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INTRODUCTION A LA TABLE RONDE

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L. MICHEL.- On this day on history of concepts I have to introduce now the abstract concept of "table ronde". What you see is an elongated table with five distinguished speakers. They will discuss in front of you. I hope that every one will ask questions to everyone ; and, as you know, questions from the audience are expected and will be welcome.

It is a very agreable duty for me to present you the speakers of this round table. They are very well known, let me still give their name : Dr Steinberger. He began as a theoretician in Chicago working with Teller and near Fermi. However his thesis was experimental : he was the first one, in 1948, to mesure the electron energy spectrum from μ - decay. As a theoretician he went to the Institute for Advanced Study in Princeton. There he studied several decay modes of the possible new mesons. Finally he preferred to be an experimentalist. Did you notice on the different list of references which were handled to us, for instance the one given this morning by professor Kemmer, Steinberger's name appears in many different and important experiments, eg. isospin and π^o and $\pi^{\pm}$ production. He has always been in the forefront , working recently with energetic neutrinos and μ's. Professor Steinberger is in CERN. I just decided to introduce the speakers in the opposite order to that of their main contribution to the table ronde. We like changes. So Professor Steinberger will be the last speaker. He told me he did not intend to give a talk. I hope he will change his mind. However since he did not prepare a text, I will give him a special privilege : he has the right whenever he likes to interrupt any speaker for asking him questions.

Next, I introduce professor Nishijima. He already spoke this morning and explained how was the situation in Japan when his country was somewhat isolated. His name is attached to the Gell-Mann-Nishijima relation. But you would be wrong if you believe he has done nothing else. He will speak of his work in Japan (among that of other Japaneses). He came to USA around 1958 to stay about ten years. I remember well of his work on renormalization by masses. More generally his subjects have been from phenomenology to very general and fundamental. He wrote two interesting books in English on elementary particles. I enjoyed them. I know that he wrote several books in Japanese, on classical mechanics and advanced relativistic mechanics. Although Japanese is spoken by more people than French, it just happen that most of you and myself we are not able to read this language.

It is the same for Chinese, the most spoken language in the world and Professor Yang mother 's tongue.

Everybody here knows that Professor Yang received the Nobel prize for the discovery of parity violation. This discovery could have been a beautiful topic for this conference But there has already been a book devoted on this subject, where all actors have been able to express themselves : this book belongs to a serie edited by Maglić and the title is, as far as I remember, "Adventure in Experimental Physics". May be Professor Yang will say more about it. Lee and Yang did find parity violation at first. I remember to be fascinated by the parity doublet theory they tried just before. This theory is now forgotten by the physicists but probably historians of science have to know about it. Professor Yang has also his name attached to Yang Mills equations and he is very active in gauge theories which seem to be now our best approach to fundamental equations of physics. Professor Yang has worked on all theoretical aspects of particle physics starting from angular correlations when he arrived in USA in 1947

writing also several paper with Fermi. His influence has been very great as you all know, and not only in particle physics. Lee and Yang made also lasting contributions in statistical mechanics. I assume that the story I read from Dyson is true : before discovering parity violation, Lee and Yang had found a beautiful theorem of zeros on a cercle by a statistical mechanics function and they were more excited by this discovery than for that which was rewarded by the Nobel prize. May be Professor Yang will comment on it.

My next duty is to introduce professor Wigner. When I was a young physicist, following his lectures in Princeton, I would have never dream that one day I would have to present this geant of physics. I feel unable to do it with my own words so I will make some quotation from what was written on him on his sixtieth birthday, twenty years ago. Let me just tell you before that Professor Wigner got a degree of chemical engineering in Zurich and he is a well known engineer. He wrote a fundamental book with A. Weinberg on nuclear reactors. He cared the first diseases of reactors and his treatments are still world wide used. Professor Wigner is also a well known mathematician. In his paper which appears in Annals of Mathematics in 1939 he was the first mathematician to give a complete serie of unitary irreductible representations of a compact non semi-simple group : the Lorentz inhomogeneous group that we call rightly now, thanks to Wigner, the Poincaré group. This paper is very fundamental for quantum physics and has an interesting history. At the end of the introduction of this Wigner thanks Dirac (he often says of "his famous brother-in-law" when he speaks about Dirac) for giving him the idea of writing this paper in 1929.

E. WIGNER. - I do not remember.

L. MICHEL. - I will show it to you.

E. WIGNER. - Good.

L. MICHEL. - We would all be pleased to know more about such a discussion between Dirac and Wigner at that time. It must have then appeared as an impossible task to write these physical Poincaré groups representations. Wigner finished it only in 1937, using several yet unpublished results of J. von Neumann (one, with Szilard, of the famous schoolmates of Wigner in Budapest). This fundamental paper was refused for publication by several physics journal and even by a mathematics journal. Finally, J. Von Neumann took it for publication in Annals of Mathematics. It may comfort all of us, when one of our paper is refused by a journal, to know that Wigner paper became at last one of the most quoted paper of the century ! I believe professor Wigner even received a diploma about this paper. Is it from Citation Index ?

E. WIGNER. - It was the same people who refuse the paper who wrote me about thirty years later that it is one of the twenty five most frequently quoted paper.

L. MICHEL. - Finally you all know that Wigner is also a famous theoretical physicist. We are happy and grateful that he worked more than fifty years as a physicist. This has also enable him to see the experimental discovery of many of his predictions. For intance it seems that last year were made for the first time electron crystals (in a surface of superfluid helium). Wigner had invented electron crystals more than forty years ago ! Let me simply read from Review of Modern Physics, the issue of 1962. "It sometimes appears that there are communication barriers between Wigner and other physicists. But there seems to be no barriers between Wigner and Physics".

"A characteristic feature of Wigner's way of working is its down to earth quality. There are many young men who, their heads bulging with information in Hilbert space, have come to him with an idea only to have him try it out first on two by two matrices. It usually helps. Another aspect of his down to earth quality is a great respect for knowledge of facts. If one comes to study a crystal with him, it is a good bet that he will be able to give off-hand its density, its structure, thermal conductivity, and the slow neutron cross section of its elements. Morever the stuff he is thinking about is probably of the right color".

I want to add something more personnal. It is about his generosity for quoting other physicists'works. Many of us, as young men, benefitted from his generous quotations. I have a theory about that : what professor Wigner is doing is just reading the title and introduction of a paper. Often it is then faster for him to think of what should be in the paper than to read it. And in his quotation, he just credit you for everything he would have written himself after your introduction.

E. WIGNER. - I should contradict the chairman.

V.K. WEISSKOPF. - Many of us benefitted of that.

L. MICHEL. - I want to introduced professor Perrin who will speak in French. You see how is French life. I knew much better Professor Wigner than Professor Perrin, for which I have a high respect. He was in the jury of my thesis, presided by Professor L. de Broglie who will be ninety on next August 15. Professor Perrin made this thesis more lively by asking me many nasty questions ; it helped me very much. As you have already heard during this meeting, Professor F. Perrin was very active from the discovery of neutrino up to the discovery of the uranium fission ; his last work with Joliot Halban and Kowarski is famous. After the war he did not published so much himself because he had heavy duties. He has been a long time the head of the French Atomic Energy Commission and the French nation is grateful to him for the great and good influence he had at this post.

Professor Perrin will speak first on the discovery of artificial radioactivity. I do not know exactly the subject of the talk of the other members of the round table . They have been free to speak as they like with the only constraint of a time table. However I do not want to rush them. So let us agree that this round table will be , like the T.V. show, open end. It will finish when every member of the round table has finished to speak and ask questions to the others.

I now call on Professor Perrin.