SOME REMINISCENCES*

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One of the privileges that come with age is that you are allowed to reminisce in public. In recent years, starting when I was sixty, I had many occasions to think back about my career and I think there are a number of things which might interest you. That is why I chose this topic this afternoon.

I was a good student in high school. It was obvious to myself and my teachers and parents that I was talented in mathematics. I was also very fortunate to have had as a father a professor of mathematics. That was of great importance to my development for at least two reasons. One was the fact that when I was very little my father sometimes would teach me bits and pieces of mathematics. For example, when I was in elementary school my father one day asked me what the result would be if I added 1 to 2, to 3, up to 20. I didn't figure out how to do it other than by just brute force. He told me how to do it, and then he taught me how to do the sum of a geometrical series. I absorbed this very fast but I later learned that was not the most important thing. The most important thing was to

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Professor Yang is Albert Einstein Professor of Physics and Director of the Institute of Theoretical Physics at the State University of New York at Stony Brook since 1966. He was awarded a Nobel Prize in Physics in 1957 (jointly with T.D. Lee) for work on the non-conservation of parity in weak interactions. He and R.L. Mills founded modern gauge theory (Yang-Mills fields) in 1954.

appreciate that this problem was an interesting thing. And indeed I appreciated it. Once taught I always remembered it because it was a trick which looked beautiful to me. I say it because I have my own children many years later (I have three children, in fact). When they were little, I also taught them how to add 1 to 2, to 3, up to 20. All three children learned very fast but there is a difference between them and me. A year later, if I asked them about it, they didn't have the faintest idea that we had discussed it before.

This was an experience from which I drew a lesson and I think the lesson is a correct one. It is that many children are very talented in many things but somehow to some of them some aspects of knowledge have a certain specific meaning and those who like a specific type of knowledge are in some sense privileged. If they then have the opportunity to go into that field they are likely to go a long way. Merely being able to learn something fast is not enough. It is the value judgement that is important. We learn so many things all the time that we must choose and the choice is dictated by something that is partly perhaps genetic and partly by experience. But once you have acquired the right value judgement which happens to suit your ability, you will be launched into a very good possible career. In my case, I was very fortunate. It happened that my father was a professor of mathematics. It happened that I absorbed mathematics value judgements very rapidly. The accumulation of such knowledge was of tremendous benefit. There was another reason why I was very fortunate. He had many books and I would browse through the books and try to puzzle out some of the statements. I, of course, couldn't understand them in the first place - they were mostly in English or German. In the thirties, German mathematics was extremely strong. There were many text books in German. This is no longer the case - English is the dominant language in physics and mathematics today.

I remember that when I was in high school I took from the the bookshelves of my father a book on number theory. There was clearly stated a theorem which I could read and which said that every integer was a sum of four squares. There followed a proof which I could not follow - it was too complicated. The statement was understandable to me and it was a very interesting statement. I tried to prove it. I did not succeed. Nevertheless such contact conditioned my later taste and that was of great importance. I also remember looking at a beautiful book *(Die Theorie der Gruppen von Endlicher Ordnung)* in German by Speiser *(1885 - 1970)* and it was, of course, more difficult for me to figure out. It was about finite groups. In particular, it had a lot of illustrations. If you had gone to my lecture the day before yesterday, you would have noticed that I talked about the 17 space groups in two dimensions. And all those 17 space groups in decorative form were all shown in Speiser's book. I looked at all those beautiful patterns. I did not understand, so I asked my father, and he said it had something to do with group theory. That was the first time I heard about group theory. He could not explain to me what that number 17 meant. He could not explain to me what the space groups were all about. But the beauty of those patterns left a profound impression on me. When I was writing my bachelor's thesis at the age of 20, I finally understood what those patterns meant.

During the wartime, my family moved to Kunming in Southwest China and I went to Xinan Lianda (the Southwest Associated

University). That was also a very fortunate circumstance for me. The university during the wartime was a combination of three of the most distinguished universities in China - Qinghua, Beida and Nankai, all from Peiping and Tianjin. The professors in the university were among the most important scholars in China then. Reflecting on my days in Xinan Lianda, I am deeply grateful. The university had great difficulties, The lecture halls which were by any standards extremely simple and uncomfortable. The roofs often leaked and the windows were always leaky. The floor was of pounded earth and through years of use there developed many holes in the floor. The situation was difficult but the spirit was very great. Everybody took the courses very seriously - from the professors to the administration to the students. And I often thought about my experience and had always concluded that the foundation of all my later work was laid in those years in Kunming. I stayed in the university longer than most students because from '38 to '42 I was an undergraduate student, and from '42 to '44, I was a graduate student earning a master's degree. There was no doctor's degree offered in China at that time. From '44 to '45 I taught for one year in a high school which was attached to the normal college of the university. So altogether from '38 to '45, I was associated with that university for seven years. Even during the year that I was teaching high school, I attended the seminars at the university and maintained my research contact with the university.

Those seven years were of determining influence in shaping my taste about physics and also in my acquisition of knowledge in physics. The courses we took in Xinan Lianda were generally very well prepared. The lectures were usually rather polished and we were made to do exercises. I rememeber that for our sophomore course in mechanics we used as a text a book by Jeans *(1877 - 1946)*, a distinguished astrophysicist who lived early this century. If you are acquainted with the book, you will find that each chapter has a lot of exercises. They are usually rather ingenious, of the type of things like a ladder leaning against a wall. You study the question of the balance of the ladder against the wall and against the floor or, if it starts to slip, of the speed with which it falls down. Usually Jeans managed to make a complicated twist so that the exercises were not simple. Many of them were Tripos problems in the Cambridge examinations. Now toward this type of exercises, there are two dramatically different attitudes. One is that these are very good training grounds. If a person enjoys doing them he would learn a lot of useful things. The other diametrically opposite view is that they are totally useless and emphasise certain tricky aspects of mechanics which are really of no importance in physics research. I think both views have some merit, but whatever it was, I enjoyed doing those exercises and I benefitted from them because some of the tricks, though different in character, are similar in spirit with some of the research work that I later did.

At the university (Xinan Lianda), we had great difficulties. For example, we had to go and hide in various ditches because there were constant air raids. I remember that one day in 1940 there was a big air raid. I had taken two of my younger brothers with me to the countryside. We saw the Japanese bombers coming in and dropping bombs, and then we saw a fire with smoke. When the alarm was lifted we began to go back, and the closer we got to the city the more we became worried that it might have been the house we were renting that had scored a hit and was burning. We finally arrived at the scene. It turned out that the unfortunate thing was precisely that the house we were staying in had scored a direct hit. Our central courtyard had houses surrounding it, and a bomb which we later estimated to be something like 300 tons of TNT had hit the centre of it, and therefore the four buildings had collapsed backwards. Fortunately the whole family was out of the house. I took two of my brothers to the countryside, my father was at the university, my mother and two younger brother and sister were hiding in a shelter close to the house and they were completely safe. So that evening we had to move in with some friends. We moved to the countryside altogether. I still remember distinctly that after a few days I went back with my father and we began to dig into the pile of rubble. We recovered many things. If you had no experience doing something like this, you would find that digging into a wreck due to a bomb oftentimes produced very strange things. For example, rigid structures like desks would be completely smashed. It would be twisted, broken and splintered so that it became totally useless, but I discovered a basket of eggs completely intact although it had been moved. I did not understand how that happened. And I have today a very vivid memory of trying to locate where the bookshelf had fallen and then digging in that region. And sure enough, I was right. I did locate where the bookshelf had gone, and out of the digging I was able to recover a few books. It is difficult today to understand how great a satisfaction I had then. Those books had a value to me in those days that is not possible to understand today. We valued the few possessions we had and we made maximum use of them.

I got an extremely good training in physics. It was not only because I had learnt the technology of physics. I am particularly grateful that I had acquired a taste of physics. I acquired a distinctive taste of my own. In those days there were papers in physics that I learned to like and papers that I learned not to like. This does not mean that the papers I did not like were bad papers. They could be very important papers but it was just the subject or the methods which I did not like. And it is very important in the maturing process of a young scholar to acquire taste. This is not only true of a writer, an artist or a musician, but it is also true of a scientist. Science itself is supposed to be objective but the research worker doing science is not as objective. It is an interaction between the objective subject and your own subjective judgement, and in this interaction it is important that you acquire a specific viewpoint. A student who never acquires this viewpoint cannot become a truly good research worker because he must develop his own value judgement. The value judgement is not always right but he must have a value judgement. If he finds some of his value judgements wrong, that very process will teach him something so that he will adjust his value judgements later. During those years of contact with physics in Xinan Lianda I acquired a taste in physics which still remains with me today. The best way to describe what that taste was is to tell you who were the twentieth century physicists I admired most in those days, and they were Einstein *(1879 -1955)*, Fermi *(1901 -1954)* and Dirac *(1902 - 1984)*. They are still the twentieth century physicists whom I admire most.

Einstein is indeed the greatest twentieth century physicist. In fact, I have said recently in writing that I believe Einstein ranks with Newton *(1642 - 1727)* as the two greatest physicists of all time. Why was Einstein so great? It was because he was able to see things which other people saw but did not understand. He was able to look at things in an extremely profound way. Now mind you, the extremely profound way is often the simplest way. But it acquires a daring perception and insight to look at it in the simple way and say, "That is it." In the year 1905 when Einstein was 26, he wrote three brilliant papers. Of these the most important is the one that gave the world special relativity. Special relativity was not a term invented by Einstein. It was invented by Poincaré *(1854 - 1912)*, a mathematician many years older than Einstein. Special relativity was about the meaning of Maxwell*'s (1831 - 1879)* equations. You probably know that through some 60 years of experimentation, from 1780 to 1860, the four great experimental laws of electricity and magnetism were discovered. They were Coulomb's *(1736 - 1806)* Law, Gauss's *(1777 - 1855)* Law, Ampere's *(1775 - 1836)* Law and Faraday's *(1791 - 1867)* Law. These were all laws which were empirically found and they formed the pillars of electricity and magnetism. Then in the hands of Clerk Maxwell in 1865 these four laws were formulated into partial differential equations - the so-called Maxwell equations. That was undoubtedly the greatest achievement of nineteenth century physics, but understanding them took many more years. By the early twentieth century there was a great need to understand Maxwell's equations.

What happened was that in the first five years of this century, it was discovered by Lorentz *(1853 - 1928)*, a towering figure in physics in those days, and by Poincaré that Maxwell's equations had properties of being invariant under some transformations. Today we call these properties Lorentz invariance. But they did not understand the physics of these transformations. The transformations were observed to leave Maxwell's equations mathematically invariant, but it was thought by such powerful mathematicians like Poincaré and by such learned physicists like Lorentz that those transformations which transform space-time variables x, y, z, t into other variables x', y', z', t' was only a mathematical artifice. It was the young Einstein who said it was no artifice at all: x', y', z', t' in those transformations are physically as meaningful as the x, y, z, t. This was a revolutionary thought because it says that time is not absolute. In all previous thinking people thought that my time which I measure with my clock is the same as your time which you measure with your clock. The concept of simultaneity (that two events occur at the same time) was regarded as universal and absolute. If I observed two events to be simultaneous, it was taken for granted that any other observer would reach the same conclusion. Why? Because experience told us that this was true. Einstein said, "No, this is only approximately so. Because we do not move relative to each other with very high velocities, we do not detect that the concept of simultaneity is different for different observers. If you have two observers moving relatively to each other at large velocities, then this difference of concept of simultaneity could be measurable." It is a revolutionary thought which is derived from a concept of simplicity. Einstein had the insight and courage to propose this, thereby launching the theory of special relativity.

A few months later, he wrote another three-page paper.

That paper for the first time gave us the equation $E = mc^2$. In it, he essentially said, "Let us take my previous paper and drive it to its logical conclusion. Then we will have to find that energy is equivalent to mass." It was also characteristic of Einstein that he not only knew how to draw logical conclusions, he not only knew how to manipulate mathematical formulae, but he also knew what those mathematical formulae mean. In this particular case, at the end of the short paper where there was the equation $E = mc^2$, he said this equation could be tested. How? There were already known radioactive substances and when uranium disintegrated, a large amount of energy was released. It was not clear where the energy came from. Einstein said that the energy came from the fact that the original uranium atom before decay and the final product had a small difference of mass and that mass is released in the decay process. That was a conclusion to be drawn from his formula $E = mc^2$. So he proposed this test and indeed, of course, when the test was later made, he was absolutely right. And of course, you also know that this is the reason why the reactor which generates energy and the atomic bomb which releases energy are all possible.

Einstein's greatness lies in his great perception and insight into the meaning of physics and into the structure of the mathematical theory of physics, in his courage and daring in looking at old problems in a new and simple way and also in his ability to manipulate the mathematics and draw the right physical conclusions from those manipulations.

I had the privilege later in 1949 of meeting Einstein in person. In 1949, one year after I got my Ph. D. in Chicago, I went to the Institute for Advanced Study in Princeton. Einstein was 70; he had just retired but he still came to his office everyday. So I had the privilege of knowing him - not well, but I did know him. There were about 30 postdoctorals in theoretical physics at the Institute - that was the year with the largest number of postdoctorals in physics and I was one of them. All of us greatly admired Einstein as a great leader in physics, but at the age of 70 Einstein was no longer working in problems of current interest to us. He was working on his unified field theory which none of us was working on. We felt that we should not bother him. He was very old and always very kind. He gave lectures which were well-attended; less than half the audience were physicists, the rest were newspapermen. He did ask me to go to see him when he chanced upon a paper that I had written on something that he was interested in in his younger days. Beyond that, I usually saw him at a distance. But one day it was very fortunate for me. I was taking a walk with my first child (who was four) when we saw Einstein walking to the Institute. (Einstein never learned to drive; he didn't have a car, so he walked about two miles' distance to the Institute everyday.) We saw him coming and so I took my son over to him and said, "Professor Einstein, this is my son. Could I take a picture of the two of you?" He said, "Sure." So I had the picture taken with Einstein patting my son on his head, and my son now has an enlarged photograph hanging on his office wall.

Another physicist that I learned to admire when I was still in Kunming was Dirac. Dirac was born in 1902 and he died at the age of 82. If you had talked to Dirac, you would never forget that experience. He was very different from anyone else. He did not say very much. He only said a few words each time. His sentences were separated by long pauses and those sentences were logically connected in general. For example, there is a good friend of mine named Pais *(1918 -)* who is now a professor at Rockefeller University. When he first met Dirac in 1947 at the Institute for Advanced Study in Princeton, Dirac was visiting. He came to visit the Institute very often in those years. One day Pais was having lunch with Dirac. Pais was eating three sandwiches and was sitting across the table from Dirac, And Dirac said, "Pais, do you always eat three sandwiches?" Pais said, "Yes." So there was a pause, and Dirac said, "Do you always eat these same three sandwiches?" Pais replied, "Yes." A pause. "Do you always eat these three sandwiches in the same order?"

Indeed, if you think about what Dirac said, one characteristic of it was that he did not say very much, but what he said were usually deeply connected with one sentence following from the previous one. That is also characteristic of his scientific papers. Everybody who has read Dirac's papers know that those papers could only have been written by Dirac. There is a logical connectivity to them. There is a logical inevitability from one sentence to the next. So when Dirac has written on something, the most important thing you must do is to first abandon what you have thought before and just follow Dirac, and after you have understood his way of doing logic, you will find that he usually builds a beautiful structure. And it's amazing that Dirac had repeatedly struck on the right structures which have caused several revolutions in physics. In 1925, at the age of 23, he saw a paper by another young man of 24 - Heisenberg *(1901 - 1976)* - and recognised that there was profound truth in it.

Now classical physics - Maxwell's equations, thermodynamics - flowered in the nineteenth century, but by 1890 it became clear that although classical physics was greatly successful, there were new phenomena which could not be explained by classical physics. In particular, there were the spectral lines. The spectral lines were very sharply defined; they were invariant. In fact, Maxwell in the mid-nineteenth century already marvelled at the spectral lines. He said, "Look at these lines. They are at very specific frequencies, they are very regular and are not influenced by anything that we know. We apply pressure on the gases, we heat them up, we do this and do that, but the spectral lines remain very accurately at their spectral positions." Furthermore, these same spectral frequencies or wavelengths are also observed as emissions from stars. There is no classical concept which can explain this. In the classical concept things were continuously changing and so you do not expect lines. You would expect sort of a general spectrum. So Maxwell already knew there was something very peculiar about these things. Clearly very intrinsic but not fitting into any classical picture. By the turn of the twentieth century, it became clear that there was a wealth of information in the spectral lines which represent things which cannot be described by classical physics. So therefore they ushered in a crisis in physics and the first break in the crisis was a paper by a 24-year-old German physicist named Werner Heisenberg. That paper is universally regarded as having caused one of the great revolutions not only in twentieth century physics but also in the intellectual history of mankind. However if you read the paper you will find it extremely difficult. In retrospect, it has obviously deep insight but that is embedded in noise. There is so much noise that the whole thing is unclear, and Heisenberg didn't understand what was the main point of his paper. He had perception, he knew that in this area there will be profound truth but he was confused. In particular, he knew very little mathematics, so he was not able to find out the essence in what he was writing.

The paper came to Dirac. The story was that Dirac heard a lecture by Heisenberg about the subject and later Dirac also got a copy of the preprint. Dirac studied it and he understood what was the essence of what Heisenberg was doing. Namely, in classical physics, when you multiply A by B the result is equivalent to B multiplied by A: AB = BA. That is called commutative algebra. But what Heisenberg launched into was a kind of new algebra which Heisenberg did not extract but which Dirac extracted. Dirac said, "What Heisenberg was doing was a kind of mathematics in which AB is not necessarily equal to BA." That is called non-commutative algebra. It turns out, as many of you know, that matrix algebra is not commutative. If A and B are matrices, AB is not necessarily equal to BA. So Dirac then wrote a series of papers which told what Heisenberg's new algebra was really about. Therefore both Dirac and Heisenberg and a number of other people made revolutionary contributions which between the years 1925 and 1927 resulted in a profound revolution in physics called quantum mechanics.

Heisenberg is one of the greatest physicists of this century. I have told you that at the age of 24 he wrote his paper and Dirac was 23. For a while the older physicists called this type of physics, which they did not understand, could not understand and resisted understanding, "Knabenphysik" ("children physics"). But the "Knabenphysik" was true and caused a great revolution to which many people contributed. There was Einstein who contributed to it in the early years. I told you about one of his great papers of 1905. There was another great paper of 1905 which talked about an entirely different subject and that subject had a lot to do with the original concepts of quantum mechanics. Einstein's ideas had a lot of influence on both fields although from 1925 to 1927 when the formalism of quantum mechanics was developed, he did not write a single paper on this. It was accepted by all the participants that his earlier papers had a great influence on their thinking. There was Bohr *(1885 - 1962)* who constantly asked questions as to how this new type of mathematics was to be interpreted and he played a profound role. And the more specific contributions by Heisenberg, Dirac and by another great physicist Schroedinger *(1887 - 1961)*.

It was a combination of tastes of judgement that went into it. I had earlier said to you that it is important that an aspiring scientist develops his own judgement and his own taste. I am not saying there is only one taste. There are many tastes in physics just as there are many tastes in music and in art. In the case of physics, the miraculous thing is that people with different tastes and inclinations all contributed. In some sense they contributed in complementary ways, and the development of quantum mechanics is a great example of this. It required the Heisenberg insight which was deeply confusing and deeply unclear. It then required the clarifying type of Dirac to lift out of this messy situation the beautiful structure that Heisenberg had introduced. And this interplay occurred repeatedly . In fact, it is very interesting to reflect on how Heisenberg regarded Dirac and how Dirac regarded Heisenberg.

They both had profound respect for each other. Each knew that the other could do something which he could not. They could not imitate each other because they were very different. They had different taste. Dirac characteristically and repeatedly wrote papers that were completely out of the thinking of everybody else. Dirac was standing alone from everybody else. His style was dramatically different. Dirac had just written several papers which were amazingly original. Now you must remember that these people were very young. They were all very brilliant and had already made a name for themselves. They were also very confident. A person who develops a taste is likely to acquire a certain confidence. Why? Because taste means that the person believes some things are good, other things are not. To reach that level, he must have a certain competence. He must have already tested his ideas repeatedly and found that his judgement is good. So these people were very confident people. And yet, Heisenberg repeatedly found that Dirac would introduce a totally new way of thinking and arrive at spectacular results. There was a letter written by Heisenberg to his friend Pauli *(1900 - 1958)*, another great physicist, a young physicist at that time. Heisenberg and Pauli were more alike. He wrote to Pauli: "I have recently done something in a very different field. This is because I found that I have been continually irritated by Dirac." Why did he say this? Because he continually found that he was outsmarted by Dirac in a way he could not understand. Now this coming from a confident and brilliant physicist like Heisenberg is a very significant statement. So I think this letter describes the psychology of physicists in a very vivid way.

After 1925, there was the question of, "Okay, this is a great formalism, but could we solve any real problem such as the hydrogen atom?" The hydrogen atom was the simplest testing ground because one proton and one electron is the simplest two-body problem. Could we solve the equation that describes the hydrogen atom using the new non-commutative algebra. This was a very difficult problem. This was not the direction in which Heisenberg and Dirac were particularly strong. Entered Pauli. Pauli was mathematically extremely powerful. So he used some very ingenious methods and solved this problem. He found that the solutions did agree with the spectral lines. That was a great triumph.

However, the next question came. Take the next complex atom, the helium atom, which has one nucleus with two electrons. It is a three-body problem. The question was to find the spectral lines which came out of this three-body problem. Now immediately a fundamental question arose because it was already known for a long time through experimental spectroscopy that there were two different kinds of helium states called triplets and singlets. In 1926, a famous physicist Goudsmit *(1902 - 1978)*, then in his twenties, went to Copenhagen, and Bohr, who was in his forties, gave him the problem of the helium atom to solve. In particular, Bohr wanted Goudsmit to figure out why the triplet helium state was so different in energy from the singlet state. Now triplet and singlet mean that the two electrons are either having spins parallel to each other or anti-parallel to each other. That was already known a year or two earlier at that time. In fact, Goudsmit was one of the people who proposed those states. Some forty years later, Goudsmit was interviewed by people who were writing the history of quantum mechanics. They asked Goudsmit how he approached the problem in 1926. Well, history showed that he did not solve the problem, but then he revealed what went through his mind. He said, "Well, I figured that if the two particles have vastly different energies this way and that way, it must be because there is a force between these two spins. So I tried to figure out all kinds of possible forces between interactions this way and between interactions that way. After I proposed these various possibilities, I calculated the numerical values and the result was always extremely small. So I was totally discouraged by the result."

The problem was solved within a few months by Heisenberg through a completely different idea. And forty years later, Goudsmit in reflecting on this said that the problem was completely out of his depth. It was characteristic of Heisenberg's genius that though less experienced than Goudsmit and much less experienced than Bohr, he was able to see that the spin parallel and anti-parallel had some other connotation than that which Goudsmit had a fixation on. The explanation came from a symmetry consideration and Heisenberg was the first to have the insight that it was through the influence of the spin orientation that there was a difference of symmetry and that the difference of symmetry could lead to tremendous differences in energy. But the paper of Heisenberg was extremely confusing because it had this fundamental idea together with a lot of other noise which was wrong. So you have to learn to understand which was the right idea and which was the wrong idea. This was characteristic of Heisenberg's physics whereas Dirac was crystal clear. This tremendous difference between the two is characteristic of frontier work in science.

If you think about more recent physics, you will find that different physicists have entirely different styles. I think one lesson I would like to draw with you is that for the progress of science a student or a research worker must develop his own style. If he does not, he will not be able to concentrate. By developing his own style, he singles out those matters and approaches which he considers important. If these happen to bear on the then important problems in the right way, he has a possibility of making a great contribution. In retrospect, you will always find that the greater the breakthrough is, the more people of different tastes it requires. Each contributes to a different aspect of the same problem, and that has repeatedly happened in the history of physics.

Now I have mentioned that Einstein and Dirac are the two people that I admired most. There is a third and that is Fermi. I learned to admire Fermi greatly when I was a student in Xinan Lianda. Of these three, Fermi was the one who seemed to me to be the most accessible as a thesis advisor. I knew that Einstein was old (he was in retirement) and he was working on a specific problem which very few other people worked in. Dirac was in England; in any case, Dirac was a person who almost never had graduate students. So I wanted to work with Fermi. Fermi was a great physicist of the twentieth century. His style is again different. He had brilliant mathematical insight; in fact, he had written some very important papers in differential geometry. There is a theorem in differential geometry called Fermi's Theorem. If you want to know what that is, take the book *Riemannian Geometry* by Eisenhart *(1876 - 1965)* and in it there is one exercise which is this theorem due to Fermi. Although Fermi had great mathematical insight and ability, he was much more interested in physics. In fact, his interest in physics is very characteristic of him and is very different from that of any other people I mentioned before. He was able to look into many phenomena. He was much closer to the phenomenon aspect of physics than any of the other physicists that I have talked about so far. He was able to amalgamate the most practical phenomenological aspects of physics with the most abstract. He moved back and forth among them with great ease. After the second world war, he was a towering figure in physics in the sense that if you wanted to know something about the practical aspects of physics or if you wanted to know how these practical aspects were related to fundamental physics, you knew he was the man to ask.

He kept a large series of notebooks into which he entered all his learning. I later learned to know him very well when I became a graduate student at Chicago. The reason I went to Chicago as a graduate student was because I learned that Fermi had moved to Chicago or about to move to Chicago. I wrote a paper with him in 1949. He had a great influence on the way I tried to study physics and to do physics later. He would get into one subject and would usually concentrate on it for maybe two or three weeks, depending on the difficulty of the subject. This may be something that was writtten as a piece of research or some topic which he wanted to learn and which he knew had already been studied by other people. He would think about it and sometimes he would read what other people had done about it. Then at the end of that period, after he had digested what had been done by himself or other people, he would write a short summary and enter it into his notebook of that month. This is usually a rather concise summary but it starts from the abstract ideas, goes through the fundamentals, gives the concluding equations and usually there are some plots of these equations. He was not just satisfied with the given abstract equation which most theoretical physicists would be satisfied with. He would then put in a few numbers and make a plot. So this would become, say five pages of his notebook. Then he would move on to another subject. So after a couple of months, this notebook would be finished and he would make a careful index at the back: page 1 to 5 is about neutron diffusion, page 6 to 20 is about the influence of the earth's magnetic field on cosmic rays, and so on. Over the years, he had a huge collection of notebooks. These notebooks have now been contributed to a museum in Italy. These notebooks are great because they have across the board knowledge about the whole of physics in great details. So when a subject came up, you could depend on it that Fermi not only could supply you with the abstract ideas and the equations, but he also knew what the numbes were.

He was also very great at taking a complicated formula and extracting from it approximations which he could remember in his head and then use. An example of this is the following. In 1945, he was one of the principal participants in the design of the first atomic bomb which was tested in New Mexico. Everybody who went to observe the test was told to lie face down and not look at the explosion but they were told that after a count of maybe ten they could look. Then they all looked and saw the mushroom cloud coming up. And then the blast came (the shock wave came), and Fermi had already prepared for this. He had some paper torn into small pieces, and he held them in his hand and let go. So the pieces of paper dropped down and the shock wave blew them over various distances. He took an estimation of how far each piece of paper had gone. He already had a formula made up in his mind and he fitted the experimental data of his simple experiment into his formula, and said ,"This bomb is about 20 kilotonnes of TNT."

This was characteristic of his integration of the most abstract with the most practical. Fermi was unique in this. I was an admirer of him and that was the reason why I went to Chicago. Indeed, under his influence, I learned what was important in physics and what was the right way to choose questions in physics and to approach the answers.

Let me now mention Edward Teller *(1908 -)*. When I went to Chicago, Fermi was one of the professors and was a towering figure not only at the University of Chicago but in physics altogether in those days. Another professor was Teller. He was about 40 at that time. He had not yet made a big name. Later he had the brilliant idea about how the hydrogen bomb was to be made, so he is oftentimes called the "father of the hydrogen bomb". Teller was my thesis advisor. The thesis I wrote in Chicago in 1948 was with Teller. His way of doing physics was an eye-opener. He is a man with many ideas. I would say he has ten bright ideas everyday but nine of them are wrong. But of course, if you had one bright idea right each day, you would have made a contribution, and he did. He also was very anxious to discuss with whoever was willing to discuss with him. It was very impressive to watch him discussing with others. He was not afraid to tell you an idea which might not be right. This was an eye-opener for me because in China I had been told I should not open my mouth until I knew I was right. Not with Teller. He was exuberant and bubbling with ideas, and he knew many of his ideas were not right, but who cares what is right or wrong?

The process of discussing them was what he liked and if you interacted with him, you would find that yes, he would have an idea, and if you said that it was not right, he would argue with you. When you did have a good point, he would be convinced that you were right and he would not insist on his ideas. So discussions with Teller were extremely stimulating and also, he oftentimes would ask his students, "Are you having fun?" This was an important eye-opener for me too. In China, we had the impression that to study is to work hard and to suffer. In fact, you know this is also true in the Japanese culture. The Japanese word for "studying" is *benkyo* (*mianqiang* in Chinese characters, meaning "to be forced into doing something"). Not Teller. Teller wants to have fun. He wants to have discussions and through these discussions both parties would understand more things, and this brings great enjoyment. I learned that the way we learned things in China was one way of doing things, but not the only way. This way of Teller - with great exuberance, with great inclination to argue with people and through these arguments to arrive at new understanding - was also an exceedingly important way of doing things. People may choose which mode of operation they like, but they are both important ways of advancing one's knowledge in science.

Lastly, I will tell you something about one physicist whom I have got to know quite well, and this is Feynmann. He is also an extremely exuberant physicist and he is singularly a unique, original physicist. Almost everything that he does is different from how others do it. He was born in 1918 and is currently still a professor at Caltech. He is also a great performer and enjoys performing. There are many stories about him. In particular, he is a great drummer. He can use his two hands to beat two different kinds of beat. It's incredible. With one hand, he would make a three-beat, and with the other, he could make a seven-beat. He himself told the story that during the wartime when he was a young man, he was tested for his physical condition because he might be drafted into the army. He went to see the doctor and the doctor said to him, "Stick out your hands." So he did this. *[Professor Yang stretched out his hands with left palm facing upward and the right palm facing downward.]* "No, no, no, turn them over." *[Professor Yang turned over both palms simultaneously so that they again face opposite directions.]* He was rejected as unfit.

One day, some twenty years ago, there was a BBC man who went to the United States to interview physicists. They first went to see Geoffrey Chew *(1924 -)* in Berkeley. "Chew" is also an English name; he is not Chinese. Chew is very famous and the interviewer asked him questions about what he was doing in physics. At the end of the interview, the BBC man said, "Professor Chew, how old are you?" Chew said, "I am 41." So the BBC man said,"Professor Chew, I understand a theoretical physicist's 'life' is short. A theoretical physicist who is over 40 is finished. Would you like to comment?" Chew thought about it and said, "No comment." The next day, the BBC man went to talk to Feynmann at Caltech, and after the interview was over, the BBC man said, "How old are you, Professor Feynmann?" Feynmann said, "I am 46." So the interviewer said, "Yesterday I went to see Professor Chew of Berkeley and asked him how old he was. He said he was 41. So I said to him, 'I understand a theoretical physicist is finished by the age of 40.' I asked him to comment, and he didn't comment. Would you comment?" Feynmann said, "Sure, Chew is finished."

Well, I think I have spoken long enough.