World Scientific

Special Newsletter on C. N. Yang (Nobel Laureate)

C. N. Yang in 2015

Conversation and Pieces

World Scientific
Connecting Great Minds

Paper I

The Future of Physics Revisited* 2015

C. N. Yang

I

In April 1961 there was a big Centennial Celebration at MIT. That was when science and technology were making unprecedented progress for mankind and when the United States had just inaugurated her young and ambitious new President. Naturally it was a proud, joyous, even intoxicating Celebration. At the week-long event there was a panel discussion on "The Future of Physics", chaired by Francis Low, with four speakers in the following order: Cockcroft, Peierls, Yang and Feynman. It was originally understood that the talks were to be published by MIT, but somehow that never happened. Much later the talks by Cockcroft and Peierls were summarized by Schweber in his 2008 book "Einstein and Oppenheimer". My talk and Feynman's were published, respectively in 1983[1] and in 2005.[2]

In my talk I said,

> *"But since there seems to be too ready a tendency to have boundless faith in a "future fundamental theory", I shall sound some pessimistic notes. And in this Centennial celebration, in an atmosphere charged with excitement, with pride for past achievements and an expansive outlook for the future, it is perhaps not entirely inappropriate to interject these somewhat discordant notes."*

I then argued that to reach the present level of understanding of field theory, according to Wigner's counting, one must penetrate four levels of physical concepts formed out of experiments. Furthermore to reach deeper levels of penetration will become more and more difficult.

*Based on talk given at the Conference on 60 Years of Yang–Mills Gauge Field Theories, 25–28 May 2015, NTU, Singapore.
Editorial comment: We think this is a very interesting article. For the convenience of the readers, we reprint below the talks by Yang and by Feynman at that 1961 panel (App. A and App. B).

> *"Here physicists are handicapped by the fact that physical theories have their justification in reality. Unlike the mathematicians, or the artists, physicists cannot create new concepts and construct new theories by free imagination."*

I was followed by Feynman, who began as follows:

> *"As I listened to Professor Cockcroft, Professor Peierls and Professor Yang, I found that I agreed with almost everything they said.* (But) *I don't agree with Professor Yang's idea that the thing is getting too difficult for us. I still have courage. I think that it always looked difficult at any stage."*
>
> *"One possibility is that a final solution will be obtained. I disagree with Professor Yang that it's self-evident that this is impossible."*
>
> *"What I mean by a final solution is that a set of fundamental laws will be found, such that each new experiment only results in checking laws already known."*

Feynman was one great intuitive theoretical physicist of my generation. Reading these passages today, I wonder

(1) what type of "final solution" he had in mind in 1961, and
(2) whether he still held such very optimistic views later in his life.

II

What have we learned in the fifty odd years since that 1961 panel?
A lot. Through very intensive collaborative efforts between theorists and experimentalists important conceptual developments were proposed and verified:

- One specific model of Symmetry Breaking
- Electroweak Theory
- Renormalizibility of non-Abelian Gauge Theory
- Asymptotic Freedom and QCD

Capping these developments was the dramatic experimental discovery in 2012 of the Higgs particle.

We have now a workable

$$\textbf{\textit{standard model}}\textbf{, a SU(3)} \times \textbf{SU(2)} \times \textbf{U(1) gauge theory}\,.$$

Thus in these fifty odd years since 1961, we have reached *one more layer* of physical concepts, which is based on *all* the previous layers *plus* a large number of very large experiments.

III

Are there additional layers of physical concepts deeper than those that we have reached so far? *I believe yes, many more.*

When can we expect to reach the next level? *I believe in the distance future, if ever.*

Why are you so pessimistic? *I am not pessimistic, I am just realistic.*

References

1. C. N. Yang, *Selected Papers* (Freeman, 1983), p. 319; E. P. Wigner, *Proc. Amer. Phil. Soc.* **94**, 422 (1950) [Reprinted below as Appendix B].
2. M. Feynman (ed.), *Perfectly Reasonable Deviations from the Beaten Track*: *The Letters of Richard P. Feynman* (Basic Books, 2005), Appendix III [Reprinted below as Appendix A].

Appendix A

The Future of Physics‡

Richard P. Feynman

As I listened to Professor Cockcroft, Professor Peierls and Professor Yang, I found that l agreed with almost everything they said. I don't agree with Professor Yang's idea that the thing is getting too difficult for us. I still have courage. I think that it always looked difficult at any stage. On the other hand, I agree, as you will see, with something about this pessimism, and I don't think that I can add anything sensible to anything that the other speakers said. So in order to proceed, I have to add something that is not sensible, and if you will excuse me therefore, I am going to try to say something quite different than what they said.

First of all, to make the subject not infinite, I am going to limit myself very much and discuss only the problem of the discovery of the fundamental laws in physics — the very front line. If I were talking about that which is behind the front line, things like solid-state physics, and other applications of physics and so on, I would say very different things. So please appreciate this limitation of the discussion.

I do not think that you can read history without wondering what is the future of your own field, in a wider sense. I do not think you can predict the future of physics alone with the context of the political and social world in which it lies. If you want to predict, as Professor Peierls does, the physics a quarter of a century in the future, you must remember that you are trying to predict the physics of 1984.

‡As published in The Technology Review, 1961–1962. Extracted from "Perfectly Reasonable Derivations from the Beaten Track: The Letters of Feynman" (Basic Books, 2005), Appendix III.

The other speakers seem to want to be safe in their predictions, to they predict for 10, perhaps 25, years ahead. They are not so safe because you will catch up with them and see that they were wrong. So, I am going to be really safe by predicting 1,000 years ahead.

What we must do according to the method of prediction used by the other speakers is to look at the physics of 961 and compare it to the present, 1961. We must compare the physics even a century before the age when Omar Khayyam could come out the same door as in he went, to the physics of today as we open one door after the other and see rooms with treasures in them, and in the backs of the rooms five or six doors obviously going into other rooms with even greater treasures. This is a heroic age. This is an exciting time of very vital development in the fundamental physics and the study of the fundamental laws. It is not fair to compare it to 961, but to find another heroic age in the development of physics, the age of, perhaps, Archimedes, of Aristarchus; say, the third century B.C. Add 1,000 years and you find the future of their physics: the physics of the year 750! The future of physics depends on the circumstances of the rest of the world and is not merely a question of extrapolation of the present rate of progress into the future. If I go a thousand years, I have a difficult problem. Is it possible that it's all going to be over?

One of the most likely things, from a political and social point of view, is that we have soon a terrific war and a collapse. What will happen to physics after such a collapse? Will it recover? I would like to suggest that the physics, fundamental physics, may possibly not recover, and to give some arguments as to why not.

In the first place it is very likely that if there were sufficient destruction in the Northern Hemisphere the high-energy machines, which seem to be necessary for further research, would become inoperative. The machines themselves may be destroyed, the electrical power to operate them may be unavailable, and the industrial technology needed to repair or maintain them may no longer exist, at least for a while. Experimental physics techniques are the quintessence of our technological and industrial abilities, and so they must suffer some temporary setback.

Can physics slide back temporarily and then recover? I don't think so. Because, in order to have this heroic age an exciting one, one must have a series of successes. If you look at the grand ages of different civilizations, you see that people have an enormous confidence in success, that they have some new thing that is different, and that they are developing it by themselves. If one were to slide back, you would find for a while, no great successes. You would be doing experiments that were done before. You would be working on theories that "the ancients" knew very well. What could result would be a lot of mouthing and philosophizing; a great effort to do the physics in the sense that one should do it to be civilized again, but not really to do it. To write, instead, commentaries, that disease of the intellect, which appears in so many fields. Physics is technically too hard to recover immediately. There would be practical problems at that time that would occupy the attention of intelligent people. The difficulty is that there would be no fun in it. The new discoveries wouldn't come for a while. The other feature is that it would not be useful. No one has yet thought of a use of the results of the experiences we have with the high-energy particles. And finally, it is possible that antagonism is produced by the terrible calamity; there might be a universal antagonism toward physics and physicists as a result of the destruction which people might blame on the scientists who made it possible. Another thing to remember is that the spirit of research may not build itself up again because this spirit is concentrated in the Northern and advanced industrial countries, and this spirit does not exist fully in the other countries.

Well, I said 1,000 years. Maybe in that much time there will be another renaissance. What kind of machinery could there be for a recovery from this thing? (I said I wouldn't talk about anything sensible. I can't.) Some success somewhere must be the cause of a new renaissance. Where will this success lie? Perhaps in other fields. Perhaps in some other field than physics one would find a new age developing a success above "the ancients" and then getting a new confidence and growing. When this grows, it can pass its enthusiasm to physics. Or perhaps there would be a new aspect to physics, some other point of view, or some other completely different thing. That I cannot tell.

Another interesting possibility is that the renaissance may lie in some nation or people discovering a success by using a scientific attitude as a kind of morality, in society, government and business. You know what I mean — when someone says something, looking for what it is that they are saying, not why they are saying it. Propaganda would be a dishonest thing, and no one would pay attention to someone who would say something not for the content of the idea they want to get across, but because they want to show that they are big, or good, or some such reason. It is possible that if any success results from using such a scientific attitude, a country would be encouraged to go on from this success in its society and develop a re-interest in the scientific problems.

Well, now let's take the opposite view. Suppose there is no collapse. How, I don't know, but suppose there is no collapse. Then what? Suppose we can imagine a society somewhat like our own continuing for a thousand years. (Ridiculous!) What would happen to fundamental physics, the fundamental problems, the study of the laws of physics?

One possibility is that a final solution will be obtained. I disagree with Professor Yang that it's self-evident that this is impossible. It has not been found yet, but if you were walking through a building to get from one side to the other, and you hadn't yet reached the door, you could always argue, "Look, we've been walking through this building, we haven't reached the door, therefore there is no door at the other end." So it seems to me that we are walking through a building and we do not know whether it is a long infinite building, or a finite building, so a possibility is that there is a final solution.

What I mean by a final solution is that a set of fundamental laws will be found, such that each new experiment only results in checking laws already known, and it gets relatively more and more boring as we find that time after time nothing new is discovered that disagrees with the fundamental principles already obtained. Of course, attention would then go to the second line about which I am not speaking. But the fundamental problem will have been solved.

I would say that one thing that would happen if such a final solution were found, would be the deterioration of a vigorous philosophy of science. It seems to me that the reason that we are so successful against the encroachment of professional philosophers and fools on the subject of knowledge, and the way of obtaining new knowledge, is that we are not completely successful in physics. We can always say to such people, "That was very clever of you to have explained why the world just has to be the way we have found it to be so far. But what is it going to look like to us tomorrow?" Since they are absolutely unable to make any predictions, we see that their philosophy does not have real understanding of the situation. But if the solution is all present, how many people are going to prove it had to be four dimensions, it just had to be this way because of such and such and so forth. And so the vigor of our philosophy, which is a vigor which conies from the fact that we are still struggling, I think that may fail.

What other possibilities are there? Suppose that the building we are walking through is infinite, as Professor Yang feels. Then there will be a continual exciting unfolding. We will rush through this house, one door after another, one treasure after another. A thousand years! Three unfoldings in sixty years is fifty unfoldings in a thousand years. Is the world going to have fifty exciting revolutions of our basic physical ideas? Is there that much treasure in fundamental physics? If there is, it will become somewhat boring. It will be boring to have to repeat it twenty times, this fact that things change always when you look deeper. I do not believe that it can last 1,000 years of active investigation. Well, if it doesn't stop (I mean if you can't get the final solution), and if I can't believe that it will keep on being excitingly developed for fifty revolutions, what else is there?

There is another possibility, and that is, that it will slow up. The questions will become more difficult, How will it look then? The strong couplings are analyzed, the weak couplings partially analyzed, but there are still weaker couplings that are harder to analyze. To obtain useful experimental information has become extremely difficult because the cross-sections are so tiny. Data comes in slower and slower. The discoveries are made more and more slowly, the questions get harder and harder. More and more people find it a relatively uninteresting subject. So it is left in an incomplete state with a few working very slowly at the edge on the question of what is this third-order tensor field that has a coupling constant 10 to the power of -30 times smaller than gravity?

It is possible, of course, that what we call physics will expand to include other things. I believe, for example, that physics will expand, just as Professor Peierls says, into the studies of astronomical history and cosmology. The laws of physics, as we presently know them, are of this kind. Given the present condition, what is the future? The laws are given by differential equations in time. But there must be another problem: what determines the present condition? That is, what is the whole history of the development of the universe? One way to see that this may someday be a part of physics and will not always be called astronomical history is to note there is at least a possibility that the laws of physics change with time. If the laws of physics change with absolute time, then there will be no way to separate the problems of formulating the laws and of finding the history I think that it is very likely that cosmological problems will be enmeshed in physics.

Finally, I must remind you that I limited myself to talking about the future in fundamental physics. I'd say that there will be an important return from the front line into the applications and the development of the consequences of the laws. This will be a very exciting thing, and I would say quite different things about its future than I would say about the future of the fundamental laws.

We live in a heroic, a unique and wonderful age of excitement. It's going to be looked at with great jealousy in the ages to come. How would it have been to live in the time when they were discovering the fundamental laws? You can't discover America twice, and we can be jealous of Columbus. You say, yes, but if not America, then there are other planets to explore. That is true. And if not fundamental physics, then there are other questions to investigate.

I would summarize by saying that I believe that fundamental physics has a finite lifetime. It has a while to go. At the present moment it is going with terrific excitement, and I do not want to retire from the field at all. I take advantage of the fact that I live in the right age, but I do not think it will go on for a thousand years.

Now, to finish, I would summarize two points. First, I did not talk about applied physics or other fields, about which I would give a considerably different talk. And second, in these modern times of high-speed change, what I am forecasting for a thousand years will probably occur in a hundred.

Thank you.

Appendix B

The Future of Physics†

C. N. Yang

In the last four or five years much effort and attention have been devoted by theoretical physicists to the analytic continuation from physically observable experience into unphysical regions. In particular, it has been tried by extrapolation to study properties of the singularities in the unobserved region. Such attempts have been beset from the beginning with great difficulties. But interest in them maintained. In a similar spirit, this morning we try to pursue a parallel approach: By extrapolation we want to look beyond our past experience and learn something about the so far

†Panel discussion by C. N. Yang at the MIT Centennial Celebration, April 8, 1961.

unobserved future development of physics. We cannot hope for any real success in this pursuit. But I believe we all agree the attempt is highly interesting.

By all standards the achievements in physics in the twentieth century so far have been spectacular. Whereas at the turn of the century the atomic aspect of matter was just emerging as a new subject of study, we have today progressed dimensionwise by a refinement of a factor of a million: from atomic dimensions to subnuclear dimensions. Energywise the progress is even more impressive: from a few electron volts to multibillion volts. And the power and ingenuities of the experimental techniques have fully kept pace with the increased depth of the physicists' inquiries. The influence and impact brought about by the advances in physics upon other sciences — upon chemistry, astronomy, and even upon biology — have been important beyond description. Similar influence upon technology, upon human affairs, has been so preoccupying in the postwar years as to need no further emphasis here.

But it is not in these influences that the glory of physics and the heart of the physicist lie. It is not even in the continued enlargement of the domain of physical experimentation, important as it is, that the physicists take the greatest pride and satisfaction. What makes physics so unique as an intellectual endeavor lies in the possibility of the formulation of concepts out of which, in the words of Einstein,[1] a "comprehensive workable system of theoretical physics" can be constructed. Such a system embodies universal elementary laws, "from which the cosmos can be built up by pure deduction."

Judged by such a lofty and rigorous standard, the sixty years of the twentieth century stand out as nothing short of heroic; for besides the numerous important discoveries that *widened* our knowledge of the physical world, this period has witnessed not one, not two, but three revolutionary changes in physical concepts: the special relativity, the general relativity and the quantum theory. Out of these conceptual revolutions were constructed *deepened*, comprehensive and unified systems of theoretical physics.

Endowed with such a distinguished heritage of the recent past, what are the prospects for its future?

Surely the rapid widening of knowledge will continue, both in what Professor Peierls refers to as the foundation of physics and in what he calls behind the front line.

In the former our present knowledge is sufficient to enable us to say with some certainty that great clarification will come in the field of weak interactions in the next few years. With luck on our side we might even hope to see some integration of the various manifestations of the weak interactions.

Beyond that we are on very uncertain grounds. To be sure we can already formulate a number of questions, the answers to which seem to us at this moment to be crucial: How should one treat a system with an infinite degree of freedom? Is the continuum concept of space time extrapolatable to regions of space 10^{-14} cm to 10^{-17} cm, and to regions smaller than 10^{-17} cm? What are the bases of the invariance under charge conjugation, and the invariance under isotopic spin rotation, both of which, unlike space–time symmetries, are known to be violated? What is the unifying basis of the strong, the electromagnetic and the weak interactions? What is the role of the gravitational field relative to all these? The list can go on, but we are not even sure that these questions are meaningful as we here phrased them: in fact much of the progress in physics had developed from the very recognition of the meaninglessness of some previously asked questions.

Of one thing, however, we can be sure. The accumulation of knowledge will proceed at a very rapid pace. We need only remind ourselves that not so very long ago, the time scale of discoveries in physics was measured in years, if not in decades. The Michelson–Morley experiment, for example, was first performed in 1881, then repeated with greater precision in 1887. To explain the negative result of the experiment Fitzgerald invented a contraction hypothesis in 1982, and then Lorentz invented the Lorentz transformation in 1902, culminating in Einstein's special theory of relativity in 1905. Imagine that Michelson's first experiment were done today!

The general awakening of mankind to the importance of science and the amazing ingenuity of the human mind at technological creativity virtually ensure for us further quickening of the pace of the experimental sciences.

But where shall we stand with respect to the "comprehensive system of theoretical physics" that was referred to a few minutes ago? Can we reasonably expect further success in the glorious tradition of the first sixty years of the twentieth century?

If it is difficult to locate singularities of functions by extrapolation, it is as difficult to predict revolutionary changes in physical concepts by forecasting. But since there seems to be too ready a tendency to have boundless faith in a "future fundamental theory," I shall sound some pessimistic notes. And in this Centennial celebration, in an atmosphere charged with excitement, with pride for past achievements and with an expansive outlook for the future, it is perhaps not entirely inappropriate to interject these somewhat discordant notes.

First, let us emphasize again that the mere accumulation of knowledge, while interesting, and beneficial to mankind, is nonetheless quite different from the aim of fundamental physics.

Second, the subject matter of subnuclear physics is already very remote from the direct sensory experience of mankind, and this remoteness is certainly going to increase as we delve into even smaller dimensions. A direct and graphic proof of this can be easily found in the growing physical size of the laboratories, the accelerators, the detectors and the computers.

What we call an experiment today consists of elaborate operations with elaborate equipments. To make any sense at all of the results of an experiment, concepts have to be formulated on all levels between our direct sensory experience and the actual experimental arrangement. The difficulty inherent in this state of affairs is as follows. Each level of concepts is connected with and in fact built upon the previous levels. When inadequacies manifest themselves, one must reach for greater depth by examining the whole complex of previous concepts. The difficulty of this task rapidly diverges with the depth of the considerations, much like in chess playing, it becomes increasingly difficult to practice always examining one more move ahead as one's skill improves.

According to Wigner's counting,[2] to reach our present investigations in field theory one must penetrate at least four levels of concepts. The details of the counting may be a subject of discussion, but there is no denying that what we envisage as a construction of a deeper and more comprehensive theoretical system represents at least one more level of penetration. Here physicists are handicapped by the fact that physical theories have their final justification in reality. Unlike the mathematicians, or the artists, physicists cannot create new concepts and construct new theories by free imagination.

Third, Eddington[3] has once given the example of a marine biologist who uses a net with 6 inch holes and who after careful and long studies formulates the law that all fish are larger than 6 inches. If this imaginary example sounds ridiculous, we can easily find examples in modern physics where because of the complexity and indirectness of the experiments, it has happened that one does not realize the selective nature of one's experiments, a selection that is based on concepts which may be inadequate.

Fourth, in the day-to-day work of a physicist it is very natural to implicitly believe that the power of the human intellect is limitless and the depth of natural phenomena finite. It is of course useful, or as is sometimes said, healthy, to have faith so as to have courage. But the belief that the depth of natural phenomena is finite is inconsistent and the faith that the power of the human intellect is limitless is false. Furthermore, important considerations have also to be given to the fact that psychological and social limitations on the development of the creative ability of each individual may be effectively even more stringent than natural limitations.

Having voiced these cautionary remarks, we must ask are they relevant to the development of physics, say, in the remaining forty years of this century? We cannot know the answer to this question now, but let us hope that it is in the negative.

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2. E. P. Wigner, *Proc. Amer. Phil. Soc.* **94**, 422 (1950).
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Paper II

C. S. Wu's Contributions: A Retrospective in 2015*

C. N. Yang

I

Chien-Shiung Wu started her research at Columbia University after World War II. The area she was working in, beta decay, was popular at that time. Wu had a solid foundation in theory and excellent ability to conduct experiments, so she quickly became famous. Around 1948, the major problem puzzling physicists working on beta decay was: Fermi's theory, Konopinski's theory, or other theories, which one is correct?

Many experiments gave different results, which was very confusing.

Wu realized that the thin films used in the majority of these experiments were too thick. She thus elaborately designed an extremely thin film to conduct a new experiment. After the experiment, she published a brilliant paper in 1949,[1] "*The Beta-Ray Spectra of* Cu^{64} *and the Ratio of* $N+/N-$", with the conclusion:

> *"The good agreement between the theoretical and experimental curves in Fig. 2 indicates that the Fermi theory probably does approximate the true distributions for negatrons and positrons at low energies. In any event, any remaining true deviations must be much smaller than has been previously suggested."*

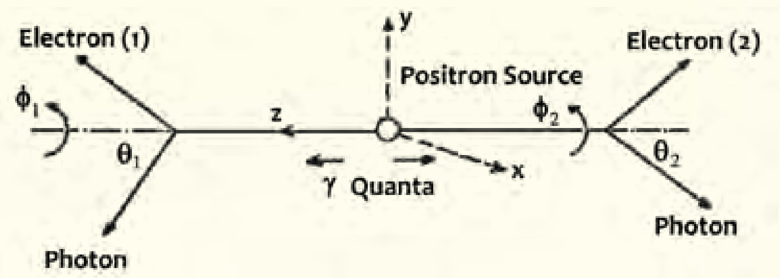
Thus Fermi's theory was verified to be correct! And Wu became the leading physicist in the experimental field of beta decay!

*Based on a presentation dedicated to the 103rd anniversary of the birth of C. S. Wu, celebrated at the C. S. Wu Institute of Technology, China, 28 May 2015.

II

In the same year, she published another paper,[2] "*The Angular Correlation of Scattered Annihilation Radiation*", studying the polarizations of the two γ in

$$e^+ + e^- \to \gamma + \gamma\,.$$



That was the first experiment on *quantum entanglement*, which is a very hot new area of research in the 21st century.

III

In the summer of 1956, Chien-Shiung Wu undertook the greatest experiment in her life.

"Experimental Test of Parity Conservation in Beta Decay."[3]

She discovered that parity is not 100% conserved.

This discovery of parity nonconservation was front page news on *The New York Times*, January 16, 1957, under the headline

Basic Concept in Physics Is Reported Upset in Tests

Conservation of Parity Law in Nuclear Theory Challenged by Scientists at Columbia and Princeton Institute

IV

Eight years later, Wu published another very important paper,[4] "*Experimental Test of the Conserved Vector Current on the Beta Spectra of B^{12} and N^{12}*", with the following conclusion:

> *"This investigation confirms that the deviations from the allowed shape of the observed beta spectra for B^{12} and N^{12} have the correct magnitude and sign due to the weak magnetism term. This unique relation between the beta interaction and electrodynamics strongly supports the conserved vector-current theory."*

Thus she verified the conservation of vector current (CVC), thereby influencing later theories:

- The Electroweak Theory
- The Standard Model

In a comment in April 1997, I wrote:

> *"C. S. Wu's work is world-renowned for her accuracy, but her success derives from an even more important factor: In 1956, nobody was willing to do an experiment to test parity conservation, why then was she willing to undertake this difficult task? Because she alone had this wisdom, believing that even if parity was not overthrown, this fundamental law should nevertheless be proven experimentally. This is where she surpassed all others."*

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Paper III

The conversation between C. N. Yang and students during the Conference on 60 Years of Yang-Mills Gauge Field Theories in Singapore, May 2015

On 27 May 2015, six C.N. Yang scholars from Nanyang Technological University, Singapore, had the privilege of conducting an informal discussion with Prof. Yang. Coming from different faculties and subject groups, the students represented the spectrum of subject areas that were influenced by Prof Yang's work. Centring on the topics of inspiration and research, Prof. Yang, accompanied by Prof. Kok Khoo Phua (Director, Institute of Advanced Studies, Nanyang Technological University) and Prof. Choo Hiap Oh (Professor, Department of Physics, National University of Singapore), dispensed advice with some humour.

Prof. Yang: Let me say something. I think everybody has some speciality in some things. Some of these are obvious, others are somewhat hidden. If you pay attention, you may find that there are some things that you are particularly **interested** in. It is good for you to think about it, analyse it, and see whether it is a direction that is worth exploring further.

I will give you two examples. You know, one of the great mathematicians of the 20th century was Prof. Chern Shiing-Shen, who was born in 1911 and passed away in his 90s. His greatest work was a paper that was published in 1944, called *'A simple intrinsic proof'*.

Throughout his life, he said that this was his greatest contribution, and it was, because with this theory, it was possible to find new features of different branches of mathematics: geometry, algebra, analysis and topology. All of these four areas of mathematics are deeply related to this simple paper which was only seven pages long. How did he produce these seven pages? As a graduate student, he was very interested in a well-known theorem called the *'Gauss-Bonnet theorem'*. The *'Gauss-Bonnet theorem'* is a generalization of the *'Euler characteristics'*. Do any of you know about the *'Euler characteristics'*? A cube has 6 faces, 8 corners and 12 edges. Next consider a tetrahedron or an octahedron or an icosahedron or even a bigger solid. It does not need be a regular solid, it can be an irregular solid but is formed out of cutting planes. For each of these solids, you will have three numbers, the number of corners C, the number of edges E, and the number of faces F. As you may already know, centuries ago, Euler formulated a theorem:

$$C-E+F=2$$

for any such solid. This beautiful simple theorem got generalised in the 19th century and formed a differential geometry theorem called *'Gauss-Bonnet'*. Chern was very ***interested in this theorem***. He felt that this theorem was very beautiful, so he kept on thinking about it and then realized that a new differential geometry technology invented by a great French mathematician Elie Cartan could be used to simplify its proof.

In 1943, he went to Princeton and a friend of his, a few years older, a great mathematician, named André Weil, showed him a paper of more than a hundred pages which generalized the *'Gauss-Bonnet theorem'* to a higher dimension. It was a very complicated paper, but it contained an integral. With Chern's **interest** about the *'Gauss-Bonnet theorem'* and with his knowledge of the mathematical method he had learnt from Cartan, he studied this integral and within two weeks wrote that seven-page paper which revolutionized mathematics.

Prof. Yang: Let me give you a second example. We were talking at this conference about my contribution called the 'Yang–Mills theory'. How was it that I did formulate that theory with Mills in 1954? When I was a graduate student in Kunming there was already a theory called 'Gauge theory' of electromagnetism. It was very simple and very beautiful, though not very useful. Somehow I felt that this was something that I *cared about, and gave my attention to* it. Later when I was in Chicago new experiments showed that there were many new particles. So I asked: "There are so many new particles. They will have interaction with each other. What is the *principle* of how they interact?" Since I had liked the 'Gauge principle for electromagnetism' in Kunming, it was natural to try to generalize this *simple principle* to other interactions. That led to the 1954 paper with Mills.

If there is something which intrigues you, which you think is very beautiful or very elegant or very powerful, think more about it, get thoroughly acquainted with it . Later with proper new stimulation, you may be able to twist it around and create a new direction of research.

Erickson Tjoa: My question is, sometimes it gets very hard to pick out a meaningful and interesting question with so much information. Some of them have progressed so much that we cannot see an open question that we can even understand. So how do we match the fact that we have to look for interesting questions?

Prof. Yang: I do not think there is any general rule. Let me give you an example of a successful idea. When I was at Stony Brook, a distinguished mathematician, Prof. James Simons, who was the chairman of the mathematics department and is 15 years younger than me, went to Wall Street around 1980s and became a billionaire. He is currently one of the leading hedge fund managers in the world. So I asked him: "What is the secret of your success as a fund manager?" He said, "Usual fund managers would look at stock information every day and formulate their speculations about the future." He added, "That is not enough, I need massive data." So he hired around 50 PhDs in physics and mathematics working for him. They all collect information, not daily, not hourly, but every five minutes. One of them may be specialising in energy stocks, and he would collect information for energy stocks every 5 minutes. Others do it similarly for chemical stocks. It is out of that massive amount of information that they perceive certain patterns. They had seminars like physics seminars and one of these PhDs may say, "I see certain patterns", and they would discuss it and arrive at a majority view. If these patterns were likely to be correct, they would make a massive investment into that particular stock. It was a new kind of analysis *using massive amount of data and mathematical theories*.

When I was a graduate student in Chicago, the great Enrico Fermi always had lunch with us and we always asked him: "Should we tackle big problems or small problems?" His answer was: "Tackle small problems most of the time, and only tackle big problems occasionally." I followed that advice. I think it was a very good advice. The problem that Dr Ma Zhong-Qi reported on was a very special problem, and those are the types of problems that I tackle most of the time. The advantage of tackling small problems is that it is easier, and not many people knew about these small problems, so you have a chance of making a kill. If you tackle a big problem that has already been worked on by hundreds of people, your chance of hitting the right button compared with predecessors is small.

I also want to say another thing. Compared with the time when I was a graduate student, physics is now more complicated. As a graduate student, if I study five physics fields, for each of them if I learn the fundamentals, I was already, in some sense, at the top of the fields of physics. Physics only had five big areas at that time. If you do well in those five areas, you would feel that you are more or less at the top at that time. Today it is impossible. Physics has already fragmented into an enormous number of subfields. For you that is a tremendous disadvantage, but it is also a tremendous advantage: In every one of these subfields, you might be able to do some very important things. There are many more channels of success waiting for you to try.

Smrithi Keerthivarman: How intuitive or how easy is it to go against what the general scientific community believes in? Because, going back to your non-conservational theory, it was not easily accepted by most people. How much did you believe in it and how easy or difficult was it?

Prof. Yang: We certainly believed, as everyone else, in parity conservation. Thus we did not start with any idea to make a revolution. But we wanted to solve a puzzle, the so called theta-tau puzzle. So we examined *the details of each step* invoked in the arguments that led to the puzzle. Finally we found one weak step: the experiments that were believed to establish parity conservation in weak interactions were not correctly analysed theoretically. That was the key breakthrough.

Next we suggested new more complicated experiments to test parity conservation. One of these more complicated experiments was what Prof. Chien-Shiung Wu chose to do. At that time, she was one of the greatest experts in beta decay. She performed one of these more complicated experiments which took her half a year.

Actually Lee and I had fully expected that she would prove that parity was conserved, because that was what we all had believed. When it showed that that was not true, we all knew she performed a very very important experiment.

Smrithi Keerthivarman: So you did not expect it at all until the very end?

Prof. Yang: We did not expect it. She did not expect it. I think nobody expected it. Most people said it was a waste of time. She should not do it because she was bound just to find conservation of parity. Prof. Wu's great achievement lay in her perception that this was an experiment that was worth doing even though she did not expect any revolutionary results.
Prof. Oh: Prof. Yang, this issue has already crossed 50 years? That should have passed the Nobel Prize 50 Year Secrecy Rule. So the Nobel committee should explain why Prof. Wu did not get the prize.

Prof. Yang: It used to be said that 50 years after the event, a historian of science would be allowed to look at the records of the Nobel Committee. Although it has been more than 50 years after 1957, I believed they have changed the rules. Now it has to be at least 50 years and also has to be when all the people involved have passed away. Do you know how Prof. Wu initialled her experiment? Her experiment was very difficult because it combined two technologies, beta decay and low temperature physics. She also needed to be very precise in her experiment because of the low temperature requirement and the fact that the source of radioactivity is cobalt. The material has to be in the form of a big crystal. Now, how to form a crystal is a specialized field. Prof. Wu did not know how to form big crystals, so she went to the chemistry department and consulted some chemists.

The chemists gave her a big bunch of books on how to form crystals. So she got these books and started reading with her students on how to make big crystals. They tried hundreds of bottles of solutions but did not succeed in getting big crystals. They were very disappointed because without a big crystal they could not do the experiment. But one morning, one of her graduate students came in with a beaker with a big crystal in it. Everybody was extremely happy and said, "How did you make this big crystal?" Upon examining what happened during the previous day, they found that student had taken the beaker home and accidentally placed it near the oven. Thus it is best to put the beaker of liquid in some warm place. So you see, all successful experiments consist of collections of ups and downs.

Ji Dongxu: Research is very taxing and stressful, did you experience any period of unsuccessful research?

Prof. Yang: Of course I had. When I went to Chicago, I was determined to write an experimental PhD thesis because I said to myself: "I have learnt a lot of theory in China but not a lot of experiments, and I knew physics must be based on experiments. So I wanted to write an experimental thesis." At first I wanted to work with Enrico Fermi, but I could not because Argonne National Laboratory where Fermi was based did not allow foreign nationals. So I worked with Professor Allison for 20 months and learnt that I was clumsy. More importantly when we were in the laboratory, there were a lot of problems. Problems like some things did not work and I did not know how to fix them. My fellow graduate students knew how to fix them. So in some aspects, I developed an inferiority complex, but on the other hand, I could solve mathematical problems for my fellow graduate students, so we had good relationships and helped each other. But after 20 months, I knew I was not made for experimental physics. It happened that I had written a theoretic paper and Edward Teller, who was my theoretical advisor, said: "That paper is quite good. You make it a little bit longer and I will accept that as your PhD thesis." After debating with myself for a few days, I decided that he was right. So I abandoned experiments and came back to theory. That experience also taught me that indeed a person should not be so stubborn as to try to do something that he or she is not destined for.

Prof. Oh: Physics is now a very wide discipline, as you said. What area of research do you think will be very active in the next years?

Prof. Yang: I think one very important area which I am sure will become more and more important in the next 20 or 30 years is the application of physics in medicine. I am wearing this hearing aid (referring to his hearing aid). The technology of hearing aid is at least 50 years old, but only recently has it become better. My father was in his 60s when he had hearing difficulties. So we bought for him a pair of hearing aids, but he refused to wear them. He complained that the hearing aid amplified everything and there was too much noise. In the last, maybe, 20 years, hearing aids have improved. Why? Because they now do not just amplify the sound, they make a Fourier transformation and divide the spectrum into bands. Different bands would be given different amplifications. For me, I have become more and more insensitive to high frequency. So they amplified the high frequency bands and do not amplify as much on the low frequency bands. When I first began to wear a hearing aid, there were only six bands but that was about 12 years ago. These are the improved ones (referring to his hearing aids), there are 12 bands. Now they also make 18 bands one. Still I think even if it has improved to 48 bands, it is still not good enough because there are a lot of other problems. For example, how to hear different consonants clearly will require further research. At the present moment, the largest company in the world that makes the most hearing aids is Siemens. The next largest is a small company in Denmark. Why is a small company in a small country like Denmark so successful? It is because there were some acoustic physicists in Denmark. Starting from that, they formed an industry.

Prof. Yang: Fibre optics is another good example. I think many hospitals would have to close their doors if not for fibre optics. It is extremely important. You know why Prof. Kao got the Nobel Prize for fibre optics?

Erickson Tjoa: Prof. Charles K. Kao?

Prof. Phua: Yes, from Chinese University of Hong Kong.

Prof. Yang: Prof. Charles K. Kao was the president of the Chinese University of Hong Kong, and he received the Nobel Prize for his thesis on the ground-breaking achieve¬ments concerning the transmission of light in fibres for optical communication, while working in England (Standard Telecommunication Laboratories). What did he do? At that time, I think it was in the late 60s, people already thought about transmitting optical signals along glass fibres, but it was not considered useful because the signals that are being transmitted along the glass fibres would dramatically decay in short distances. What Kao did was he analysed the cause of this attenuation and theorized that glass itself was not the problem. It was the impurity in the glass tube. Some years later, when glass companies started to make purer glass fibres, they found out that he was correct. The purer the glass fibre is, the lesser attenuation there will be. So that is why he later received the Nobel Prize in 2009. It is another successful story.

Paper IV

C N Yang on Hao Bailing, 2015*

C. N. Yang
Tsinghua University, China

Professor Hao Bailin is one of China's most talented and most versatile theoretical physicists. He has made important contributions to a wide variety of research fields, including biology in which he pioneered a multidimensional method for studying the evolutionary pathways of bacteria. Indeed he calls himself, appreciatively I believe, a guerrilla fighter.

I think I had first met Hao in 1980 when he was one of the members of a team that visited Stony Brook for a few days. [Cf. photograph.] We met again at CERN a few years later and had for the first time the opportunity to sit down to discuss physics. It was then that I learned to admire his breadth and depth of knowledge about contemporary physics. More important I learned to admire his straightforward and principled personality. In 2003 I moved back to Beijing. Regrettably around that time he also moved, to Shanghai. Thus we missed the opportunity to get to know each other more intimately.

At this time of very rapid social transformation in China, Hao stands courageously as a pillar of personal integrity and rectitude.

Brief Biography of C. N. Yang

Born 1st October 1922, the Anhui native graduated from National Southwest Associated University in Kunming, China with a B.Sc. degree in 1942. After earning a master's degree from Qinghua University in 1944, Professor Yang left for the US, pursuing his passion at the University of Chicago. Graduating from Chicago with a doctorate, he began a long and illustrious career as a member of and heading up various university research faculties and institutes. The list of institutions that Professor Yang influenced includes Princeton Institute of Advanced Studies and the State University of New York at Stony Brook (where he held the Einstein chair), among others. Apart from winning the Nobel Prize in Physics 1957 with Tsung-Dao Lee for the "penetrating investigation of the so-called parity laws which has led to important discoveries regarding the elementary particles", Professor Yang has continued to influence the physics and science communities through the Yang-Mills theory, first promulgated in 1954. Other examples of his groundbreaking work include the Yang-Baxter equation and the Wu-Yang Monopole.

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