silent when my ignorance is at play. But, yes, in this respect I have seen lately in Spain a development that I had previously noticed in some Eastern European countries. Specifically, it is the substitution in Universities and in secondary education of the basic sciences by other supposedly more "applied" subjects. The teaching of applied sciences is, surely, necessary, but I strongly believe it should be done at a master's level, not as a feature of undergraduate education. Consider, indeed, that society changes fast and modern technology even faster. Anybody of my generation, when looking back, is amazed by how society and technology have changed during their own lifetime. Hence, it happens that applied knowledge that today may be very important becomes, in a decade or so, completely obsolete. Only a good education in basic sciences will give our young students the sound foundations required to cope with the future challenges of an ever-changing world.

Many of the readers of our magazine are young physicists. What is your advice to them, in particular about the selection of a subject for graduate studies?

My advice is very simple and is something I have tried to do during my whole life. Do not undertake anything unless you are really interested in it! There are lots of fascinating things to do in Physics, in the other Sciences, in other professions and in the World at large. Your time is precious. Do not waste it! ■

References

[1] M.E. Fisher, *Rev. Mod. Phys.* **70**, 653 (1998)

[2] L. Onsager, *Phys. Rev.* **65**, 117 (1944)

[3] E. Kim, M.H.W. Chan. *Nature* **427**, 227 (2004); *Science* **305**, 1941 (2004)

[4] S. Sachdev. "Quantum Phase Transitions", Cambridge University Press, 1999.

[5] X.-G. Wen. *Quantum Field Theory of Many-body Systems: From the Origin of Sound to the Origin of Light and Electrons,* Oxford University Press, 2004.

[6] C. Jarzynski, *Phys. Rev. Let.* **78**, 2690 (1997)

[7] L.D. Landau and E.M. Lifshitz, *Course of Theoretical Physics* (in 10 volumes). Butterworth-Heinemann.