Acceptance speech by Professor E.C.G. Sudarshan on Sunday October 26, 1986

at the Award Ceremony at the International Centre for Theoretical Physics,

Trieste, Italy

Citation read by Prof. Hassan, Secretary of the Third World Academy of Sciences.

The physics prize is awarded to Professor E.C.G. Sudarshan from India and the citation reads: "For his fundamental contributions to the understanding of the weak nuclear force, in particular for his part in the formulation of the universal V-A theory of Sudarshan and Marshak."

Professor Sudarshan was born in India on 16 September, 1931. He obtained his Ph.D. from the University of Rochester in 1958, stayed there as an Assistant Professor from 1959-1961 and Associate Professor 1961-1963. He was Professor and director of the Elementary Particle Physics research program of the University of Syracuse from 1964 to 1969. He has been a professor at the University of Texas since 1969 where since 1970 he has been Director of the Center for Particle Theory. He is Director of the Institute for Mathematical Sciences, in Taramani, Madras, India. Professor Sudarshan has made important contributions in addition to his work in collaboration with Professor R. Marshak on the V-A theory, in quantum optics, classical dynamics and quantum field theory. He has been the recipient of several awards and has served on editorial boards of journals of physics and mathematics.

Professor Salam, Your Excellency Ferdinand Salleo, my dear wife, ladies and gentlemen! This occasion gives me great pleasure; and I would like to express my gratitude at this great honor that has been conferred on me. I have not prepared slides, transparencies, illustrations or anything of that kind partly because I just found out whether I was to talk or not. But I am glad to have an opportunity to talk to you. Since the work for which I am being honored was done more than half my life ago, I would talk to you a little bit about the ambiance of that particular time and how I wish those times would come back again.

It was in the spring of 1952 that I joined the Tata Institute of Fundamental Research as a research student. The shining light in theoretical research at that time was Homi Bhabha who had by then completed his work on cosmic ray cascades and relativistic a wave equation. Bhabha was deeply involved in organizing a nuclear research team in Bombay. But he was still very much the academic leader and presided over the weekly colloquia. He also brought to Bombay many distinguished physicists like Dirac, Goeppert-Mayer, Lévy, Marshak, Mayer, Pais, Pauli, Salam and Tomonaga; and he got Bernard Peters to join the TIFR faculty. I got to work with Professor Peters; and quickly graduated from developing emulsions and scanning nuclear emulsions to developing theoretical aspects of experimental particle physics. During this time I had studied a number of physical theories because there was really no one to tell us what to study; in some sense this was the best possible education. But it was a highly disorganized study; one knew very many details of the advanced topics without necessarily knowing much about the foundations. And one of the areas about which I did not know very much about was nuclear physics because I had spent most of my effort to understand particle physics and quantum field theory, and whatever time was left I listened to lectures on pure mathematics from people like Chandrasekharan, Harish-Chandra, Kosambi, and Ramanathan, but nuclear physics had very few people who were experts in theory; though Maria Goeppert-Mayer lectured to us. Of course you never get away without knowing such a field; and it became my fate to have to study nuclear physics in some detail.

I joined the University of Rochester in the Fall of 1955 with my bride Lalita, who decided that even though it is said "*vivāho vidyānā*" (marriage is the end of all studies!) she was in fact a source not only of romantic excitement but also of spiritual support for my research studies. During the first year that I was at the University of Rochester as a graduate student Prof. Abdus Salam came on a lecture visit. He was a very humane person. I had seen him many months before at the Tata Institute but at that time I dared not approach him; but this time, since we were both "southern people" in a "northern climate", it was much easier for me to approach him. It turned out he was really not as fierce as he appeared to be! Though I had published several scientific papers from Bombay, the first piece of research that I did as a graduate student at Rochester was the continuation of something that I had learned in Bombay, the second piece of research was directly the result of a discussion that Professor Marshak and I had with Professor Salam. This gave me a chance to do a new calculation which nobody else had done up to that particular time. The idea in Bombay among theorists was that you became a scholar and learned what other people had done but you never did anything on your own if you could help it! But this time it was to study the electromagnetic properties of the new particles (which were not completely identified at the time I was in Bombay). Salam suggested to Marshak that I should make this calculation; so I calculated these using quantum field theory with a good deal of excitement. In later years of course I have told people how easily I did those calculations (and they were easy) but little do they know with how much trepidation I did the work. This also led S. Okubo, Marshak and I to the first use of broken symmetry in particle physics for the calculation of electromagnetic properties of baryons.

Soon after I joined the University of Rochester I was to take an exhaustive examination which was euphemistically called a qualifying examination. Its main purpose was to disqualify people who did not measure up to the level. Four of us Robert Knox, Fred Seward, Giogio Giacomelli and I wrote this examination together. The person who was supervising us was such a fierce-looking man and his mustache made my wife really afraid; she had brought me coffee to drink during the test, so that I may survive the examination. Finally, she did persuade the examiner that drinking coffee was not violating the rules of the examination.

We were expected to have a thorough knowledge of nuclear physics and in particular nuclear beta decay. So something that I had always thought I had escaped came to haunt me. It was a good thing for me because I got acquainted with the crucial experimental details and the theoretical thinking in that area. Marshak had earlier done pioneering work on what were called unique forbidden beta spectra which were systematic tests of the details of relativistic beta decay theory, or whatever was existing at that time. In the course of preparing for this test I did study around the subject and was able to find out a number of things.

It is usually said in my tradition that three things in life are very difficult to come by, *manuṣyatvam, kmumukṣatvam, mahāpuruṣasañgamam* : to be born human, not really, with two legs and so on, but really to be concerned with human endeavor; second, to be curious and intelligent; and to be touched and influenced by great souls. I was particularly fortunate: all the studies that I had done in Bombay had come to fruition because I met Professor Marshak who agreed to be my guide. Marshak was able to teach many things which he himself did not know: when I told him that I did not know very much about so called discrete symmetries in quantum field theories, he said that we should have a seminar and that I should give the first 4 lectures. It is bad enough to discuss such abstruse topics with your teacher but it is much worse when the teacher is presiding over a seminar where other people can ask questions.

In the course of the study of beta decay it became clear to me that while there was considerable discussion as to the nature of beta decay and how the details of relativistic beta interactions were to be constructed and people were very sure that it consisted of scalar and tensor interactions, the evidence for this was flimsy. Most of the experiments already done were consistent with various possible beta decay interactions. Among these were experiments on the so called electron neutrino correlation: in the beta decay process an electron along with an anti-neutrino, goes out. The anti-neutrino is a very elusive particle which we don't see but the nucleus recoils and from this the neutrino motion could be inferred, so by studying the details of the correlation between these two we could find out whether the electron and neutrino like to go together in the same direction or they avoid each other and when the electron is going north the neutrino will go south and vice versa. This is a very difficult measurement: you have to deal with the recoil of the nucleus; and it was only after the war that people could do the experiments well. The results on electron-neutrino correlation experiments on various nuclei like Ne^{19}, He^{6} and the neutron had come in; people had simply relied on whatever was available and a flimsy edifice of beta decay theory was built on this evidence.

This knowledge as to how slim was the basis of the scalar-tensor assignment came in handy when a few months later it was suggested to me by Professor Marshak that since I like to do as little work as possible but yet prefer to deal with fundamental physical theories that I might study the beta decay theory in particular and weak interactions in general. There was always the question whether all the radioactive processes not only in atomic nucleus but in all the other elementary particles could be considered as the consequence of one universal theory. It was the kind of integration that Newton had achieved regarding gravitation at the beginning of modern physics and Maxwell had achieved regarding electromagnetism during the past century. The idea is to see if there was one common law which was applicable to all processes of a large class. The electron and the neutrino as well as the nuclear particle are spinning particles and when they combine together one has to find out whether the spins are aligned parallel or are they misaligned. If they are both aligned then of course you get a vector out of the two because the two spin halves add up to produce spin one, spin one means that it corresponds to a vector: the scalar combination of such a vector with another vector made up of the spins of the other two particles in the "interaction". Or is it a case of the two spins misaligning (anti-aligning) so that they add up to zero and thus produce a scalar; the combination with the corresponding scalar made up of the other two particles produce a scalar interaction. When you consider the relativistic theory of course these things become a little more complicated: you have to decide whether the particle part and the anti-particle part are related in the same fashion. In the case of the electromagnetics the basic structure of the interaction involves the vector, namely the flow of the electric charge that produces electric current; the electric current was coupled with the vector potential which is characteristic of the electromagnetic field. It was this interaction which was the general electromagnetic interaction. The question was whether there is a similar law which is applicable for weak interactions: whether the spins are aligned parallel or antiparallel and if they are aligned parallel which of the relativistic interactions would it be. The aligned (spin one) interaction could be axial vector or tensor and the anti-aligned (spin zero) interactions could be scalar or vector. This was not the whole story; in 1956-57 it was determined that parity, the left-right symmetry was not satisfied by weak interactions; therefore the number of interactions became twice as many, at least, as before. I do remember very well Professor Salam visiting during that time in Rochester, and discussing the theory of the left-handed neutrino

The question that this raised for my work was: Was this a contagious disease? were all the other particles also left-handed, or could some of them be right-handed just to be different? The study and analysis of the existing experimetal work at that time soon convinced me that not all the experiments could be correct! It was somewhat like having a very small blanket and trying to cover yourself during the night; if you tried to cover one part of your body the other part gets exposed. There was simply no theory which was consistent with all the existing experimental data. This was a rather important discovery because from that time onward I could choose to disregard whichever experiment was inconvenient. Marshak kept reminding me that it is not safe to disagree with experimentalists because theories can change but experiments don't change. My own feeling was that some experiments have to change so that there could be a theory!

Towards the beginning of 1957 we discovered that provided one set of four experiments (which were supposed to be very good experiments) were wrong, then there was a very simple universal interaction that would account quantitatively for all the processes; and that it was very, very similar to electromagnetism. It was not only a vectorial interaction but also a flow of the left-handed particles only. So all interacting particles are left-handed and the only combination you could make out of them was a vector. Technically it becomes a "vector minus an axial vector" (V-A) with maximum parity violation. One of the consequences would be that the pion would decay not only into its dominant mode of the decay into muon and (its) neutrino but also into a mode which contains the electron and (its) neutrino: this should occur approximately once every ten thousand times the pions decay. One could calculate more precisely: but this was very crucial. Just about a few months before an experiment was done at Chicago specifically to test this possibility: and they found that not even one in ten million was decaying in this fashion. We had to require that this experiment must be wrong.

This was the time that one of the so called Rochester Conferences took place. These were annual conferences in which the leading high energy physicists gathered together. In those days the number of high energy physicists was sufficiently small that all the leading ones could gather in one city. I was told that I would be given 5 minutes to present "The universal theory of weak interactions". Two days before the conference Marshak had second thoughts because he said "You are not even a Ph.D., so you have no business to address the meeting". So I said "That is alright, you can talk about it", but he said "No, I am giving another major talk and I am a host. I should not take up all the lectures". So we asked somebody else to do it for us, who "forgot" to do it. So while all the people were there discussing and debating what could possibly be done, here I was sitting with the complete answer! I was considerably smaller in those days and I did not have a beard or even a mustache. I was approximately 100 pounds and was a very law abiding alien who dare not speak up. If I knew what I know now I probably would have shouted and made a spectacle of myself but got the message across. So it turned out that no one recognized that our "universal theory of weak interactions" was sufficiently important that it should be presented at that particular time. A few months later, Marshak and I discussed the matter in great detail with various other people. It was not a wise thing to talk about such a profound scientific discovery before it was published!

By the 16th of September 1957, when I was precisely 26 years old, a paper was finally distributed which was written about four months before that time. I am therefore particularly gratified that it is recognized to be very important 28 years later!

The experiments which disagreed with our V-A interaction and experiments (there were 4 of them) were redone during the next few months. In the summer of 1958 Professor Salam came to the University of Rochester after attending a conference at CERN, Geneva, (The Rochester Conference had started moving instead of staying in Rochester) and told me that all the experiments that we had wanted to be remeasured have been done by the experimentalists with new results in complete agreement with our theory. Unfortunately, at that time I was in America in what is euphemistically called "docket control": I had no visa, I was staying in the United States with the permission of the United States government but if I so much as stepped outside the bridge on the Canadian border I would be out and could not come back in. So obviously, I could not go to Geneva. But it was so very nice to hear from Salam that the experimental results had changed.

The interesting thing about this particular discovery is the fact that in a sense all the theoretical equipment for making this discovery was at my disposal when I was a student in Bombay, except that there was simply nobody there that thought that one of us could solve such problems. In fact amongst my classmates at Bombay there was one who took himself a little too seriously: and people used to say "He thinks he is going to solve the Universal Fermi Interaction". Little did I know at that particular time that I would myself be solving it about a year and a half later. The second thing is that the University of Rochester at that particular time was in a sense a pool of Third World talent. Marshak went around and collected the best students from all kinds of places and put them together. There were eleven students who were doing theoretical physics at that time out of which eight were from third world countries. The Japanese and Indians were the majority (those days Japan was part of the Third World) but there were also other assorted countries amongst them. (We all talked to each other in peculiar forms of English. There was one poor man there, an American, who was completely bewildered by all these arguments going on between these Third World people about what was the right pronunciation of English and what was the precise meaning of a particular word. So much so that he actually bought an expensive dictionary and kept it there saying "You guys might study it"; we looked at it and then said "But this is an American dictionary it is not an English dictionary.")

My involvement with weak interactions continued. The next two years I spent as an apprentice to Julian Schwinger at Harvard. Harvard was a very interesting place at that time because they had simply not recognized that the weak interaction problem was there and it had been solved! During this time I used to commute by the Greyhound bus from Boston to Rochester so that we could carry out some research work on particle physics. In his travels, Marshak met a young theorist by name Steven Weinberg and brought him to Rochester to discuss physics with me and Okubo. The first paper that Steven Weinberg wrote on weak interactions was written in this particular collaboration. Weinberg went on to do more work on weak interactions later.

At that particular time the University of Rochester did have a very large number of third world students and the majority of us did much of our study without, in a certain sense, paying any attention to what was happening outside. It was a very important thing which does not obtain in most of the universities at the present time: Rochester itself and at my own University of Texas. It becomes even more difficult in third world countries; and my own institute in Madras is no exception. I have been trying hard to convince people that they should have sufficient confidence in attempting the hardest possible problems right then and there. Second, there is nothing as good as having the very best from many different regions come together and talk because they don't really have to worry about the other person being better than you; we were all there for a temporary and short period of time and would go on from there to better and bigger things. I was also fortunate in having as my classmates Susumu Okubo and Tullio Regge. Third: Professor Marshak was at that time the Department Chairman but he made it very clear that the most important thing was scientific research; if you gave him a manuscript or a writeup you did get it back within 24 to 48 hours, irrespective of his other commitments. I have, over the years, tried to pattern myself somewhat after Professor Marshak who seems to be invariant with regards to time translation. He has as much enthusiasm now as he had when he was 35 years younger and he seems to have the same excitement about what he did in his latest paper. The world needs great teachers like Marshak.

I feel very honored to have been given the Award. I had almost given up any recognition for the work that I had done in discovering the universal V-A theory of weak interactions. I am very gratified that the Third World Academy has recognized this work to be important enough to award the very first Prize in Physics to me. I thank you very much and I hope this is the beginning of even better things to come.

Remarks by Prof. Abdus Salam, President of the Third World Academy of Sciences! Prof. Sudarshan has retold the story, I am glad he told you the story. He is much more relaxed today of all days that I have seen him (laugh). He, as he told you, is the inventor of V-A theory that was the theory on which we built up our theory of electroweak unification and the recent work which got the recognition for Rubia and Vandermeyer. All this was based on the work which has been done by Sudarshan and Marshak. Instead of their being recognized there were others who were for a long time credited with the same theory: with the same theory, which was done after this work. We know it. All the people in the subject knew it. But somehow there was a conspiracy, I should say a conspiracy of silence on the subject which harmed him and recognition of his work. I am very glad that we have been able to honor him. We would have liked to honor Marshak also at the same time; he is a great friend of the Third World, but Marshak is not from the Third World. We have been able to give Prof. Sudarshan some recognition after a very, very long time of what he has achieved.