Mazur

Proffesor Abragam, it is a real pleasure to have been assigned the task of presenting the 1982 Lorentz medal to you. Of course, *you* know that the field to which you devoted your efforts so successfully is of great importance to a wide range of disciplines. To convince the audience, let me first read the opening sentences of your book on Nuclear Magnetism.

> *Since the first successful detection of nuclear resonance signals late in 1945, nuclear magnetism has developed at a pace which after fifteen years still shows no sign of slackening. Besides its first and obvious application to the measurement of nuclear moments, it has become a major tool in the study of the finer properties of matter in bulk. Structure of molecules, reaction rates and chemical equilibria, chemical bonding, crystal structures, internal motions in solids and liquids, electronic densities in metals, alloys, and semiconductors, internal fields in ferromagnetic and antiferromagnetic substances, density of states in superconductors, properties of quantum liquids, are some of the topics where nuclear magnetism has so far provided specific and detailed information.*

These words, written in 1960, are just as valid another twenty years later: the pace has not slackened yet. Two examples that mark the last decade spring to my mind. The further penetration of NMR as a tool in the unravelling of complex biochemical problems, now even in living organisms, and the remarkable progress made in your laboratory with the creation of nuclear order in solids via ingeneous resonance methods.

Of course, one is taught not to perplex one's audience by a long enumeration like I just gave. That I have done so nevertheless, is to provide a convincing background for what the jury thinks ? has been one of your great contributions. *Through your papers, your inspiring personal contacts, and above all through your books, you have helped to provide the foundations for the application of magnetic resonance to a bewildering variety of problems.*

Born in what is now the Soviet Union, you received your ~~higher~~ education in France. After a start in mathematics you were preparing the ground for a doctorate in theoretical physics under the guidance of M. Francis Perrin when war broke out. At the beginning and at the end of this unfortunate period you served in the army, while during the years in between you had to combine teaching with a further study of mathematics. Although somewhat younger than you, I think I know how you must have felt when this period of disarray came to an end: could one just sit and think, or should one do something ostensibly useful instead? You took up a study in the Ecole Supérieure d'Electricité from which you emerged with the Diplome d'Ingénieur Radioélectricien; I joined industry to do research in petroleum technology.

In 1947 your scientific career started in earnest. You joined the French Atomic Energy Commission, and went to Oxford to work with Pryce. In those days nuclear magnetic resonance and electron resonance had just been discovered, and the Clarendon laboratory at Oxford was one of the leading centres in the world.

In this laboratory Brebis Bleaney and his colleagues were producing a wealth of data by applying their brand new electron resonance techniques to crystals containing transition metal ions, such as chromium, iron, or manganese. You accepted the challenge to improve the theoretical framework needed for the interpretation of their findings. In your thesis you worked out the idea of the "spin hamiltonian", which today, associated with the names of Pryce and yourself, represents the basic tool for anyone interpreting such experiments. Mr. Chairman, I wish I were able to convey the beauty of their method to the uninitiated. The spin hamiltonian is an elegant half-way house, designed according to the strict rules of symmetry, and which stands between the great mass of numbers gathered in the laboratory and the ultimate, unattainable goal of a complete analysis in terms of the interaction between all the particles making up the crystal.

In one respect you went beyond the level of description offered by the spin Hamiltonian: you explained the then puzzling fact that ions like Cu^{++}, in which the electrons held responsible for the paramagnetism were supposed to move in d orbitals, nevertheless showed such large hyperfine splittings. Making use of earlier ideas of Fermi and Segré you pointed out that, although d electrons have a vanishing probability density at the atomic nucleus, a substantial contact interaction may yet occur ~~between the electrons and nuclear~~ sp~~in~~s through ~~what is now~~ configuration interaction ~~called "core polarization". In terms of your original analysis, the ground state of the ion may not be described by a single electronic~~ configurations in which the unpaired electrons are ~~restrictued to the 3d orbitals, but owing to interelectronic~~

~~repulsion it becomes contaminated~~ with ~~other~~ configurations in which an electron is excited from an inner to an outer s orbital.

Very few of us here present are old enough to have met Lorentz, and none of us has had the privilige to see him at work. To us he is one of the great names in the fascinating history of science. But as Casimir, a former president of this Academy, remarked when offering the Lorentz medal to Van Vleck: *the former recipients of this medal are, as it were, the pillars of a bridge between Lorentz and the present*. In your case, this again is clear. In his theory of electrons Lorentz was the first to explain the influence of matter on electric and magnetic phenomena systematically in terms of atomic charges and currents. Van Vleck took this idea as the starting point in his famous monograph on the theory of electric and magnetic susceptibilities, while you pursued it further. ~~to explain the much more detailed results obtainable via resonance methods.~~ The formulation of the spin Hamiltonian by yourself and Pryce F ~~is rooted in~~ ideas developed by Van Vleck ~~just before the war~~, and the dedication to Van Vleck of your later book with Bleaney ~~on electron paramagnetic resonance of transition metal ions~~ testifies to this intellectual relationship. ~~after Penrose's discovery of hyperfine structure in the electron magnetic resonance spectra of certain crystals, many students must have taken up the challenge to analyse the magnetic interactions involved. Mentally, I see most of them bogged down pitifully in a morass of integrals that depend on the coordinates of the spins, while you cross lightly on stilts taken from irreducible tensor calculus.~~

F, with its elegant use of irreducible tensor calculus, must have drawn inspiration from

After two years in Oxford you went for a year to the United States, where you had a golden opportunity to work with van Vleck on the Zeeman effect in the microwave spectrum of atomic oxygen. With great thoroughness you traced all the interactions that have to be considered before being in a position to make a valid comparison with ~~of~~ experiment. with ~~the higher order quantum electrodynamical corrections to the magnetic moment of the electron~~. I myself, when stuck with a problem, have dared to consult Van Vleck on a few occasions. For him the way out usually was simple, and phrased in the language that was so characteristic for his thinking. For you it must have been a great experience to do a solid piece of research together with him. In the same year you worked with Pound on the angular correlation between α and γ emission in the decay of radio-active nuclei.

I now come to the period that started with your return to France, where in 1955 you obtained the means to found the now famous laboratory for the study of nuclear magnetism under the aegis of the Commissariat à l'Energie Atomique. Even though for a long time you held a much wider managerial responsibility at the C.E.A., you still remained the driving force of the laboratory during more than a quarter of a century. You have forged a team of exceedingly competent collaborators; Solomon in the past, and Goldman now are two of the names that immediately spring to mind. With your team you provided a solid foundation for nuclear magnetism and explored, and of course still are exploring, exciting new areas in this field.

Research in most areas of magnetic resonance and nuclear magnetism would be unthinkable today without the backing provided by your books:

The Principles of Nuclear Magnetism, first published in 1961;

L'effet Mössbauer et les Champs Internes, an account of your first series of lectures given as *Professeur de Magnétisme Nucléaire au Collège de France* in 1960/1961;

Electron Paramagnetic Resonance of Transition Ions, written in collaboration with Bleaney from Oxford and published in 1971;

Nuclear Magnetism, Order and Disorder, written together with your colleague Goldman and just published.

I must confess, I have not read all. But, whereas formerly I might have found it embarrassing to admit this in public, I have been greatly comforted by what our colleague Dresden, the preceding president of this Academy wrote about book reading in his farewell essay, *Het Einde* (*The End*) "I know that especially leafing on stands in bad repute and often arises from less important considerations ("how does it end?"), but this does not deny that the method by itself is correct and, actually, ought to be made compulsory".*

Rather than to attempt to cover the main body of your work systematically, let me choose a more subjective approach. One of the first times I heard you lecture was at a Gordon Conference on Magnetic Resonance in the early sixties, where you discussed spin temperature in the rotating frame. My own work was too far

---

*Ik weet dat vooral het verder bladeren niet gunstig staat aangeschreven en ook dikwijls uit minder belangrijke overwegingen gebeurt ("hoe loopt het af?"), maar zulks weerhoudt niet dat de methode op zich zelf juist is en eigenlijk dwingend zou moeten worden voorgeschreven).

removed from the centre of your theme for me to really understand the details, but I sensed I had heard a message with great potential implications. Back at home, I wanted to find out more, and tried to explain the gist of your message to my friends, experts in statistical mechanics. This, then, unleashed a somewhat spirited discussion which showed that the concepts set forth by you were by no means generally accepted yet. I think everybody now agrees that the developments in the next two decades have shown how fruitful your way of thinking is, not in the least in the design of new experiments.

At a day like this I feel at a loss for not being a better historian of science. In particular, I am in want of a clear picture of the development of ideas at the interface between magnetic resonance and [illegible] equilibrium statistical mechanics. After (Ok ?) all, the craftsmanship that evolved from Erwin Hahn's discovery of the spin echo has come frightfully close to creating a Maxwellian demon...

The suggestion to assign a temperature to a system of spins, distinct from that of the lattice, was first made by Casimir and Dupré in their discussion of experiments on paramagnetic relaxation by Dutch physicists in the late thirties. The usefulness of their idea stems from the realization that, whereas the spins (through the dipolar interaction) rapidly come to a state of equilibrium amongst each other, thermal relaxation between this system and the lattice usually is a far slower process.

For an ensemble of weakly coupled I = ½ particles in a strong magnetic field, in which each spin has the choice between two eigenstates, this concept of a spin temperature is almost trivial. Also the generalization to spins greater than ½ presented no problem, provided the Zeeman levels are equally spaced. But what is the meaning of negative spin temperatures, attained via a population reversal? Or, what to think of a system of spins that has come to thermal equilibrium with the lattice in a strong external field and is then removed from the field ~~*adiabatically* (actually, in your presence I ought to be~~ more ~~careful and say~~ *isentropically* ? A quarter of a century ago a number of well-known physicists were grappling with such questions; Purcell and Pound, for instance, and Ramsey, but soon, I think, ~~it is fair to say, however, that gradually~~ you assumed the role of quizmaster: you posed the most crucial questions and sorted out the correct answers.

Of course, nowadays the definite answers to these and a great number of related questions can be found in the books by Goldman and yourself. But someone who wants a touch of the flavour of the original should read the paper by you and Proctor of 1958 in the Physical Review. I found it a delight on a rainy Sunday earlier this month. In this paper you examine the situation that arises when the external field is reduced in strength and vanishes relative to the local fields caused by neighbouring spins and where, therefore, the notion of discrete Zeeman eigenstates available to the individual spins breaks down. Nevertheless, you show how to define a spin temperature and establish its identity with thermodynamic temperature.

In parallel with your earlier work on spin temperature, but not yet firmly linked to it for an outside observer, you think about methods to orient nuclear spins. Because of the smallness of the nuclear magnetic moments, a "brute force" method using a very strong external magnetic field is impractical, and more stealthy mothods that rely on the much greater magnetic moment of the electron are called for. Inspired by Overhauser's NMR experiments on metals, you suggest the idea of *dynamic nuclear polarization* in liquids by means of a paramagnetic ~~which the polarization of the electron spins of an~~ "impurity" added on purpose~~, to a liquid sample is transferred to the nuclei via saturation of one of the electron resonance transitions~~. Subsequently, and almost simultaneous with the Phyical Review article on spin temperature, you and Proctor publish~~ed~~ a paper in the Comptes Rendus on what is now called *dynamic nuclear polarization by the solid effect.* Here the nuclear spin system is cooled by bringing it in contact with electron spins via the saturation of a "forbidden" electron resonance transition in which the spins of an electron and nucleus are flip-flipped or flip-flopped simultaneously ($|\Delta S_z| = 1$, $|\Delta I_z| = 1$).

Clearly the thinking about problems in nuclear magnetism in terms of distinct thermal reservoirs at different temperatures, with heat flows between them that may be switched on or off by the experimenter, proved very rewarding. This pseudo-thermodynamic approach not only led to an understanding of existing problems but, in particular, helped to design fascinating new experiments, of which those mentioned were among the first. ~~during the past two decades.~~

Since time is running out, I do not want to dwell on the further theoretical justification of what I loosely called the pseudo-thermodynamic approach and to which, besides yourself, Redfield, Provotorov, and your younger colleagues Goldman and Solomon have made important contributions. Instead, I want to select a few of the experimental highlights achieved in your laboratory in the past two decades.

In the study of nuclear reactions one likes to work with nuclei in a definite state of polarization, both in the beam from an accelarator and in the target. First ~~of all~~ Winter and you worked on ~~proposed a~~ methods ~~which r.f. devices placed in the beam cause the~~ to produce a beam of polarized protons or deuterons ~~emerging~~ from a cyclotron. ~~to be in a single state of polarization.~~ Second ~~On the other hand~~, the solid effect was ~~being~~ put ~~exploited~~ to use at Saclay ~~and elsewhere~~ to ~~build~~ make polarized targets. ~~for nuclear accelerators all over the world~~. Soon These were no longer the efforts of individual scientists, but involved large teams of physicists and engineers all over the world. Whereas in 1959, Borghini and you were quite pleased to report a proton polarization of 1.5% in a tiny sample ~~of 10 mm^3~~ of polystyrene doped with DPPH, polarizations coming close to 100% are today routanely achieved in targets as large as 20 cm^3.

In the instances just mentioned your ideas played a crucial part in the work of others, the nuclear physicists. Your own interests were primarily focussed on the study of nuclear ordering in solids for its own sake. With a singleness of purpose that reminds me of Captain Ahab's quest of the white whale across the Oceans in Melville's *Moby Dick*, you went to search for nuclear antiferromagnetism. Fortunately, your quest ended happier than Ahab's: in 1969 Chapellier, Goldman, Vu Huang and you observed an antiferromagnetically ordered state of the ^{19}F nuclei in a crystal of CaF_2 doped with U^{3+} as a paramagnetic impurity. The logical sequence of experimental steps that led to this success may seem simple. You started with a sample in thermal equilibrium at 0.7 K in a field of 2.7 T, in which the electron spins ~~have a degree of~~ are highly polarized. ~~in excess of 99%~~. First the electron spin~~s~~ polarization was largely transferred to the ^{19}F nuclear spins via the solid effect, and ~~which reduced their spin temperature to 3 mK.~~ ~~Then~~ Subsequently these nuclei were demagnetized, thereby lowering th~~is~~eir spin temperature ~~further~~, to below 1 μK. Finally, the ordered state of the nuclei was observed via the transverse susceptibility. I shall not go into any details of such experiments because, as you once said yourself: "La simplicité du principe contraste avec la difficulté de la réalisation". It thus took you ten years of a determined struggle with your very competent team to have a first, split-second glimpse of a nuclear ordered state ~~in~~ a during rapid passage through resonance.

Mr. Chairman, I hope you will allow me to stop here, and to skip the further, very beautiful experiments on nuclear ordering that ensued, and which I can assure you, the laureate is continuing with unabated vigour. In some experiments neutron diffraction is used in a most intriguing manner to probe the ordered domains, in others this is done via the NMR signals of rare isotopes present in the sample. Dr. Wenckebach, here present, can tell you how stimulating it ~~is~~ was to participate in these very sophisticated investigations.

I trust that with this superficial sketch of a lifetime's work I have made the Jury's choice for the 1982 Lorentz medal clear. Let me finish with a few more personal words. Professor Abragam, many physicists in the Netherlands, and I myself in particular have enjoyed the lively contacts with you; not just for the sake of science, but also for the remarkable *bon mots* with which your science is spiced. (We try to remember them - too often in vain.) The scientific seeds you have sown during the semester in which you held the Lorentz chair in Leiden appear to germinate well, and you have made some very good friends. The lectures you gave, all without notes, were a source of inspiration for many, and as a result, some of my colleagues in the audience are now trying to achieve nuclear ordering in a molecular crystal. May they be successful!

I am convinced that I speak on behalf of all when I congratulate you, and also Mme Abragam, with the distinction you have won. Let me now hand the Lorentz medal to you in the name of the Royal Netherlands Academy of Science. Accept it as a well-earned distinction and token of our friendship.