

Adriano as Physicist, Teacher, Leader and Friend

(Paolo Rossi, January 26, 2006)

Strange as it may appear, I have a very personal recollection concerning the beginnings of Adriano's career as a physicist. Actually, when I was in my last year before graduation in Physics, I was given by some teacher (I don't even remember who) the task of working out a problem in optics, concerning absorption in a gas in the presence of two e.m. waves with frequencies near the gaps between two contiguous resonances. I worked hard and I made some progress, but my approach was messy and unconvincing, even for myself. Then some older friend gave me a reference: it was the first published paper by Adriano, written fifteen years before, at the age I was having at the time. I was impressed (and quite uneasy, I must say) by the fact that, with much simpler techniques, he had found a clear and elegant solution to the problem I was messing up. And I must say that, during the years, this has been a constant in all of Adriano's scientific activity and production: he does not like heavy formalism, but especially he does not like purely formal developments. Mathematics may be beautiful, but it must be used to solve physical problems, not just to give them a better shape.

Adriano was born seventy years ago, in 1936, in Matera, a very special town in Southern Italy. He always kept a very strong link with his region of origin, and even if he never taught in Universities in Southern Italy he has always had strong attention towards their development, especially in physics, and he has established deep connections and friendships with many colleagues working there (especially in Naples and Bari, but also elsewhere).

His official scientific history does however begin in Pisa in 1954, when he was admitted to the Scuola Normale. He graduated in 1958, and he spent three more years (until 1961) in the Scuola di Perfezionamento, which was then about the only thing in Italy vaguely resembling a graduate course.

After some time in Saclay as a postdoctoral fellow he became an R5 researcher in INFN, but he soon gave up that position to become, in 1963, Assistant Professor at the Scuola Normale. A few years later, in 1966, he got the Libera Docenza, which I would somehow compare to becoming now an Associate Professor. He started teaching almost immediately after graduation, and especially in his first years as a teacher he taught almost everything, from Earth Physics to Atomic Physics, from Thermodynamics to Analytical Mechanics and to Advanced Electromagnetism.

As a student of Radicati's he had begun his research activity in high energy physics, (which in 1960 meant studying pion-nucleon scattering), but in those years his prevailing interests turned out to be oriented towards the physics of matter, probably under the spell and charismatic influence of Adriano Gozzini, an experimental physicist possessing exceptional skills. In particular, starting from his first published works, he studied phenomena in nonlinear spectroscopy (like those observed when radiating a system on two transitions having a level in common), relaxation phenomena in presence of high pressures, and other issues in QED, with an extended use of density operator techniques.

Adriano was clearly a field theorist, and in the Sixties it was neither easy, nor popular to employ field theory methods in the domain of high energy and elementary particle physics. Whoever has experienced, if only marginally, the cultural climate of that period knows what I am talking about: the keywords were nuclear democracy, analytical S matrix, bootstrap, Regge poles, and the very notion of a field theoretical description of elementary particle interactions was repelled as heresy by most authoritative physicists.

The time of change for Adriano was the period he spent at CERN (1968-70), when he went back to high energy physics, probably because high energy physics itself was dramatically changing. Those were the years of duality and the Veneziano model. These are ideas that were

born in the above described context, not within field theory as we know it, but they are ideas bringing rapidly out of that context, and paving the way towards the introduction of strings in theoretical physics. It used to be dual strings, not superstrings: but we know that new interesting ideas in physics usually go well beyond the purpose for which they were introduced.

It was also due time for the coming back of quantum field theory: in the early seventies people started talking about such exotic things like nonabelian gauge theories, Goldstone and Higgs phenomena, dimensional regularization, proofs of renormalizability, asymptotic freedom.. Adriano was a visiting scientist at MIT in 1972 and 1973, and he immediately picked up the new trend: at the end of 1973, when I had to prepare my third year report at the Scuola Normale and asked him to be my advisor, he made me first study Abers and Lee's Physics Report, that had just appeared, and then reproduce in detail Gross and Wilczek's calculations, which were a few months' old. One year later, he pushed me to focus my thesis on solitons and 't Hooft-Polyakov's magnetic monopoles.

But Adriano has a sceptical nature: for him, in order that a theory be interesting, being beautiful is not enough: it must also be useful in order to describe and understand new physics. And physics is not just theory: ideas must be experimentally checked, otherwise it's just chatting. I still remember very well his enthusiastic attitude when the J/Psi resonance was discovered, at the end of 1974: the new phenomenological scenery was intrinsically interesting, even before its deeper theoretical implications had been explored and fully brought to light.

In the meantime his academic life went on: in 1975 he won a national competition for full professorship, and in June 1976 (that is about thirty years ago) he was called as a Professor in the University of Pisa, in the Faculty of Mathematical, Physical and Natural Sciences, including then about sixty full professors (less then half as many as today, but Faculty meetings are now extended to roughly 400 people). He also entered the Physics Institute, which included about ten full professors, about 25 assistant professors and a comparable number of people with a temporary position (all these numbers are a factor of three smaller than today). For comparison, notice that there were about seventy technicians, twice the present figure.

Adriano became almost immediately, and until 1985, the Director of the Scuola di Perfezionamento, but his main effort went into creating a theoretical physics group within the Institute, where Theoretical Physics was at that time virtually absent, since most theorists were at the Scuola Normale. Many of us, younger people, were then graduate students at the Scuola Normale. Adriano managed (and I can now imagine the effort needed) to find physical space for us within the Institute. We all got a desk in a single very big room on the last floor of the old building in Piazza Torricelli, and there many of us wrote down their first scientific papers. But Adriano not only gave us space, he also taught us a new way of staying and working together. Each Monday one of us made a presentation of a recent preprint that had impressed him, and a general discussion usually followed.

Adriano organized and gave the first true course in Theoretical Physics at the Institute; by the way the name of the Course for many years was Quantum Mechanics, since the name "Theoretical Physics" had been already taken by an older professor who taught General Relativity. Adriano wrote down his lectures, which later became the reference book for many generations of Physics students in Pisa, and started the project of publishing his examination problems, with their solutions. He also followed an impressive number of students preparing their Thesis: nobody knows their exact number, but I would bet that altogether they must be no less than a hundred.

Once you enter the business of organizing and directing research, unless you make a strong effort to protect yourself you are bound to be established into an ever growing number of official positions and to get an even larger number of tasks. Adriano did not even try to protect himself, and so in 1979 he became president of the IV National Scientific Commission of INFN and a permanent member of the Scientific Committee of the Della Riccia Foundation.

Three years later he became Vice President of INFN (the President was Zichichi), and kept the position until 1983. Those were years characterized by rapid changes in the Universities and in Research Institutions. A general reform law was issued, which amongst other provisions gave the possibility of creating University Departments and Doctoral Schools. Our Physics Department was then born, and Adriano wrote down its first set of Rules and was the first Coordinator of its Doctoral School (1982-1986).

But I must make a few steps back in time and return to scientific research. This is also the place for another personal recollection. At the beginning of 1980 Giampiero and I were at MIT when Gross and Witten's paper on solving two-dimensional Yang-Mills theory on the lattice in the large N limit appeared. Both of us were literally fascinated by the possibilities that it seemed to open: there was a limit in which QCD might possibly be solved exactly and a calculation scheme (lattice theory) in which dynamics might be analyzed in a fully nonperturbative regime. That was also the time when the first papers applying Monte Carlo techniques to the study of gauge theories saw the light. When Giampiero and I came back to Pisa we immediately went to meet Adriano and told him what we had learnt and the scenarios we imagined for the near future of research. But Adriano cooled us, since he was very critical and sceptical about the possibility that the large N solution might really be found and, most important, that it might be seriously used as a starting point for an analytical study of QCD. He was also very sceptical about the idea that strong coupling simulations (the only ones accessible at that time) could give serious quantitative information on continuum physics.

After that meeting my best bet would have been that Adriano would never spend a minute in his life working on lattice QCD. But individual history is usually at least as paradoxical as general history, and in less than one year both our illusions and Adriano's diffidence were swept away: at the beginning of 1981 Adriano and Giancarlo wrote their famous paper on extracting the expectation value of the gluon condensate from lattice data, and that was his new turning point and the start of the research in which he has been involved in the last 25 years.

Most of you know very well (and many of you know better than me) Adriano's work on Lattice QCD, and therefore I will not expand on this issue here; I only want to remind you that Adriano's entries in HEP Spires after his paper with Giancarlo are 182 to date, and probably about half of them are conference papers and lectures. I stress this point because it is important to underline that Adriano is not only a very good physicist; he is also a very good and dedicated communicator of physics. During his career he never gave up the heavy task of going around the world to tell people his own (and other researchers') work. Such a task has by the way become more and more important, with shrinking times and simplified technologies of scientific communication. Indeed because of the ever growing and basically uncontrolled offer of scientific information, most people in practice no longer read published papers, and the only really efficient way left to scientific communication seems to be nowadays the one based on personal relationships.

Adriano has always taken great care of personal relationships. He has friends everywhere in the world, but one should especially appreciate the fact that many of his relationships involve people living in countries, like Brazil or Russia, that for different reasons required an additional effort. Adriano always felt as a personal duty to give people interested in physics greater opportunities of communication, especially if their initial chances were lower.

I must now go back to Adriano's institutional activities, because I want to come to an initiative of his whose consequences are now exactly under your eyes. In 1985 Adriano was elected by his colleagues as a member of the Administrative Council of the University of Pisa. He was in the Council for three years, and he spent that period, with essentially no help by anybody, to start the process that led the University to buy the dismissed buildings of a factory and perform all the reconstruction activity that was needed in order to turn those buildings into

the present seat of the Mathematics, Computer Science and Physics Departments, of their Library, of the Classrooms for Science Students, of the Science Faculty and, last but not least, of the Pisa Section of INFN, which had been for thirty years confined in the middle of nowhere, physically (and sometimes also culturally) removed from the rest of the scientific community.

In order to realize such an ambitious project Adriano had to bypass all kinds of difficulties, from financial to political, from bureaucratic to technical, including the vicious bound that the external shape of the buildings be preserved, in the name of “industrial archaeology”. There were times in which Adriano was probably the only man on Earth who kept believing in the ultimate feasibility of the project. But he went on anyway, and he got the result: this is typical of his.

Another of his never-ending activities is being a Commissioner in all kinds of competitions and selections of scientific personnel, from choices of full professors to junior INFN positions, from postdoctoral grants to admissions to Scuola Normale or to Doctoral Schools, from evaluation of scientific projects to rating of scientific production. Many of us got their initial position, or their promotion to higher status, thanks to his presence in some Commission. But I must stress, even if this may sound like some kind of vanity, that Adriano’s efforts were always made under the assumption that the involved person was worth the effort, and never as a pure power exercise.

We are obviously grateful to him, for the physics he has been doing, for the physics he taught us, for the opportunities he was able to create for many of us. But I think that we must be grateful not just as individuals, but also (and more) as members of these institutions, our University and especially our Department, because this place exists, physically and culturally, and has some relevance for the international community of theoretical physics, mainly thanks to his intelligence, to his passion, to his work and to his constant effort: he is a man whose deep beliefs have never been an obstacle to practising his good sense, and whose solid good sense has never been an obstacle to affirming his beliefs.

I don’t like a systematic use of quotations, but some of them (and especially those coming from old times, I believe) catch the spirit of a man better than many long descriptions. So let me conclude with a quotation from Pierre Abelard, a XII century cleric and philosopher, which in my opinion is a good representation of the scientific spirit Adriano has tried to teach us:

Dubitando ad inquisitionem venimus; inquirendo veritatem percepimus
(Petrus Abelardus, *Sic et non*, Prologus)