Some Recollections of the early Seventies...

T.T.T Truong
Unviversité de Cergy-Pontoise
Laboratoire de Physique Théorique et Modélisation
F-95031 Cergy-Pontoise, FRANCE

Dedicated to John Klauder at the occasion of his seventieth birthday

In 1971, while working towards my Ph. D degree in Physics under John R. Klauder, I never thought that I would be writing these lines in his honor at the occasion of his seventieth birthday. However it seems that I have the good fortune to survive that long in Physics and I would take this opportunity to pay tribute to John for the guidance he gave me during my doctoral work and for the support he provided all along my career. I hope that the few personal recollections presented here will reflect some of the enjoyment I derived during the time we spent together in the early seventies studying some subjects of common interest in Physics.

My first encounter with John goes back to the fall of 1968, after my Ph. D qualifying examination at Columbia University in New York City. Successful students were supposed to find a thesis advisor and start doctoral research. The Columbia Physics Department was at the time very strongly oriented towards particle physics with several leading figures such as Prof. T.D. Lee, Prof. L. Lederman, Prof. J. Steinberger, etc... . As I wanted to do theoretical Physics in some "general" topic, and was a bit scared of not being good enough in particle physics, I turned to the chairman, Prof. A.M. Sachs for advice. He suggested that I could do off-campus research and that I should see Prof. J.M. Luttinger, who had contacts with Bell Labs, a very large research laboratory in Murray Hills (N.J.). Very quickly Luttinger arranged for a meeting so that I could meet some of the members of the Theory Group in Bell Labs who were interested in having a doctoral student.

I departed early from New York City and found myself for the first time in the middle of the beautiful landscape of New Jersey. It was exactly as described in books I read as a high school student. Then I came to this long and huge building on a hilly area with an immense car park outside. Dr. E.O. Kane, the Head of the Theory Group greeted me at the reception and we came down the famous quarter mile corridor of the main building. I was particularly impressed by the volume and the size of the research apparatus in each office and, above all of the tense working atmosphere of scientists in a real industrial research center! Dr. Kane took a few minutes to present his group and then introduced me to several theorists. I spoke successively to M. Lax, J.C. Phillips, N. Werthamer, P. Platzmann, W. Brinkmann, and E. Blount, but could not see some of the members of the group who were absent like P.W. Anderson or B. Halperin. At the end of the day, I was taken to the office of John R Klauder.

Just a few months before, I have come across a new book entitled *"Fundamentals of Quantum Optics"* by J.R. Klauder and E.C.G. Sudarshan, which had been just bought by the Physics library. I was interested in this topic and had written a Master's thesis for my degree in research engineering in Paris on the coherence of some light sources, the year before coming to Columbia University. I was pleasantly surprised to find the author just now in front of me and not in Syracuse University as indicated in the book. I felt somehow that I had found the right thesis advisor! John greeted me very warmly and we began to talk.

He wanted to know my background and my interests. I told him that as a teenager in Vietnam I had already wanted to study physics and become a physicist, but I could only go to France and study engineering. In Paris I continued to study physics on the side, working towards an undergraduate degree which eventually led me to Columbia University. What seemed to be important to John was my background in coherent states; I had told him that I had worked through the series of papers by R.J. Glauber which appeared just at the time of the invention of the laser. This made me sort of well prepared as a candidate. As the conversation went along we spoke of other interests. I then mentioned that I had contact with Prof. G. Pétiau of the *Institut Henri Poincaré* in Paris. Pétiau was known for the Dirac like equation and matrices he invented simultaneously with Kemmer and Duffin for integer spin particles in the forties. But now Pétiau had changed to General Relativity and had proposed that I should take the subject of spinor formulation of electrodynamics in curved space as thesis topic if I decided to work under him. John was very delighted that Relativity and Gravitation were also familiar to me. I would be following in the foot steps of E.A. Aslaksen, who had just graduated under his guidance with a thesis in this field at Lehigh University, and an agreement was to be signed between Bell Labs and Columbia University allowing me to work under John's guidance.

At the beginning it was understood that I should work through John's papers on the "Continuous Representation Theory" and that we should meet periodically to discuss progress on my understanding of this theory. But as I realized afterwards, I had not received a problem to work on. John insisted, when he gave me his reprints, that I should be able to choose the precise question to work on, but of course after a while! At first, this way of proceeding frightened me; I wasn't sure if I would be able to find a worthy problem in the Theory of

Continuous Representation to solve! But I trusted his approach and went on. It took me a very, very, very long time to grasp all this theory. One day as we discussed the nature of coherent states, I suddenly decided to ask a question which had been floating around in my head for quite some time. Up until I came to the U.S., the concept of coherent state was always linked to the name of R.J. Glauber, who introduced it in the context of the physics of laser fields, whereas I had now just discovered the real mathematical foundations of these remarkable states. I ventured to ask John: " Are you the real inventor of the coherent states?". He looked at me with a smile and said: " You should know that in physics *nothing* is really *new*. The coherent states were introduced in quantum mechanics long ago by E. Schrödinger [1], but of course he did not give the framework of the Continuous Representation Theory." This struck me as a deep and profound piece of truth. I suddenly recalled someone who had told me long ago that Einstein did not invent any of the equations occurring in the theory of Special Relativity but rather put them in a logical seminal structure.

Discussions with John were always informative and educative. Not only he answered my questions on his specialized field, but he gave me interesting views on radar physics, high energy scattering of particles, renormalizability of field theory, as well as impurity effects in solid state.... I had a great time learning from his far reaching experience in Bell Laboratories. In 1971, he edited a book [2] in honor of J.A. Wheeler, his former thesis advisor at Princeton University. Contributors are naturally people close to Wheeler and former students, among them Prof. R.P. Feynman. John told me that Feynman was reluctant to write two papers on his work on the quantization of the gravitational field which had, to this point, not been published. But now John had found excellent arguments to convince him. I was curious to know what kind of arguments he had in mind. "Well, he said to me, I am going to tell Feynman that this is the unique opportunity to published this long awaited work without having to cite anybody in the field!" Needless to say that I was struck by such a brilliant idea! I was in his office when he called Feynman and Feynman agreed! Later on, John asked me to look for misprints in the galley proofs of the Feynman papers, and I felt very honored to be the first graduate student to read them.

In the Summer 1971, John was invited to lecture at the last Boulder Summer School at the University of Colorado at Boulder. He took me along and I got my first opportunity to be a participant of a Summer School. Besides this was also my deepest penetration into the western part of the U.S. The beautiful landscape of the surrounding national parks, filled with vestiges of the golden rush, had brought a poetic touch to the on-going research of John. I met him one-day in the university library. "You know, he said with a triumphant smile, you must always find the right name for the objects you are creating. I just found the right name for the model field theory I am going to talk about in this school." The fact of being in the middle of a geological site had led him to give his new model a geological character: " Covariant *diastrophic* quantum field theory", which fitted remarkably with its content. And I never could forget the nostalgic ending of his lecture notes inciting the reader to plough into the hard layers of rocks as *"the miner of yesteryears"*, who thought *"there must be gold* *in thar hills"* [3].

In the meantime, I had not made any progress in my thesis work. I desperately looked for an idea, since I was supposed to find it by myself. One afternoon in my office, after staring days and nights at the same formulae of the Continuous Representation Theory, it suddenly occur to me that one should see what can this theory could say about the simplest *"interacting"* theory which was very popular at the time: the quartic anharmonic oscillator! I discussed the idea with John and he encouraged me to try. The idea was to treat this single degree of freedom problem in the frame work of the regular representation of the canonical commutation relations, contained in John's Continuous Representation Theory and see what happened. It turned out that there exists a commuting operator with the Hamiltonian, which helps in finding the form of the eigenstates in the Schrödinger representation. In fact, one can show that the Schrödinger wave functions obey a linear homogeneous integral equation. John was amazed by this result and suggested that a test should be made, since there exists a natural way for obtaining very good approximate wave functions for the ground state from the structure of the integral equation. He made arrangements for a numerical computation to be performed by a member of his staff and within a few days the value of the ground state energy was obtained and it agreed with astonishing precision (five significant figures) with other calculations using large order perturbation theory, [20,20] Padé approximants or semi-classical methods [4]. We were so delighted and this was a very exciting moment shared with John and (if my memory is correct) Prof. H. Ezawa, who was at the time visiting Bell Labs.

In December 1972, I defended my thesis and started to look for a job. But with the difficult economical situation, I could only teach part-time in a college north of Manhattan. John arranged for me to be interviewed in many places, his support for my search has never failed. At the end, while lecturing at the Karpacz Winter school in Poland, he learned that openings at a junior level were available at the newly founded *Institut für Theoretische Physik* at the Free University of Berlin, Germany. I went to Berlin in the fall 1974, and during the following years we still saw each other on numerous occasions, for example at the Summer school in Antwerp (1977), at the International Congress of Mathematical Physics in Lausanne (1979) and Boulder (1983), etc... And he never stopped writing recommendation letters for me until I finally got a tenure job at the university of Tours (France) in 1990.

I must say I have the good fortune to have met John and to have studied under him. His work, albeit abstract for most physicists, has penetrated many fields of physics [4], as I can myself witness in my present day work in image processing using wavelets, which are after all contained in his 1968 papers with E. A. Aslaksen [5]. On this, the occasion of his seventieth birthday, I wish him many more years of fruitful research and may he receive the gratitude and the friendship of a former student from the far eastern part of the world.

References

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