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Oral History Interview

The beginnings of theoretical condensed matter physics in Rome: a personal remembrance*

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Abstract. This oral history interview provides a personal view on how theoretical condensed matter physics developed in Rome starting in the sixties of the last century. It then follows along the lines of research pursued by the interviewee up to the date of the interview, in March 2006. The topics considered range from the phenomenology of superfluid helium and superconductors, critical phenomena and renormalisation group approach, quantum fluids to strongly correlated electron systems and high temperature superconductors. Within these topics, fundamental problems of condensed matter physics are touched upon, such as the microscopic derivation of scaling, the metal-insulator transition and the interaction effects on disordered electron systems beyond the Anderson localisation, and the existence of heterogeneous states in cuprates.

1 Formative years

L. B. Let’s start from the very beginning, from your birth…

C. D. C. I was born in Rome to a relatively prosperous Roman Jewish family on 14 August 1937. Early photographs of me, with my parents and two older brothers, paint a picture of a happy family. Following the enactment of the racist laws of 1938, our family experienced increasing difficulty to the point of complete insecurity. My father Angelo Di Castro, 37 years old at that time and already an established

* The English text presented here and revised by the authors is based on the original oral history interview recorded in Italian at Carlo Di Castro’s office, Physics Department of Sapienza University, Rome, Italy, March 2006.

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architect, was struck off the register of architects[1], and found himself, with three children, having to get by doing odd jobs and temporary work, always subject to possible blackmail. Later, after 8 September 1943, under the Nazi occupation, the difficulty and uncertainty brought about by the racist laws turned into fear for our very survival.

My earliest memories are those of a prematurely aged child, with the weight of the world on his shoulders, preoccupied with the fate of mankind. When I was six, we had to flee our home and go into hiding, memorising false names under the threat of terrible consequences, and in constant fear that cousins of the same age might not be as responsible in memorising false names, and might betray you. Despite my mother's constant and reassuring presence, it was at that time that the figure of the ogre took shape in my mind – the ogre of fairy tales, which was however, a tangible part of my reality. This kind of weight obviously has an impact on the whole life. I remember for example the fall 1944, when I entered a school for the first time after the liberation of Rome on June 4 of that year. I was alone in a huge room, at the Pestalozzi school in Via Montebello, near our house, to take an entrance exam for the second grade, since I had been impeded to attend the first grade. Although it had been hard for me, never having been able to go to school while hiding, I always thought how much harder it must have been for my older brothers to have been actively expelled in 1938. Ever since that time I have lived with the idea that life can be engulfed by darkness at any moment, although, over the years, I think I have also acted upon this idea of impending fate. In fact, I am convinced that the professional path I have chosen is related to the firm belief that fear of our surroundings, fear of the unknown, must be overcome through knowledge and reason, eliminating the unknown.

It probably was not a coincidence that, in fall 1948, while still in middle school, I enrolled in the German-language section of the Liceo Classico "Torquato Tasso", where I remained until matriculation, in 1956. Perhaps learning German was a way of getting to know the beast that had attacked me as a child, in an attempt to overcome my fear.

L. B. After these early traumatic experiences, which were your feelings as a young boy?

C. D. C. I felt the need to understand the label that "the other" considered so contemptible, and that had had such a terrible impact on my childhood. It was no longer enough for me that it was the natural course taken by my family. The need to organise the various aspects of life along rational lines also led me to actively explore Judaism, as opposed to passively accepting family tradition. From Judaism I selected and constructed a secular vision of my own, with a metaphysical foundation that left no room for religious irrationality. The idea of the absolute transcendence and unity of God dominates Jewish thought. In his absolute transcendence, God cannot be confused with his "creation", and man is therefore free to explore nature in the constant search for signs and traces left by the "Creator", without the danger of confusing them with the Deity and venerating them in his place. Contact between God and man occurs through morality, ethics, which should guide men to recompose in unity the multifaceted aspects of life and the impulses that should not be rejected as evil and condemned as sinful. There is no contradiction between science and religion, if

[1] According to the Law No. 54 "Disciplina dell'esercizio delle professioni da parte di cittadini di razza ebraica" [Regulation of the Exercise of the Professions by Citizens of the Jewish Race], 29 June 1939, my father was requested to provide "immediate notification of membership in the Jewish race", with a letter dated 12 August 1939, signed by Arch. Plinio Marconi, Secretary of the Inter-provincial Fascist Union of Architects (Arch. A. Di Castro, personal papers, Central Archive of the State, Rome).

these are understood correctly. The task of religion is to identify human goals and aspirations, but it should not interfere on the plane of the knowledge, offering irrational explanations of the unknown and the conception of a redemptive and retributive God. The rational reconstitution of the various aspects of nature on the part of human beings, free of dogma, and the unitary reconstruction of knowledge, lie at the foundation of scientific research, but also of the teachings that may be derived from Judaism. Fear of the unknown is thus overcome. If phenomena appear to be inexplicable or unpredictable, it is due to the complexity of the factors that contribute to them, and not because they are driven by evil, infernal forces to be exorcised.

During the period of the reconstruction, in the 1950s, we engaged in endless discussions, among us – members of the re-established Federation of Italian Jewish Youth (Federazione Giovanile Ebraica Italiana) – in our newsletter Ha-Tikwà, and with members of Catholic youth movements, regarding the vibrant and active role that we claimed for Judaism, in order to restore the proactive dignity it had been denied. We debated whether the humanisation of the transcendent God that occurred in the passage from Judaism to Christianity had resulted in the deification of man and consequent negation – often quite marked – of the material component in man, as evil or sinful. We proposed, as a key element of modernity, the unitary reconstitution of man's earthly inclinations at image of divine unity, fragmented in the concept of the Trinity. The humanised God is no longer above human activity or beyond cognitive practice that, no longer free, thus assumes the role of ancillary support of faith. Individual freedom – including freedom from fear and evil – can on the contrary be regained by means of the free acquisition of knowledge and the reconstitution of our own personality.

L. B. How did all this cope with your other cultural interests?

C. D. C. All this took place while I was at high school – a period in which my cultural education was of a more humanistic, philosophical and artistic nature. If I had to name some of the books that had an influence on me, beyond the Pentateuch, the Gospels, treatises on Judaism ranging from the tradition-affirming works of Dante Lattes, to the "rational" mysticism of Alfonso Pacifici and the universalistic innovations of Leo Baeck, which gave tradition a new appearance and old thought a new language; the philosophical classics come to mind: from Aristotelian metaphysics to Kant's Critique of practical reason and the philosophical writings of Marx, which rendered metaphysics inessential in philosophical reasoning, while Judaism, with its absolute transcendence, avoided the contamination of metaphysics with knowledge. Figurative art played a special role in my family. Beyond architecture, which I lived and breathed in my father's office, where I would leaf through his professional journals, painting was one of my main interests. Books such as B. Berenson's Italian Painters of the Renaissance [Berenson 1948], from which I learned to love Masaccio, or Longhi's work on Piero della Francesca [Longhi 1946], my favourite painter, come to mind.

It is at that time that I also began my life together with Franca Tagliacozzo, who taught me the values of a profoundly secular culture.

2 Physics: a difficult choice

L. B. In the light of this background, how did the choice of physics come about?

C. D. C. Actually, I had some difficulty in deciding between philosophy and physics. Physics intrigued me, because it was not a part of my primarily humanistic upbringing, but certainly fell within the realm of my interest. During the summer preceding my matriculation, I studied mathematics, reading a book that had just come out in Italian at the time – *What is Mathematics?* [Courant 1950], by R. Courant and H. Robbins – and began to explore the world of physics. In my presumption – given my supposed humanistic and philosophical knowledge – I judged the prevailing idealism at the School of Philosophy in Rome inadequate, and studies abroad were unthinkable at the time. I therefore decided on scientific studies, because it was a world I was curious about and with which I was unfamiliar. My father was not happy with my original choice of philosophy or physics, considering both fields impractical. The pressure he exerted was entirely psychological, intimated rather than imposed, but – dutiful son that I was – it resulted in a compromise, and I registered for engineering. At the time, the first two years of engineering studies coincided with those of physics and mathematics. The mathematics classes were overflowing. We had to bring folding chairs in order to find a decent place. Coming from classical studies, I remember that in the first few months I was somewhat bewildered by this new world of mathematics, but eventually managed to find my way in and was fascinated by it. I was captivated by the new language that was, more than a language, a logical structure, which could be applied to problem solving.

At the same time, we were introduced to physics, to experimental science, and assigned practical exercises. I studied general physics with Giorgio Salvini the first year, and with Edoardo Amaldi the second. We discovered the boundaries and connections between mathematical extrapolation and actual measurement, with implications that had never even been mentioned in our previous studies – contrary to the Anglo-Saxon approach.

Having completed my first two years of engineering, fortune (or misfortune, from my father's perspective) had it that engineering classes never got off the ground in November, the beginning of the academic year at the time. I therefore started to attend physics classes and, at a certain point, decided to move from engineering to physics.

L. B. It is an old story. Beginning with engineering has always been a reassuring choice, also for the families. Still in the 1950s, physics was widely considered a way to high-school teaching. Actually, all the young members of Fermi's group in Rome – Emilio Segrè, Edoardo Amaldi, Ettore Majorana, Bruno Pontecorvo – came from engineering... How did you live the change?

C. D. C. It occasioned an important development in my scientific training. Although I had a relatively high grade average, Mario Ageno, who was on the transfer committee from engineering to physics, wanted to test me on first year general physics. In preparing for the exam, I chose to elaborate on thermodynamics. I was struck by the incontrovertible logic of its principles, derived from a phenomenological theory based on experience, with no possibility of contradiction. The only other example of this kind of theory, as I later discovered, is that of special relativity. In this case as well, Einstein subjected observed data to closed analysis, on the basis of which he was able to construct an internally coherent theory. The other fascinating aspect of thermodynamics, which subsequently became one of the central elements of my research, is the idea that the behaviour of matter can be described by means of a small number of variables provided the choice of this set is carefully made with relation to each specific problem under study. Since then I have become convinced of an intrinsic reductionism that, more subtle than the naïve reductionism of the past[2], enables to start from the description of a system in terms of its innumerable constituent

[2] For example the naïve reductionism à la Jeans of the beginning of the nineteenth century, who wanted detect the difference between life and non-life in the number of electrons of carbon [Jeans 1933].

elements, whether molecules, atoms, electrons, etc., and arrive at a description of its macroscopic properties in terms of a few relevant variables.

L. B. Which courses in particular aroused your interest?

C. D. C. I became interested in statistical mechanics and in the theoretical physics of condensed matter, largely ignored as research topics in Rome, at a time – end of the 1950s – when everyone was engaged in the study of elementary particle physics. The course in statistical mechanics was given by Bruno Touschek, who was brilliant and thus certainly stimulating. Statistical mechanics was not his field, however. His course was based on the short book *Statistical Thermodynamics* [Schrödinger 1957] by E. Schrödinger. He would also extemporise on specific topics, not teaching, strictly speaking, professional statistical mechanics, but rather how a theoretical physicist should approach problems with imagination, technique and enthusiasm. At that time, theoretical physics was taught by Enrico Persico, who played an important role in establishing the teaching methods employed at the Institute. As a result of his dedication to simplicity and linearity however, the range of topics covered may have been overly limited. The study of quantum mechanics in those years, for example, never went beyond wave mechanics. It was only later that I discovered quantum mechanics as an organised discipline, using Dirac's book [Dirac 1959]. During that period, we students also devoted a good deal of our time at the Institute to the study of the already-obsolete field of valve electronics (the transistor had been invented in 1947). The second part of our curriculum, as students of physics, was thus somewhat lacking in condensed matter and statistical mechanics, inasmuch as it was far from the research frontier of the day, with the exception perhaps in regard to elementary particles. The students could in this case fill the lacuna during their thesis. I recall that Marcello Cini's lectures – during which we were introduced to dispersion relations and scattering theory – seemed terribly advanced to us. I must say that while the didactic component of the second half of the programme left something to be desired, the research aspect again in the high energy physics, animated by Edoardo Amaldi, worked quite well.

L. B. At that time the 1.100 MeV electron synchrotron was beginning to work in Frascati Laboratories, becoming an important tool for the reconstruction of Italian physics, and the 28 GeV Proton Synchrotron at CERN was accelerating its first protons. It is clear that people were very excited about these new powerful tools for particle physics. Obviously, there was in any case a problem of cultural legacy. In Italy, there wasn't a great tradition of research in the fields of condensed matter physics or statistical mechanics...

C. D. C. I believe that also these disciplines would certainly have been developed by Fermi's group. Under the Fascist dictatorship however (the Racial Laws of 1938, the war, etc.), Italian physics was destroyed, and after the war, only Amaldi was left in Italy of the old Fermi group, with the difficult task of rebuilding the field. Obviously, capable young physicists wanted to pursue the physics of the moment, i.e. elementary particles. The Institute in Rome thus had little to offer in terms of my interests in condensed matter physics and statistical mechanics.

3 A Thesis on Superfluid Helium

L. B. At that point, your interests were taking shape...

C. D. C. Yes, and in 1959, having decided to do a thesis in theoretical physics, I approached Marcello Cini, head of the theory group at that time. Having no experience in the area I was interested in, he told me that he could not suggest a specific topic, but that Giorgio Careri, who had been conducting experimental research on superfluid helium with isotopic number four, ^{4}He, would soon be arriving as professor in Rome from Padua.

L. B. With his group, he conducted a number of experiments that were in complete agreement with Feynman's model of rotating helium. Actually, Careri had been working for some time on topics that were virtually unheard of in Rome. In this he was especially encouraged by Edoardo Amaldi. In 1957, he organised in Varenna an international conference on the condensed states of matter.

C. D. C. I thus began studying superfluidity. I liked that topic on which statistical mechanics and quantum mechanics are converging. Matter transport in a superfluid is characterised by an ordered motion without dissipation, and the particles must be coherently organised in a single state – something that ^{4}He atoms can do because they satisfy Bose statistics, which allows several particles with integer spin to occupy the same state. Below a critical value of the temperature a finite fraction of the total number of particles of a Bose gas occupy the same single-particle state (the famous phenomenon of Bose-Einstein condensation now observed in many alkali atoms). At that time this phenomenon was considered to occur only in the liquid ^{4}He below the critical temperature for the onset of superfluidity (2.17 Kelvin). For this system, loosely speaking, the quantum properties (e.g. quantum interference) of a single particle state, being occupied by a finite fraction of the total number of particles, are transferred to a macroscopic scale. I began to look for a topic for my thesis. In 1947, N. N. Bogolyubov [Bogolyubov 1947] introduced a simple model of a superfluid, transferring the condensation properties of a Bose gas to the interacting bosons proper of liquid ^{4}He. In 1957, BCS theory (named after its authors: Bardeen, Cooper and Schrieffer) [Bardeen 1957] offered the first explanation of superconductivity, decades after its discovery. Just as matter transport, in the case of helium, occurs without viscosity at temperatures below the critical temperature, charge transport in superconductors occurs without resistive effect at temperatures below the critical temperature, which varies from material to material, and was no larger than 10 Kelvin for the superconductors known at the time. The electrons that transport the charge, however, are fermions, particles with half-integer spin governed by the Pauli exclusion principle, whereby no more than one electron may occupy the same state. According to BCS theory, below a certain temperature, electrons may couple in pairs and then condense, like bosons. I therefore decided, for my thesis, to extend the Bogolyubov model, introducing pairing interaction in condensed bosons as well. To my surprise, while Bogolyubov's very simple approximation led to the correct excitations from the ground state of helium, i.e. phonons with linear relation between energy and momentum like elastic excitations in crystalline solids, my pairing correction introduced a wrong energy gap between the ground state and the first excited state. Cini was my advisor, and my outside examiner was Touschek, who returned the thesis with a comment written in his typical German-Italian style: "Con complimenti *del avvocato* del diavolo [With congratulations from the devil's advocate]".

While I was working on my thesis, Gianni Jona-Lasinio returned from Chicago, where he had done his famous work on spontaneous symmetry breaking, with Yoichiro Nambu. Naturally, I began to discuss my research with him, and he became a source of constant advice and intellectual enrichment.

L. B. Did you already know each other?

C. D. C. No, we met at that time and a very close professional and personal relationship ensued, one that has lasted over the years, despite our different tastes in research, which subsequently led him to mathematical physics and pure statistical mechanics, and me to condensed matter and many-body theory. During that period, we had begun to try to understand what was wrong with the pairing approximations for bosons. One day, Gianni, on his way back from the library, told me that results similar to my thesis had already been published in the *Physical Review* [Girardeau 1959]. We did not have at that time internet at our disposal and no experience in the field was available in the Institute. Although it could not be published, my thesis served to introduce into theoretical physics at our institute, a field that – with the arrival of Careri – had acquired a significant experimental presence, and which has always been present in my work.

In the summer of '61, before graduating (I graduated in the fall), I attended the Summer School on superfluid helium organised by Careri, at Varenna. I then learned how different was the international context in this field from our Institute in Rome and the significant work necessary to update it in condensed matter physics in general. All of the physicists who had played a significant role in the development of superfluid physics were there, including G.V. Chester, with whom I discussed my thesis. He told me that in Rudolf E. Peierls' department at Birmingham, where he was working at the time, John G. Valatin had found a solution for the pairing approximation with excitations, without an energy gap, however, his approach was not consistent, as I understood later. Chester suggested that I do my doctorate at Birmingham.

4 Doctorate at Birmingham and return to Rome

L. B. What happened? Did you go to Birmingham?

C. D. C. At that time, Amaldi had the foresight to send the young physicists abroad, with Italian National Institute of Nuclear Physics scholarships, to study fields other than that of elementary particles. I left for Birmingham and entered the warm community surrounding Peierls. In the meantime I had also gotten married with Franca, twenty-one at that time, and she became the protégé of Mrs. Peierls. At Birmingham, beyond G.V. Chester's courses on superfluids, Valatin's on superconductors, and R. Peierls' on nuclear and solid-state physics, I also participated in S. Mandelstam's course on field theory. With J.G. Valatin I have studied the behaviour of superconducting films as a function of the external magnetic field and, by decreasing the thickness of the films, the change from first to second order of the superconductive transition [Di Castro 1964]. The same results were obtained in parallel to Y. Nambu and S.F. Tuan [Nambu 1964].

At this point, I would like to mention that in the following I will only refer to facts and papers that are related to the development of my research. The reference list will therefore be far from complete.

On my return in 1964 to Italy I had to serve in the army for a year and half.

L. B. Military service must have been a catastrophe at that moment!

C. D. C. Yes, a real catastrophe! All of my activities were interrupted. I had to write and publish the results of my doctoral thesis and continue the projects I had started. I still remember the astonishment of my colleagues and professors at Birmingham when I told them that I would have to perform military service when I got back to Italy.

Toward the end of my time at Birmingham, Chester was invited to Cornell, to join the university where among the others in the following years would have been Ben Widom, Michael E. Fisher, my friend David Mermin (who did a post-doctorate at Birmingham while I was doing my doctorate) and later Kenneth Wilson, relevant actors in the field of critical phenomena which would soon become the field of my main interest. Chester once alluded to the possibility of going with him to the United States, for a post-doctorate.

L. B. What happened then?

C. D. C. I discussed with Marcello Cini who suggested me to return, in order to start in Rome activities in the field. Cini's advice aside, I decided in this sense, both because I thought it would be interesting to try to build something at the Roman Institute, and because I felt that as good as my English was, I would never be able to master the language to the point of using it with the kind of refinement that would allow me to participate fully in local cultural life. I felt this particularly in England at that time, where use of language was a significant factor in discrimination.

Another reason for this was Franca's reluctance to be "the physicist's wife". She would become an enthusiastic and dedicated teacher – as well as author of papers and books on Jewish history, in particular on the community of Rome – and had no desire to give up her career.

I returned to the Institute in 1965 after completing my military service, and joined the Rome division of the National Institute of Nuclear Physics, benefiting once again from Amaldi's foresight in opening the INFN to young researchers from other fields and rebuilding the physics as a whole. I began teaching at the *Scuola di perfezionamento* (the equivalent of a PhD programme, at the time) – the first course taught in Rome, on the theory of superfluids and superconductors [Di Castro 1965]. Among the active participants in the course were the young researchers of Careri's group, and regular meetings were held with his group, to discuss the latest articles published in the field. At the time, I tried to provide phenomenological explanations [Di Castro 1966], in parallel to the one of K. Huang and A.C. Olinto [Huang 1965], for some of their experiments on repeated steps in the mobility of ions and on the creation of vortices in superfluid helium.

I remained an employee of the INFN until 1969, when I became "libero docente" (the analog of Privatdozent in Germany) in theoretical physics and an assistant professor. I was also given the theoretical physics course for the students in the last year in physics, due to Persico's untimely death. It was quite an experience for me, because I enjoyed introducing topics that had never been taught before in Rome, like quantum mechanics in Feynman path integral formulations and the connection with statistical mechanics topic that would become central in statistical mechanics research in the 1970s. I don't know whether the students enjoyed themselves as well, but they made an excellent impression on me. Among them I recall Luca Peliti and Marco D'Eramo who did their theses with me, (Marco eventually decided to leave physics and become a journalist), Errico Presutti and Giorgio Parisi, who has contributed so much to the study of physics, particularly in the area of the connection between field theory and statistical mechanics.

5 Critical phenomena and the necessity of a change of paradigm

L. B. What else were you working on at the end of the 1960s?

C. D. C. I picked up where I had left off with Valatin and W. Young – who, as a doctoral candidate, had continued my work on superconducting films. With Young, I obtained a microscopic derivation of the temporal evolution of the order parameter that controls superconductor transition [Di Castro 1969a] (the so-called time-dependent Landau-Ginzburg equation), and I started to focalise my interest in critical phenomena. Until the mid-'60s, helium and superconductors were studied primarily for their specific properties of absence of viscosity or resistivity, and for the peculiarity of exhibiting quantum effects on macroscopic scale. In the years that followed, they were studied as one of many examples of phase transition, or change from one state of aggregation of matter to another one – in this case from normal viscous liquid to superfluid, or from normal dissipative metal to superconductor. There was an attempt to unify all phase transitions (gas-liquid, ferromagnetic-paramagnetic, phase separation in binary mixtures, etc.), studying their general properties in order to identify their shared, universal aspects [Patashinkij 1966; Kadanoff 1967] near the critical point. In the transition of a simple fluid from the gas phase to the liquid phase, for example, the critical point coincides with the terminal point of the coexistence curve of the two phases, when temperature and pressure are varied. Along the coexistence curve the two phases have different specific volumes, which become equal at the critical point.

Another example is the transition, in some materials like iron, from the paramagnetic phase stable above the critical temperature, the Curie temperature T_c, to the ferromagnetic phase stable below T_c. The ferromagnetic phase differs from the paramagnetic phase because of the presence of the spontaneous magnetisation, the magnetisation that survives even in the absence of an external magnetic field H. The elementary magnetic dipoles present in the system are disordered above T_c and align spontaneously below T_c without the action of an external field. In this case the critical point is therefore at $T = T_c$ and $H = 0$. The coexisting phases for $T < T_c$ and $H = 0$ are the two degenerate phases aligned in opposite directions. Again this difference is vanishing at the critical point. In general the critical point is the point of high instability, where two coexisting phases lose their individuality.

L. B. What kind of approach had been used to study these phenomena?

C. D. C. Earlier, in the 1950s and '60s, the dynamic properties of each single systems were studied, in order to identify their elementary excitations. Lev D. Landau had introduced phenomenological theories in which the thermodynamic or transport properties of each system were reconstructed from their elementary excitations or quasiparticles, for bosons (superfluid helium) [Landau 1941; Landau 1947] as well as fermions (liquid helium with isotopic number 3 or metallic electrons, for example) [Landau 1957; Landau 1958]. Systems consisting of strongly interacting particles were replaced in general with a gas of weakly interacting quasiparticles. The effects of interaction were effectively incorporated in the change of type of excitation spectrum with regard to the original free particle form, that could be detected phenomenologically from thermodynamic quantities like the specific heat.

It was necessary to find techniques for the treatment of interacting many-body systems, and perform calculations of the dynamic to get the excitations specific of each system. Hence, the enormous development of many-body theory in those years. Excellent examples of this were at that time D. Pines' collection of articles, *The Many-Body Problem* [Pines 1961], and *Quantum Field Theoretical Problems*, by A.A. Abrikosov, L.P. Gor'kov and L. Ye. Dzyaloshinskii [Abrikosov 1965]. Once the quasiparticles had been identified for each system, the statistical mechanics was trivial and essentially reduced to that of a gas. The approximate treatment of interacting systems by means of a gas of excitations or quasiparticles works well as long as the temperature is sufficiently low. Increasing the temperature increases the energy content of the system and, therefore, increases the number of excitations that begin to interact with one another and the quasiparticle concept itself becomes ill-defined. This simplified schema that was dominating research in condensed matter physics, is thus no longer valid near a critical point. A new approach to research in the field of condensed matter was necessary.

L. B. Before you describe the new approach in critical phenomena, in what sense did the simplified schemas for the treatment of many-body systems then in existence lose validity?

C. D. C. One can actually look at the problem from a different perspective. As the critical point is approached, the system becomes unstable, changing its properties and assuming the structure appropriate to the other phase. Ever larger islands of the incorrect phase form within the dominant phase, until the entire system changes phase or, more precisely, fluctuations of the parameter identifying the difference of the two coexisting phases increase. For example, approaching the critical point of the gas-liquid transition, density fluctuations will increase, while the transition from the paramagnetic to the ferromagnetic phase will result in increased magnetisation fluctuations. These pockets of phase instability consist of regions in which the single constitutive elements of the system (the molecules of fluid or the magnetic dipoles in the above examples) are strongly correlated, and can no longer be considered independent in any way. As the critical point is approached, the correlated pockets increase their extension, the correlation length, eventually comprising the entire system. Once the critical point has been reached, the degrees of freedom of the entire system (on the order of the Avogadro's number) strongly interact with each other, and the schema of independent particles or quasiparticles can no longer be applied, nor can perturbation theory be used to construct the properties of the correlated system, starting from the uncorrelated system. Technically this impediment appears as singularities in the perturbative calculations. All of the schemas at the theoretical physicist's disposal fail, and it is no wonder that the problem remained unresolved, despite the fact that it has been addressed by physicists of the calibre of Landau [Landau 1937a; Landau 1937b]. Due to the large number of strongly coupled degrees of freedom, the critical phenomenon becomes a theoretical problem of strongly coupled field theory.

L. B. As in elementary particle physics?

C. D. C. Exactly, but here the singularities appear in the infrared, not in the ultraviolet limit as in particle physics. Indeed Gianni Jona-Lasinio was very active in field theory. It was somehow in the air I breathed, both working at the Physics Institute in Rome, in which elementary particle physics was dominant, and by attending the course of Mandelstam in Birmingham. But I had no active work in this field. In 1966, I attended the 9th Brandeis Summer School on critical phenomena, held from June 20 to July 29, 1966. Here I learned the approach to critical phenomena that Kadanoff was then developing, highlighting their universal aspects [Kadanoff 1966; Kadanoff 1967]. These general features intrigued me and once back in Rome, I began to discuss them with Gianni.

6 Cooperation with Gianni Jona-Lasinio and the renormalisation group approach to critical phenomena

L. B. You've mentioned your relationship with Gianni Jona-Lasinio a number of times. Could you elaborate on the kind of work you did together?

C. D. C. I will try to outline, from my perspective, the problems that we sought to address. As I mentioned earlier, in the 1960s it began to emerge that phase transitions should not be any more analysed individually in order to understand the specific properties of each system. The analogies between different systems near their respective critical point should instead be considered. For instance, how magnetisation varies with the variation of the magnetic field in a magnetic transition, or how the difference in the density of liquid and gas varies with the variation of pressure in the gas-liquid transition, is not relevant by itself. The important point to notice is that, near the critical point, the behaviour of these quantities is identical for the two systems, as different as they may be from one another. In general, as long as a correct correspondence is established between the appropriate variables in each system (e.g. the magnetic field and the pressure, the magnetisation and the difference in the density of liquid and gas), then the critical behaviour is universally identical. Systems can thus be classified into groups, the so-called universality classes, with the same critical behaviour, based on the symmetry properties of the system, in particular of the quantity characterising the transition i.e. the order parameter, like spontaneous magnetisation in the case of magnetic transition. Recall that in this transition, above the Curie temperature, the constituent elementary magnets are all disordered and the system is rotationally invariant. Below the Curie temperature, the constituent elementary magnets are aligned in one direction and the rotational symmetry is spontaneously broken into an up or down self-organisation of the system along this direction. The one-parameter scalar representation of this ordering below the Curie temperature marks the same behaviour with the gas-liquid transition, the one-parameter ordering being in this case the difference in the density of liquid and gas. In nature the ordering of the ferromagnetic phase can also occur in any direction of a plane (with a two-component vector as the order parameter), or of space (with a three-component vector as the order parameter). Their critical behaviours are different and differ from the uniaxial ferromagnet. Near criticality, an uniaxial ferromagnet behaves as a critical fluid and differs from a planar or an isotropic ferromagnet. The problem of criticality was one of the first problems of complexity, so popular today. In fact, it was the key example of it.

L. B. It appears that the problem was almost unsolvable from a standard point of view...

C. D. C. Yes, indeed... As I already said, perturbation theory was in fact unhelpful, because it required the treatment of infinite degrees of freedom all strongly correlated with one another, and the quasiparticle schema was unhelpful as well.

L. B. How was the problem addressed?

C. D. C. It was necessary to invent a new paradigm. As it often occurs in the development of research in physics, reasons that preclude the use of previous schemas, when properly exploited, lead to the development of new ones, which then become paradigmatic. The fact that the correlation distance, the distance over which the fluctuations occur, increases to infinity as the critical point is approached, actually precluded the use of perturbation theory. Viewed as an obstacle to the application of previous paradigms, this became the basis for finding a new simplified schema. In fact, if the correlations become important over larger and larger distances, that means that everything that occurs over short distances is not important, that is to say irrelevant. The details specifying each particular system are not important near the critical point and universality becomes the new key of interpretation. Moreover, beginning from the representation of the system in terms of interacting particles on an interatomic scale, one had to find a way to describe the system in terms of variables that would regroup the original variables in larger cells of size L times the average distance between the particles. Since the correlation length at the critical point goes to infinity, the transformation might be repeated, increasing the scaling factor L. Near the critical point, the physical quantities of the original system and the scaled system should display the same behaviour. By imposing the invariance of the physical quantities for this change in the length scale, even without explicitly realising the transformation, strict relations between the parameters that characterise the critical behaviour (scaling laws) were obtained that were well satisfied for all of the phase transitions.

The phenomenological theory of scale invariance, developed in this fashion mainly by Kadanoff in USA [Kadanoff 1966; Kadanoff 1967], and by A.Z. Patashinskij and V.L. Pokrovskij in Russia [Patashinkij 1966], to mention only the most relevant works for our approach, thus worked quite well. The microscopic formulation of the phenomenological scaling theory was to be found. With this transformation of the length scale, from which the invariance of systems near the critical point was hypothesised, all of the variables are eliminated that are irrelevant to the description of the critical phenomenon, and which obfuscate the description in the original model. Only the few relevant variables needed to describe the critical phenomenon in the scaled model are instead highlighted.

Two different aspects of universality would give rise to such microscopic formulation of the scaling theory: with the renormalisation group (i.e. field theory normalisation group) that in 1969, together with Gianni [Di Castro 1969b], we introduced into the study of critical phenomena, and with Kenneth Wilson's renormalisation group of 1971 [Wilson 1971a; Wilson 1971b]. Wilson was then awarded the 1982 Nobel Prize precisely for the theory of critical phenomena which followed from it.

L. B. Which are these two aspects of universality? And how are they related to the microscopic formulation of the scaling theory?

C. D. C. Given that near the critical point correlation is established at large distances, all what is occurring at short distances becomes inessential, and in fact obscures the relevant aspects. If the degrees of freedom specifying the system at the microscopic level can be eliminated at short distances regrouping them on ever-increasing scales, the model is continuously changed until the properties of the critical phenomena become evident and easily describable, in the asymptotically scaled model. Wilson succeeded in systematically implementing this idea of Kadanoff, of regrouping the variables, and built a complete theory of critical phenomena.

A second aspect is related to observing that various physical systems show the same critical behaviour. All of the parameters that specify and diversify the systems at the microscopic level – the strength of interaction or the coupling constant, for example – must be irrelevant for the description of critical behaviour, except when they assume values which change the symmetry properties of the system and introduce for instance anisotropy. Every model system is equivalent to another model system, in terms of critical phenomena, as long as in changing or even eliminating the variables considered irrelevant, the variables considered relevant are rescaled. Deviation of critical temperature and the order parameter are relevant variables, while the values of the coupling constants are examples of irrelevant details that microscopically specified the system.

A correspondence can be established with field theory, considering the order parameter as a classic field and, in that case, the deviation of the temperature from its critical value appears as mass. Renormalisation group theory [Gell-Mann 1954; Bogolyubov 1959; Bonch-Bruevich 1962] allows us to change the coupling constant, as long as we renormalise the field (wave function renormalisation) and the mass, taking care in this way also of the singularities appearing in perturbation theory. By rendering the group equations independent of the coupling constant, we obtained a scaling theory. For a long time, with Gianni we tried to calculate not only the scaling laws, but the critical behaviour in the various systems, with particular resummation of terms in renormalised perturbation theory, until we decided, in 1969, to publish this first result on the derivation of scaling theory [Di Castro 1969b].

Fig. 1. Varenna 1970, *Summer School on Critical Phenomena*, organised by M.S. Green. Among the other participants are visible: L.P. Kadanoff (on the left, second row, wearing a striped T-shirt), G. Jona-Lasinio and C. Di Castro (both at Kadanoff's left and at M.S. Green's right), R.B. Griffiths, G. Stell, J.V. Sengers, M.E. Fisher, K.A. Müller, L. Peliti, M. D'Eramo, G. Ciccotti, F. De Pasquale, F.P. Ricci, K. Kawasaki, K. Binder, P.C. Hohenberg, M. Cassandro (courtesy of Società Italiana di Fisica).

L. B. Did you have an opportunity of discussing such results within an international audience?

C. D. C. In 1970, there was a summer school on critical phenomena at Varenna, organised by Melville S. Green from Philadelphia, who had immediately grasped the importance of our work and asked us to give lectures on the subject [De Pasquale 1971]. There, we reported on our progress up to that point, that is the derivation of scaling theory under the hypothesis of elimination of the coupling constant, and we demonstrated – together with Ferdinando De Pasquale – the connection to the works that had been done in Russia at the same time, by A.A. Migdal and A.M. Polyakov, who had built a renormalised theory of critical phenomena, investigating the repetitive skeleton structures of perturbation theory. No other reference to these works was made on this occasion. This Varenna school provided a complete picture of the critical phenomena as developed at the beginning of seventies [Green 1971]. Among others, Fisher and Kadanoff were lecturing at the school (see Fig. 1).

L. B. And what was the reaction to your cross-fertilisation between field theory and statistical mechanics?

C. D. C. It was rather cold. It may have been our fault. Perhaps we should have been more detailed and specific in establishing the connection between field theory and statistical mechanics, not giving it for granted. We didn't consider the fact that condensed matter and statistical mechanics physicists were not ready for our ideas.

L. B. Did they voice any specific criticism?

C. D. C. Not really, as far as I remember. They simply maintained an air of extraneity. Consider, however that in the fall of 1969, David Mermin invited me to present our work at Cornell, where no one was even talking of renormalisation group at the time. Before the seminar, David took me to Fisher's office and introduced me, saying "Carlo is here to tell us about his new approach to critical phenomena". Fisher replied that he was familiar with the work, because he had heard Jona speak in Paris, and that it was "either trivial or wrong". Three years later, he was working on the renormalisation group full time. He never accepted that we had paved the way.

Something that Wilson did in his first article on scaling derivation, in the comprehensive review article that he wrote with J. Kogut in 1974 [Wilson 1974], and in his 1982 Nobel lecture published in *Reviews of Modern Physics* in 1983 [Wilson 1983], in which he noted that Ben Widom had asked him to organise a seminar on renormalisation group in the fall of 1970. Widom, one of the initiators of scaling, was interested to understand the renormalisation group, that we had proposed for the study of critical phenomena, and no one in his group could follow our work. Wilson also noted that in the course of preparing his lectures he identified the need to provide explicitly calculable examples – something that we too were working on, with no particular success. In 2002, as part of the MIT Physics of Scale project, designed to provide an online history of contemporary science, Babak Ashrafi and Sam Schweber conducted a number of interviews, including one with Gianni and one with me, which has never been published on the site. Among the interviews already online there is the one with Widom. When asked explicitly about my visit to Cornell or his request to Wilson, he simply commented "I don't remember".

L. B. And Wilson?

C. D. C. As I just said, Wilson often asserted that that's the way the story went... So what actually happened? Let's go back a little. David Mermin was on sabbatical in Rome in 1970–1971, and shared my office. One day, I came into the office and he said to me: "Look Carlo, I just got this from Cornell. It's a preprint by Wilson, in which he also applies normalisation group to critical phenomena". He showed me the preprint, dated December 1970, published in 1971 [Wilson 1971a], in which Wilson – analogously to the work we did in 1969 – reformulated Kadanoff's scaling theory in the form of renormalisation group equations, subjecting the formulation of block variables to critical analysis. This first article was followed by another, still in 1971 [Wilson 1971b], in which Wilson, for the first time, achieved an explicit process of elimination of degrees of freedom that produced recurrent relations (renormalisation group flow equations) among the parameters that characterise the system. These equations, when numerically solved, predicted for the first time a correct quantitative critical behaviour of physical quantities that differed from the classic behaviour predicted by mean field theories, such as van der Waals' equation of state of real gases or Weiss' mean field theory for magnets.

L. B. What was wrong with these theories?

C. D. C. In these mean field theories, which Landau unified in a single theory [Landau 1937a;Landau 1937b], introducing the concept of order parameter, the interaction effect on a given microscopic element is considered on average, thus ignoring the fluctuation effects that are so important in critical phenomena. By the way, in 1969, it was precisely by rendering Landau's theory compatible with the fluctuation effect up to the critical point and thus dressing the parameters of the theory, that I – together with J.A. Tyson and F. Ferro-Luzzi [Di Castro 1969c] – phenomenologically obtained a renormalised theory that would have its microscopic counterpart in the group. It can be demonstrated that were we to live in a world with more than four dimensions, rather than in a world with three dimensions, the fluctuations would lose their importance and would be negligible with respect to mean values of the corresponding physical quantities, even at the critical point. In such a case, the classical theories would be valid. Reducing the space dimensions, the number of particles in direct interaction with a preassigned particle is reduced and fluctuations from average increase, in particular approaching the critical point the fluctuations of the order parameter and of the other relevant quantities become greater than their mean values and ultimately diverge.

In 1972, also Fisher began to explore critical phenomena via the renormalisation group, and – together with Wilson – made a crucial progress [Wilson 1972a; Wilson 1972b]. Starting from four dimensions that distinguish the classical theories from the world in which the fluctuations dominate the physical quantities near the critical point, Fisher and Wilson developed what is known as the "epsilon expansion". That is an unusual expansion in terms of the spatial dimensionality d, where $\epsilon = 4-d$ is the small expansion parameter. With the group, they built an asymptotically rescaled model at $d = 4 - \epsilon$, by introducing perturbative corrections to group transformation equations, starting from their expression in four dimensions, where all is known and readily computable, because mean field theories are valid and correspond to free field theories.

L. B. Which is in your opinion the relevance of the ϵ-expansion and how does it fit to your approach?

C. D. C. First of all, notice the new aspect of the ϵ-expansion: it is a perturbation on transformation, not on the physical quantities which would result in singular contributions. The importance of the epsilon expansion lies less in the fact that, for $\epsilon = 1$, critical behaviours are obtained that are in good agreement with the experimental data, but rather in the fact that it is possible to obtain analytically explicit probability distributions that are compatible with the critical behaviour of physical systems and anomalous with respect to the standard distributions of the fluctuations. It is not true in critical systems that every sufficiently large part of the system will have a mean behaviour independent of the rest, the basic principle of standard statistical approach. Collective phenomena are not merely the superposition of microscopic events, and as the number of elementary constituents grows, new aspects emerge – in which the statistical elements, like the anomalous distribution of the fluctuations, dominate. The statistical aspects in this case are thus far more important than the dynamic aspects of many body theory, that allowed to represent each specific system in terms of a gas of quasiparticles with trivial statistical mechanics.

It was immediately clear that "epsilon expansion" could be obtained with our method, as I published in a letter [Di Castro 1972] that same year with the evaluation of the critical behaviour for the superfluid transition. In the epsilon expansion, the group equations of field theory are more manageable with the available technical apparatus of Feynman diagrams, and these would in fact be mostly used for the calculation of the critical behaviour of a wide variety of systems, as developed at length by E. Brezin, J.C. Le Guillou and J. Zinn-Justin, see e.g. [Brezin 1973].

L. B. Was the renormalisation group a deep change of strategy?

C. D. C. Renormalisation group theory does not change the fundamental laws of physics, but it certainly changes the paradigm within which many problems are approached. Before the renormalisation group, problems were solved either exactly (possible only in very few models and, at most, in one or two dimensions) or approximately in perturbation theory – inapplicable in the case in question. The methodology changed. We no longer sought to resolve a model more or less approximately, but to transform the original model in such a way as to highlight the specific properties of the critical system. By reducing the number of variables while rescaling the system, passing from infinite, strongly interacting degrees of freedom to few degrees of freedom in the transformed model, capable however, of explicitly emphasising the properties at a critical point. Problems were thus no longer solved by doing approximations on a model in which the individual microscopic elements interact with one another, but by transforming it into models that are equivalent for those aspects considered relevant to the problem under discussion. This poses no fundamental problems, as it is instead often implied in dealing with "complexity" as a discipline.

The Gibbs method in statistical mechanics – and the reductionist schema for the passage from the simple microscopic to the complex macroscopic as well – do not lose their validity, if the method is applied correctly in each specific case, that is eliminating the irrelevant variables and properly selecting those few that are relevant to the universal description of critical phenomena. The same principle was actually applied to derive hydrodynamics and nobody claimed in that case the death of reductionism.

A general method was thus introduced, whereby explanatory paths could be identified for the interpretation of complex collective phenomena. For a complex system, the goal is to identify those aspects that were relevant to the problem at hand, and the corresponding variables upon which to act. The schema is general but the procedure must be specific and requires dedicated competences, for this reason I am skeptical about complexity as a discipline *per se*.

L. B. Earlier, you mentioned having gone to see Fisher. Was there any interaction between your work and that of the others?

C. D. C. Not much, I must say. We were greatly influenced by Kadanoff's approach, without a doubt, but the process was generated entirely within our institute. I don't think our work influenced Wilson, beyond what I have noted above. The contact we had with Wilson's approach occurred a posteriori, as I mentioned, when Wilson's preprint arrived in Rome in 1971; of course we then met in many occasions. It was at that point that we began a process of comparison. Gianni presented at the 1973 Nobel Symposium at Lerum the mathematical conditions that he had elaborated for the asymptotic equivalence of different realisations of the group transformations. In the same year I had the first direct contact with Wilson at the first large international conference on the renormalisation group and critical phenomena, organised – like the Varenna 1971 School – by Mel Green in Philadelphia in 1973 [Gunton 1974]. Together with C. Domb [Domb 1976], Green also edited a series of books entirely dedicated to the renormalisation group approach to critical phenomena. Volume VI of the series included a review [Di Castro 1976] that Gianni and I had written, of our work at that time.

At Philadelphia, I gave a talk [Di Castro 1974a] which includes a brief presentation of a work, developed with Gianni and Luca Peliti [Di Castro 1974b]. In this work we had made an important step forward, introducing the group through the invariance of thermodynamic potential, from which – by means of functional derivatives – it was possible to obtain the group equations for all physical quantities. Asymptotic equivalence in the use of various group transformations, differing in the process of elimination of irrelevant transients, was discussed at the conference symposium which I attended along with (to the best of my recollection) Wilson, Kadanoff, Fisher, Eduard Brezin, Paul Martin, Kurt Symanzik and Elihu Abrahams. This problem was alluded to in the transcripts of some discussion sessions that followed the presentations, which are, in my opinion, the most important part of the proceedings [Gunton 1974].

L. B. It was an opportunity to compare ideas. . .

C. D. C. Indeed, I specifically recall the discussions at that conference on asymptotic equivalence between the various transformations, precisely because it is this equivalence that Fisher never accepted. I tried to remind him of the discussions at the Philadelphia conference, which we both attended, in an exchange we had in 1997, following his request for suggestions regarding a historical article he was working on. I also stressed that of the two types of problems encountered in critical phenomena when we introduced the use of the group – the explicit calculation of the parameters controlling the critical behaviour of physical quantities, the so-called critical indexes, and the conceptual bases of the theory that explains universality and scaling derivation – the second had been addressed and partially resolved by us already in 1969.

L. B. It appears that in your opinion the conference in Philadelphia was a great event in physics...

C. D. C. It was the first large conference attended simultaneously by physicists involved in critical phenomena, renormalisation group and the connection between statistical mechanics and field theory. The organiser Mel Green understood from the very beginning (recall he was also the director of Varenna school) the importance of the method that we had proposed. Mel died in 1980. In an article I wrote for the book in his memory [Di Castro 1981], I offer a brief account of the renormalisation group, in which I describe how the field theory group we had introduced and Wilson's group corresponded to two realisation of different versions of universality, as I have mentioned above. In the same article, I explain an important aspect of our work. The renormalisation group mechanism works as long as the starting point is known, that is the order parameter and the invariants that can be constructed from it. In terms of these invariants it is possible to construct a field theory model (now usually called Landau-Wilson model) with the order parameter as the classic field on which renormalisation group transformation acts. It is thus fundamental to know the order parameter and the spontaneously broken symmetry that characterise the change in the system's form of aggregation as it passes from the high-temperature disordered phase to the low-temperature ordered phase. In cases in which the order parameter is hard to identify or it assumes a complicated form, it is difficult to know how to proceed to renormalisation, and so we must resort to the system's symmetry for help. One of the cases in which it is hard to identify the parameter unequivocally attracted my attention at the end of the 1970s, that is the metal-to-insulator transition generated by electron interactions – a mechanism that differs significantly from the normal way of understanding insulators by means of band filling in the free particle schema.

It was at that time that my decades-long working relationship with Claudio Castellani began. Claudio's deep understanding of the physical problems made him a magnificent partner and a mainstay of the entire group.

7 Correlated electronic systems and cooperation with Claudio Castellani

L. B. How was Castellani related with your interest in the metal-to-insulator transition?

C. D. C. Claudio Castellani graduated in 1972 under the supervision of Calogero R. Natoli from the CNEN-Casaccia. Since his laurea thesis Claudio was actually involved in the study of metal-to-insulator transition of a number of vanadium oxides that, upon compressing or doping, pass from an insulator to a metallic phase. This type of transition is referred to as a Mott transition. According to Sir Nevill Mott, it was Peierls who pointed out the strong interaction and thus the strong correlation between the electrons, as a cause of the insulating behaviour of various materials that, having a non filled band, according to the band schema, should be metals. The problem of strong correlation in electronic systems is an extremely complicated one, involving a wide range of materials, which later acquired particular importance with the discovery of high-temperature superconductors.

L. B. What kind of approach did you use to tackle the problem?

Fig. 2. With C. Castellani during our visit to the Aspen Center for Physics, August 1994.

C. D. C. In the late 1970s, we started to take an interest in this problem focusing on two dominant aspects that emerged already in the study of critical phenomena: the relevance of the symmetry properties and the idea of replacing the original microscopic model with an effective model in terms of the variables relevant to the problem under study. The simplest microscopic model suited to the study of metal-insulator transition was introduced by J. Hubbard in 1963, and includes a kinetic hopping term t_{ij} of the electrons from the site i to site j of a lattice, that allows the electrons to move favouring the metallic phase, and a local repulsive interaction on each lattice site between electrons with opposite spin, that tends to localise them, impeding motion and favouring the insulator phase. The competition between the two terms should result in the metal-insulator transition. Despite its apparent simplicity, the model could not be adequately solved, especially in the parameter regime of interest, in which the repulsive localising part is comparable to the kinetic part. Furthermore, the direct use of the group was prevented by the quantum aspects of the interacting fermions model. In a series of studies with Claudio we clarified in this case the difficulties intrinsic to the application of the renormalisation group [Castellani 1979a; Castellani 1982].

Finally, using only the symmetry properties of the original model, we – together with Julius Ranninger and Denis Feinberg of Grenoble – transformed the Hubbard model into a model [Castellani 1979b] expressed in terms of the system's invariants, constructed with the physical variables, essentially charge density and spin. The new model allowed us to discuss metal-insulator transition analogously to the separation transition of a binary mixture plus some intrinsically quantum terms. In the 1990s, G. Kotliar and A. Georges [Georges 1996] approached Mott transition using dynamical mean field theory, which – although a mean field theory – takes the effects of local quantum fluctuations into consideration. Their results showed similarities to a number of the properties (in particular the order parameter) we had identified with this transformation based only on the invariant properties of the Hubbard model. I would like to point out that the use of the symmetry properties to reformulate specific complex problems (disordered interacting electron systems, quantum fluids) in terms of physical variables and, at the same time, condition and identify the possible renormalisations, was a constant element in my work.

8 Disordered electronic systems

L. B. When did you start to work on disordered interacting electron systems?

C. D. C. Beginning in 1983, we actually used symmetry properties in the study of another important unsolved problem in the field of condensed matter: the generalisation to interacting electrons of the metal-insulator transition resulting from the localisation due to disorder of non interacting electrons – the Anderson transition. Besides the Mott transition, another possibility is the metal-insulator transition induced by disorder. Experimental realisations of disordered electronic systems are doped semiconductors (e.g. doping silicon with phosphorus impurities) and amorphous alloys as niobium-silicon or gold-germanium.

In order to better understand the underlying ideas, let me introduce first the non interacting systems. The electrons during their motion collide with the impurities, converting the ordered flow of charge into disordered motion, giving rise to electric resistance. Using the independent-particle model for electrons in metals, one arrives at the classical Drude theory for charge transport. On the basis of Landau's theory of quasiparticles for fermions (normal Fermi liquid theory), as long as temperatures are not too high, the physical quantities of both equilibrium and transport, if expressed in terms of quasiparticles, take the form valid for a gas. The interactions effectively manifest themselves only through a change of the values of parameters, entering quantities like spin susceptibility, specific heat, and diffusion coefficients, with no modifications of their form. When disorder is increased it was found instead, particularly by Russian physicists at Landau's Institute [Gorkov 1979], that quantum interference effects invalidate the classical theory with singular corrections (logarithmic in two dimensions) in perturbation theory. It was P.W. Anderson [Anderson 1958] however, who, already in 1958, redefined the problem by changing the paradigm. Previously, the disorder effect had been considered on average with the electrons in delocalised Bloch states, giving rise to a continuous increase of the resistivity in the metallic state and no transition. Anderson showed how strong disorder could, in fact, act as an electron trap, causing the electrons to localise around the impurities. Their movement is thus impeded and the system becomes insulating (Anderson localisation transition). The control parameter for this transition is disorder, which is inversely proportional to the conductive capacity of the material, namely its conductance. The group equation for the conductance, taking the singularities of the perturbation theory into account, controls its evolution and provides the scaling theory for this transition [Abrahams 1979; Wegner 1976]. The quantum corrections are so strong in reduced space dimensionality that already in two dimensions the charge carriers localise, no matter how small is the disorder and no metallic state can exist.

L. B. Up to now you described a situation in which disorder has been introduced in a system of independent particles. What happens when you introduce interactions between electrons which, I believe, was the core of your work?

C. D. C. We might actually think that not much would happen, and that Anderson localisation theory would still prevail within an independent quasiparticle model, based on the Landau normal Fermi liquid theory, which effectively takes interaction into account. However, B.L. Altshuler and A.G. Aronov, in Leningrad, saw that singular corrections to the physical quantities (like spin susceptibility or specific heat) derived not only from the disorder effect on the free electrons – a problem resolved by Anderson localisation – but also from electron-electron interaction in the presence of disorder [Altshuler 1979; Altshuler 1980]. At that time (the beginning of the process of liberalisation in the Soviet Union), I was in direct contact with the Russian school, because I worked with Landau's group on the organisation of two conferences – one in Rome (1984) and one in Moscow (1985) – as per agreement between our department and Landau Institute. Electron-electron interaction in the presence of disorder leads to singular corrections similar to those warranted by disorder alone. The question arises, what other parameters, beyond the inverse conductance – the parameter that controls disorder – must be introduced in order to control, in this case, the metal-insulator transition? The question is far from trivial. Starting from 1983, together with Claudio Castellani, Gabor Forgacs and Eugenio Tabet [Castellani 1983] we sought to understand which field theory and how renormalisation group could resolve the singularities found in the perturbation theory, and to construct a localisation theory in the presence of electron-electron interaction.

L. B. What happens to the interaction in the presence of disorder?

C. D. C. Physically speaking, it is very clear why the disorder reinforces the interaction. In ordinary metals, local variations in charge are rapidly screened. In metals in the presence of disorder, the electrons propagate by diffusion – a particularly slow phenomenon at large distances. In a strongly disordered system, therefore, the electrons interact each other for longer time and distance, and, as we at the end discovered, all the Landau parameters entering the physical quantities, depend strongly on the dynamics of diffusive motion, that, so to say, acquires a strong dependence on the frequency and the wave vectors associated with it. The disorder thus modifies the mutual interaction between the electrons, and thus the Landau parameters, which may then depend on the rescaling of time and position. This is at the origin of singular corrections to the physical quantities in perturbation theory.

L. B. How did you take these singularities in this anomalous phase transition into consideration?

C. D. C. As I have already said, the symmetry properties are of precious help in dealing with complex systems. We began to construct a renormalised perturbation theory, allowing ourselves to be guided in this complex structure, by implementing all the conservation laws and the corresponding invariant symmetries (technically speaking, gauge invariance and related Ward identities) that, by imposing stringent conditions on the connection between different quantities, provided us with a control of the terms we needed to take into consideration in the renormalisation, at any order of perturbation theory. We thus obtained that, in addition to the conductance that characterises the disorder, the additional couplings are the interactions with large and small momentum transfer, both dressed or renormalised by the diffusion modes [Castellani 1983].

At the same time, at MIT, Gary Grest and Patrick Lee [Grest 1983] were pursuing the same idea of constructing a renormalised perturbation theory for disordered interacting electronic systems. However, they had only identified one variable, resulting from the interaction – a linear combination of the previous ones. The control we effected through the invariance conditions bore fruit; additional terms generated the two types of coupling separately, although they were initially combined. We corresponded and eventually decided to join forces. At this point, we had the difficult task to proceed in the construction of a fully renormalised theory with three couplings in which the only small parameter was constituted by disorder [Castellani 1984a].

L. B. Who else in the world was approaching this problem?

C. D. C. In Soviet Union, besides Althshuler and Aronov in Leningrad, Sasha Finkel'stein [Finkel'stein 1983], at the Landau Institute, gave an important contribution. Rather than reconstructing renormalisation theory by appropriately controlling the perturbation theory constrained by symmetry properties and related Ward identities, he succeeded in reducing the system of interacting electrons in the presence of disorder directly into a field theoretic model. He introduced the model's specific renormalisations. Once again, however, through the conditions derived from imposing the symmetry properties, we succeeded – together with the MIT group headed by Patrick Lee – in constructing a renormalised perturbation theory [Castellani 1984a] with results similar to Finkel'stein [Finkel'stein 1984a]. We however also identified a small correction to Finkel'stein's renormalisation equations that, however, as I will clarify shortly, changed the system's response [Castellani 1984b; Castellani 1986a], a correction discussed by Finkel'stein himself [Finkel'stein 1984a].

More importantly, pursuing the discourse begun by Altshuler and Aronov [Altshuler 1983] and followed by Finkel'stein, we succeeded, with Claudio, in giving physical meaning to all of the renormalisations introduced into Finkel'stein's model [Castellani 1986b]. These results were also presented in the occasion of the eightieth birthday of Sir Nevill Mott [Castellani 1985]. The end result was surprisingly simple, from a physical point of view. Non-disordered electronic systems, as I have noted a number of times, are well-described by Landau normal Fermi liquid theory, which effectively takes interaction into account, by modifying the expressions of specific heat, compressibility and magnetic susceptibility valid for free electrons, with Landau parameters. In the presence of disorder, interacting electronic systems are still well-described by Landau's theory, but, as already stressed, the Landau parameters become now scale dependent. When constrained by the conservation laws, all the renormalisations introduced by Finkel'stein are shown to coincide with the Landau parameters dressing the physical quantities indicated above. These parameters flow according to the renormalisation group equations and when the spin fluctuations are at least partially suppressed, as in the presence of magnetic field [Castellani 1984a; Finkel'stein 1984b], of magnetic impurities [Castellani 1984a; Finkel'stein 1984b], or spin orbit coupling [Altshuler 1983; Castellani 1984c], they exhibit metal-insulator transitions induced by the interaction in presence of disorder, that differ from both the Mott and Anderson transitions. Sandro Sorella had in the meanwhile actively joined us in our work.

L. B. Which was the change in the response with respect the original Finkel'stein 1983 paper?

C. D. C. The general problem of a transition in a disordered interacting system in the presence of non-magnetic impurities is still matter of debate. Contrary to the first contribution of Finkel'stein [Finkel'stein 1983], in which the group transformation can be carried up to an infinite value of the rescaling parameter, the group transformation cannot be iterated to the far end [Castellani 1984a; Castellani 1984b; Finkel'stein 1984a; Finkel'stein 1984b; Castellani 1986a]. At a finite value of the rescaling parameter the couplings, in particular the one related to the spin susceptibility diverge, signalling the formation of local magnetic moments [Castellani 1984b]. The introduction of self generated local moments would drastically change the physical picture of this transition and reopens the problem. On the contrary, as mentioned above, when spin fluctuations are suppressed, the group equation iteration can be carried out to an infinite value of the rescaling parameter and the metal-insulator transition can be properly described.

L. B. How were Finkel'stein's and your theoretical results accepted?

C. D. C. Rather well, especially in Finkel'stein's formulation. However, beyond a limited circle, many found it difficult to enter into the new paradigm and to understand the results obtained by techniques that were a little out of the ordinary for condensed matter physicists. Even Mott – with whom I discussed the matter personally at a number of conferences – wrote to me in October 1984, asking for clarifications. The question was how there could be a continuous metal-insulator transition. Indeed, according to a very general criterion (Ioffe-Regel criterion) electrons cannot propagate when, due to disorder, their mean free path is lower than their mean distance. When applied to the classical Drude formula for electrical conductivity in terms of mean free path, a minimum metallic conductivity is obtained below which conductivity cannot drop and the metal-insulator transition, if it exists, cannot be continuous. The criterion was correct, but it could not be applied to the Drude formula, not valid in the presence of singular correction terms. The anomalies resulting from the combined effect of disorder and interaction had to be treated with the renormalisation group.

In the case of Anderson localisation in two dimensions the effect of disorder as a trap is so strong that the electrons are always localised regardless of the entity of disorder. Another interesting result was that, in the case of interaction, an anti-localisation term is generated allowing the possibility of a metallic state in two dimensions [Castellani 1984b; Finkel'stein 1984a; Finkel'stein 1984b; Castellani 1986a].

The technique for the preparation of two-dimensional electronic systems was improved toward the end of the last decade of the last century and, for the first time, metallic behaviour [Kravchenko 1995; Kravchenko 1996] could be found in a two-dimensional system, and a metal-insulator transition the nature of which is however still a source of controversy [Kravchenko 2004].

In conclusion, a complicated renormalisation theory can be resolved in a Landau theory with Landau parameters that flow according to the group equations. At a conference in Tokyo in 1987, where I presented a summary of our results [Di Castro 1988], a number of papers compared well Finkel'stein's and our theoretical results against the experiments. It was however clear that in the general case of non magnetic impurities, like in silicon doped phosphorus, the strong divergences of the spin susceptibility and the specific heat present at the approximation considered, preventing the approach to the metal-insulator transition, required further theoretical and experimental work. In the meantime, however, the discovery of high critical temperature superconductors on copper oxides exploded.

9 High-temperature superconductivity

L. B. What was your reaction to the discovery of high-temperature superconductivity, in 1986?

C. D. C. At the time, the problematics associated with disordered electronic systems were still a very hot topic, both theoretically and experimentally, but they were overshadowed by the new problematics, although we did continue to work on them for a number of years. J.G. Bednorz and K.A. Müller's discovery [Bednorz 1986] of superconductivity in a barium-doped compound of lanthanum and copper oxide with T_c in the thirty Kelvin range, which had been the chimera of experimental physicists for decades, was quickly followed by discoveries of various families of cuprate superconductors, with critical temperature values well above the liquefaction temperature of nitrogen – far more accessible than the helium required to cool materials below the critical temperatures of classic superconductors. The importance of electron correlation for the study of these materials became immediately apparent, in particular under the input by P.W. Anderson [Anderson 1987]. Those who, like us, had been working on strongly correlated electron systems, could not escape the temptation to try to work on this new type of superconductor.

L. B. Why is the strong correlation between electrons important in this case as well? And what are the essential characteristics of these materials?

C. D. C. All of these materials are made up of planes of copper and oxygen interspersed with layers of lanthanum, yttrium, barium, etc., depending on the various families of cuprates. In their stoichiometric composition e.g. La_2CuO_4, these materials are antiferromagnetic insulators, despite the fact that they have one electron hole per CuO_2 unit cell in the copper-oxygen plane, with unfilled band, and should be metals according to band theory. Hence the need for a strong correlation between charge carriers – in this case, electron holes. It was thus considered theoretically valid to use the Hubbard model for these systems as well. If, for example, in the first material discovered, the trivalent lanthanum is replaced with bivalent barium or strontium, e.g. $La_{2-x}Sr_xCuO_4$, that is chemical doping x, the number of holes increases in the CuO_2 plane and the system becomes a metal, albeit a bad metal (due to paucity of charge carriers) that, nevertheless, becomes a superconductor when the temperature is lowered.

In the metal phase, a strong anisotropy is measured in the electrical conductivity that, in the planes of copper and oxygen, exceeds the transversal conductivity by orders of magnitude. The systems in question thus exhibit strongly correlated, quasi-two-dimensional electron holes. In classic superconductors, the necessary attraction for the formation of electron pairs is mediated by phonons, the elastic excitations of the crystal lattice, as indicated by the so-called isotope effect, whereby the critical temperature for the onset of superconductivity depends on the isotopic mass of the lattice ion and therefore on the lattice excitations. The isotope effect is either absent or anomalous in the new superconductors. In addition, the metallic phase is not in line with ordinary metals and the predictions of normal Fermi liquid theory.

An international debate thus ensued among theoretical physicists, whether a strongly correlated hole (or electron) system, with an occupation of approximately one hole per lattice site of a quasi-two-dimensional system can become a superconductor, what the new paired states will be, and by what mechanisms they might be formed. At the same time, another, perhaps even more stimulating theoretical problem, presented itself: understanding the anomalous metallic phase to which the superconductivity was linked. I embarked upon this twofold adventure together with Claudio Castellani, Marco Grilli, Sergio Caprara, undergraduates, doctoral and post-doctoral students who worked with us from time to time, and particularly Walter Metzner, who was with us for three years, and is now director of one of the departments at the Max Planck Institute for Solid State Research in Stuttgart.

L. B. How did you approach the problem?

C. D. C. First of all, quasi-two-dimensionality had to be an important factor. The other question we asked ourselves was how could a system in which the interaction between charges was so strongly repulsive generate the pairing between the

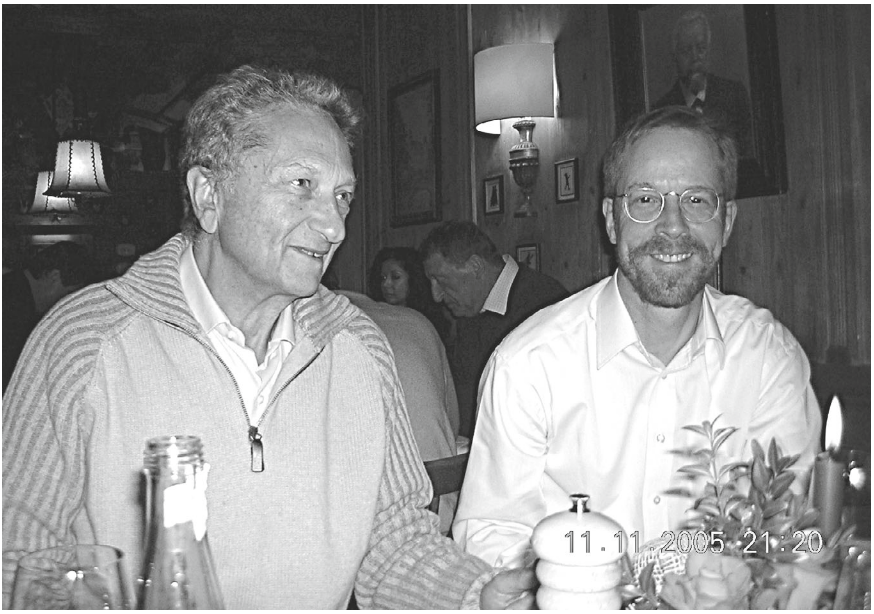

Fig. 3. Carlo Di Castro and Walter Metzner in Munich, on their way back from the conference on *Properties of cuprates superconductors*, Schloss Ringberg, 7–11 November 2005.

charges necessary to produce superconductivity? It occurred to us to relate to the phase separation, as it occurs in simple fluids, where there is a separation between the low specific volume liquid phase and the high specific volume gas phase, which coexist at temperatures and pressures below the critical values. In the case of cuprates, the separation would occur between charge-rich metallic or superconductive regions, and insulating and antiferromagnetic charge-poor zones. The strong local repulsion between charge carriers, reducing their mobility, which tends to render the system homogeneous, favours the possibility of phase separation. Indeed all of the models with strong local repulsion that were introduced to represent the copper-oxygen planes show phase separation in charge-rich regions and in charge-poor regions, as shown for instance by V.J. Emery (with whom I had interacted at Birmingham, where he was a lecturer when I was a student) and S.A. Kivelson starting in 1990 in a paper on the so-called t-J model derived from the Hubbard model and containing the kinetic hopping term for the few mobile holes and a spin-spin interaction term for the anti-ferromagnetic background [Emery 1990].

L. B. What were your further steps in following the track of the phase separation idea?

C. D. C. Besides the phase separation in various other models, we discovered that whenever phase separation is present in a model, then pairing occurs in a nearby region of phase space, thus suggesting a possible connection between the two phenomena [Cancrini 1991; Grilli 1991]. Charge inhomogeneity appeared to be the simplest mechanism by which to connect the strong repulsion necessary for an insulating anti-ferromagnet at low doping with the necessary attraction for superconductivity at higher doping. If we add a long-range Coulomb interaction between the charges, however, the electrostatic energy cost for the macroscopic separation of the charge in regions with different charge densities becomes too high. Phase separation on a macroscopic scale is thus frustrated. We did find with Claudio and Marco [Castellani 1995], that under certain conditions, starting with a homogeneous correlated metal at high doping, charge inhomogeneity can be realised locally with a modulation determined by the balance between the gain in energy due to the system's tendency toward phase separation in the absence of long-range Coulomb interaction, and the cost due to the interaction itself (frustrated phase separation). This occurs via an instability of the homogeneous liquid along a line in the temperature vs. doping plain ending at $T = 0$ in a quantum critical point nearby optimal doping [Andergassen 2001]. The incommensurate charge density wave physics may well evolve onto the spin-charge stripe deeper in the charge-ordered phase as a consequence anharmonic effects. Emery and Kivelson in a series of papers beginning with [Löw 1994] arrived at the same concept, of frustrated phase separation starting from the low doping zone near the antiferromagnetic insulating phase, in which the few charges present are expelled from the antiferromagnetic substrate and preferred to align themselves in stripes. It was then observed experimentally, in those years, that in the low-doped zone, a charge modulation is effectively present in what is called the stripe phase [Tranquada 1995].

L. B. What was the initial reaction to the idea of charge inhomogeneity?

C. D. C. When we first spoke of charge inhomogeneity, people thought we were from the moon. At the Interlaken conference of 1988, following my remark on the possible interconnection between different charge components that could exist in these materials [Castellani 1988b], an important member of our scientific community asserted that simple calculations show it to be impossible. Alex Müller, however, believed us from the start – to the point that he organised conferences on the subject (at Erice in 1992 – see Fig. 4 – and at Cottbus in 1993), in the proceedings [Müller 1993; Müller 1994] of which summaries of both our work and that of Emery and Kivelson from that early period appeared. This idea of charge inhomogeneity corresponded, at least in part, to the idea of strong polarisation that guided Müller in his discovery. I corresponded with Alex extensively on the subject, for many years. I presented the "elogio" in the occasion of his *Laurea Honoris Causa* in Rome in November 1990 (see Fig. 5) and later I was called in Cottbus for the analogous ceremony.

In our approach, by combining the strong repulsion to reduce the kinetic energy term, with a residual attractive interaction mediated by the lattice, the onset of the charge inhomogeneity phase occurred at $T = 0$ as a critical instability [Castellani 1995] of the correlated Fermi liquid nearby the so-called optimal doping, the doping level corresponding to the maximum critical temperature for the onset of superconductivity. This quantum critical point is the end point of a charged density wave instability line $T_c(x)$ of the homogeneous metal in the temperature vs. doping x plane [Andergassen 2001]. $T_c(x)$ should track closely the onset temperature $T^*(x)$ of the anomalous metallic behavior in the underdoped region, the so-called pseudo gap phase. The fluctuations associated with this critical situation can replace phonons as the new mediators of superconductivity, that appears then as a stabilising phase against the onset of a true charge ordering. We thus found a mechanism that, from repulsion, allowed us to arrive at superconductive pairs and a description of the cuprate phase diagram [Andergassen 2001][3].

[3] Note added while editing the interview. Charge density wave, very hard to detect, has been finally directly observed by nuclear magnetic resonance in YBaCuO underdoped samples under high magnetic field [Wu 2011]; by high energy X-ray diffraction in the normal state of $YBa_2Cu_3O_{6.67}$ [Chang 2012], the intensity of charge ordering below the critical temperature is increasing with increasing the depairing magnetic field; by resonant X-ray diffraction [Ghiringhelli 2012] on $(Y,Nd)Ba_2Cu_3O_{6+x}$, with hole concentration of 0.09 to 0.13 per Cu atom suggesting an incipient charge density wave instability that, when fully ordered, competes with superconductivity.

Fig. 4. Erice, 6–12 may 1992, *Workshop on Phase separation in Cuprate Superconductors*, organised by K.A. Müller and G. Benedek. In the first row: C. Castellani (third from the left), M. Grilli, H. Monien, G. Benedek, K.A. Müller, C. Di Castro, A. Bianconi. In the second row, at Grilli's right, V.J. Emery and E. Sigmund, at Grilli's left.

10 Anomalous metallic phase in high temperature superconductors and use of renormalisation group in stable quantum liquid phases

L. B. And for the metallic phase above critical temperature?

C. D. C. The same critical fluctuations that could provide the mechanism for pairing, could also mediate an effective interaction between holes sufficiently strong to generate anomalous behaviour of the non-superconductive metallic phase.

In order to explain the anomalous behaviour of the metallic phase, Anderson [Anderson 1990a; Anderson 1990b] suggested transferring the anomalous behaviour present in a metal in one dimension to two dimensions (the copper-oxygen planes). In one dimension, the charges can only move to the right or to the left, and if the diffusion potential is biased in the forward direction, the right and left charge cannot mix. Thus, beyond global charge conservation, there is also the separate conservation of the left- and right-movers. This imposes a further constraint with regard to ordinary global conservation, which, in the context of renormalisation group, allowed Walter and myself [Di Castro 1991; Metzner 1993] to reproduce in one dimension the anomalous transport behaviour of the so-called Luttinger liquid, that deviates from normal Fermi liquid theory, as it was first found by I.E. Dzyaloshinskii and A.I. Larkin [Dzyaloshinskii 1974].

Contrary to the Anderson suggestion, however, with Claudio and Walter we showed [Castellani 1994; Metzner 1997] that for dimensions greater than one, behaviour is always in line with Landau normal Fermi liquid theory. Deviations can only occur if the interaction is sufficiently singular, just as near a critical instability, where an anomalous interaction is mediated by the critical fluctuation modes. This corresponds to our findings regarding frustrated phase separation with critical charge

Fig. 5. Alex Müller is awarded the *Laurea Honoris Causa* from Sapienza University of Rome. Carlo Di Castro gave the award speech. November 1993.

and spin fluctuations, thereby identifying a single origin for the possible formation of pairs and anomalous metal [Castellani 1997a; Castellani 1998]. The demonstration with Claudio and Walter was obtained this time as well by applying the constraints imposed by the valid symmetry properties in one dimension and opportunely extended to dimensions greater than one – at all orders of perturbation. We recently demonstrated – together with Carmine Ortix and José Lorenzana – that while in three dimensions every local variation of charge density relaxes exponentially to the constant value at a distance equal to the screening length, in two dimensions the charge density decreases with a power of the distance and no limitation is induced in the characteristic dimension of inhomogeneity [Ortix 2006]. We thus have an explanation of why frustrated phase separation, as a physical phenomenon, is more frequent at low dimensionality and, in particular, why it can be viewed as a relevant phenomenon in cuprate planes. High-temperature superconductivity, with all of its implications, is still an open field of research, and remains a continued focus of our work. General consensus on a specific explanation has not been reached so far and will not be reached in short time due to the numerous phenomena involved.

L. B. Earlier, you referred to the problem of low-laying helium excitation, certain aspects of which had somehow remained unresolved...

C. D. C. This was, in fact, the solution to an old problem that had failed to attract attention, because it had gone out of style. It was instead considered very relevant in the 1960s of the previous century, see e.g. the paper by Gavoret and Nozières [Gavoret 1964]. It also is indicative of another relevant use of renormalisation group combined with the symmetry properties in stable quantum liquid phases besides the classical application in critical phenomena. Thanks to Bose condensation, the perturbation theory for a Bose system is singular, even in the stable liquid phase far from criticality. As I have already noted, an interacting Fermi gas also has a singular perturbation theory in one dimension, and is a stable liquid with anomalous metallic behaviour. In stable phases, far from criticality, all physical quantities must be finite, and eventual singularities must cancel out exactly at all perturbation orders. For this to occur, there must be special symmetry properties, as the separate conservation of left and right movers in the Luttinger liquid. Exploiting these additional properties, as we had done with Claudio and Walter for fermions, we managed – with Claudio, Giancarlo Strinati and Fabio Pistolesi – combining renormalisation group and the conservation laws related to gauge invariance for an interacting Bose gas, to close and resolve the group equations, thereby providing a solution to a decades-old problem [Castellani 1997b; Pistolesi 2004]. We derived a phonon like spectrum as within the approximated Bogolyubov solution, but now as exact low lying excitations from the ground state. We thus went from our original point of departure on the excitations of superfluid helium to the current state of affairs, finally closing the cycle. It was, in fact, in Gianni Jona's honour – on the occasion of his seventieth birthday [Di Castro 2004] – that I conducted an analysis of all of the problematic associated with the use of the renormalisation group and the symmetry properties in stable quantum liquid phases and in unconventional critical phenomena.

11 Formation of a school and politics of research

L. B. What teaching methods and professional training developed around this research?

C. D. C. Over the past decades, together with Claudio, we have managed to build an internationally-recognised school of condensed matter theory, the "Rome Group," as it is friendly referred to by our colleagues abroad. Over the years, we were joined by graduate and doctoral students, from Italy and abroad, including: Gabor Forgacs, Sergio Caprara, Roberto Raimondi, Sandro Sorella, Sabine Andergassen, Michele Fabrizio, Walter Metzner, Fabio Pistolesi, José Lorenzana, Andrea Perali, Lara Benfatto, Massimo Capone, Tilman Enss, and especially Marco Grilli, who, along with Caprara, Benfatto and Lorenzana, became permanent members of the group, taking part in and promoting numerous research initiatives.

We have introduced topics that had previously not even been touched upon in the department. In terms of teaching methodology, we have introduced in our group the practice of maintaining a constant balance, in all of our internal seminars and in the daily meeting at the board to carry on our research, between imparting scientific knowledge and providing professional training in research. When assigning a thesis, the argument was emerging slowly from the discussions at the board on the topics we were dealing in that period. In that way the students were becoming prepared researchers rather than simple executors.

In any case, since when I began my adventure in physics in the 1960s, both in terms of research and in terms of teaching, independently from our group, the department along the years became far richer and more diversified, encompassing areas beyond elementary particle physics, including those close to our own. Let it suffice to mention the contributions of Daniel Amit, who untimely passed away, Marzio Cassandro, Giovanni Ciccotti, Giovanni Gallavotti, Francesco Guerra, Gianni Jona-Lasinio, Giorgio Parisi, Luciano Pietronero, Miguel Virasoro… who have become leaders in their fields of research.

We could stop here, but, as an attempt to favor in general the formation of schools of teaching and research, I would like to mention an organisational experience I had, in the context of a general reform of the university system in Italy, which however was never completed (in Rome or in the country as a whole), for general problems of the country that are unfortunately persisting. I refer to my experience on the University Commission for Experimentation in Organisation and Teaching Methods (Commissione di Ateneo per la Sperimentazione Didattica e Organizzativa), which I headed for three years as president and vice-president starting in 1982, alternating with with two colleagues of other faculties.

We worked on the reorganisation of our colossal university[4] into more agile structures, such as departments, instead of numerous one-chair institutes pertaining to gigantic faculties, as it was mostly organized the university system still at that time. It marked the beginning of a great change, although, as I said, the expectations at the time were never fully realised. I'd like to quote part of an interview published on the journal *Rassegna sulla Sperimentazione Organizzativa e Didattica nelle Università* [Vol. 2 (2) (1983), pp. 95–98] I gave in 1983, at the end of my term as head of the commission, because, unfortunately, I believe that similar difficulties persist to this day. With regard to the reorganisation into departments, interdepartmental research and service centres, and with respect to the formation of schools and the technological innovation of the productive system of the country, I remarked as follows:

> The strategy was to introduce service centres and provide the university with a functional structure (particularly needed at the University of Rome, due to its vast size), while the research centres should connect various departments to facilitate interdisciplinary research, as well as productive cooperation with bodies outside the university and foster effective technological innovation. The latter [centres] may thus serve as the missing link in the Italian university structure, between basic and applied research… The proposed departmental organization should aim to organise the university culture so that the production of research, whether scientific or humanistic, will find its natural expression in the formation of schools. Research in Italy (topics, timing, methods) has always been conducted on the basis of considerations entirely internal to research itself, or rather to individual researchers or, at most, to the research groups to which they belong. It is thus clear that the aspects of research that most gratify the individual researcher's areas of interest, have constituted the greater part of research. The interest and motivation of single researchers should continue to be an important component of basic research, but these cannot be the only considerations, inasmuch as they favour the figure of the lone researcher in an ivory tower, over the possibility of forming of research schools. Through open exchange and the coordination of research programmes, the departmental structures should – without infringing upon the freedom of individual researchers – foster aggregation and, thereby, the formation of schools. Another

[4] In 2010 the number of students enrolled at Sapienza University was about 130 000.

aspect that should not be overlooked, in light of modern research-production models, is that much of research cannot hold its own in an international context, without a certain minimum level of aggregation.

Regarding the ramifications of academic research for the country as a whole, I enquired about the reasons of the missing links between basic research and the production:

> It is essential to conduct a thorough analysis of the reasons behind the scant reciprocal influence between industry – mainly organised on family basis – and research. In the absence of a tradition and an apparatus designed to absorb and adapt the products of research for "external" use, it is of little benefit – and often extremely detrimental – to ascribe usefulness, a priori, to targeted programmes. At present, technological innovation that responds to the needs of industry is only marginal (from a scientific point of view), and is limited to issues of cost-reduction and market-share and thus, essentially, to an analysis of production processes with no need for the acquisition of fundamental knowledge. However, it remains the basis for the research of the university. If this gap is to be bridged, enormous efforts will be required from all concerned. It is, in fact, the absence of such efforts that has reduced all of the industrial reconversions proposed thus far by the various governments, to little more than rescue operations.

I concluded the interview with hopes for the future, which however were never realised. The system instead was further deteriorated due to the political and economic crisis of the country. I must admit that, many years later, we are still in the midstream.

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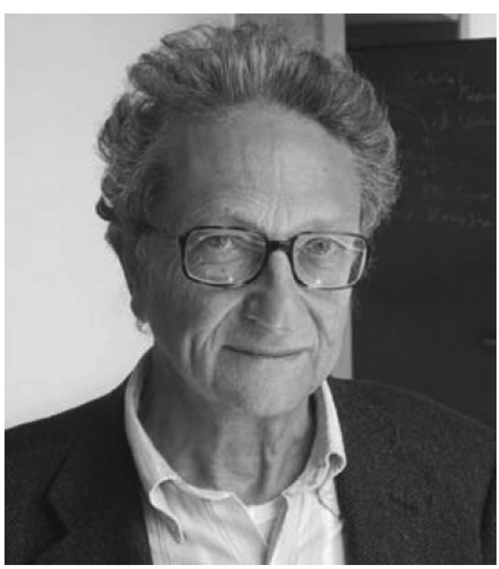

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