The Construction of a High-Energy Collider

by Shing-Tung Yau, Yifang Wang, David Gross, Gerard 't Hooft, and Hong-Jian He

Comments on the Construction of a High-Energy Collider in China, and a Reply to the Media's Questions

by Shing-Tung Yau

Recently many news media outlets are paying close attention to the possibility of building a high-energy collider in China. This is a good thing since many scientists from all over the world are excited about this project. Unfortunately, some in the news media are so eager to get attention that they have presented a distorted version of the facts and, in some cases, published fictional accounts.

I have recently come across several such cases. For example, after I refused an interview request by Caixin Weekly, they made up a report that implied they had actually interviewed me. But in fact, the report is the result of their own imagination, mixed in with some rumors found on the Internet, out of which they fashioned a personal attack on me.

Furthermore, many journalists of late keep asking me some naïve questions, encouraging me to speak to Mr. Mengyuan Wang, whom I have never heard of, while also asking me to comment on Wang's article about colliders. These journalists insisted that Mr. Wang is an expert in high-energy physics because he received a PhD from the physics department at Harvard University. That statement really surprised me. I am a professor in both the physics and mathematics departments at Harvard University, but I have never heard of Mr. Wang. (I am, in fact, the only professor appointed by the president of Harvard that can vote in both departments.) After hearing this from journalists, I asked my high-energy-physics colleagues in Harvard's physics department to see if there was anyone who knew Mr. Wang. The result was that nobody had heard of Mr. Wang. After further inquiries, I finally figured out that his advisor was an assistant professor who didn't get a promotion in the department. This explains why senior professors in the department didn't know Mr. Wang. It is said that Mr. Wang no longer wrote interesting scientific papers after his PhD thesis and has been a businessman for the past twenty years. After hearing this, I was surprised by the inability of the Chinese media to find a qualified expert for the interview.

I don't care about Mr. Wang's criticism of me whatsoever. After all, I receive letters from non-academic individuals every week, each claiming to have solved important problems in math or physics, and I am no longer surprised by such outlandish claims. However, those journalists insisted on pursuing this matter and even dragged Prof. Chen-Ning Yang into the debate, which brought the issue to a much higher level – and one that I feel obliged to say something about.

I have personally known Mr. Yang for more than forty years. He has always been the scientist I respect most besides my mentor, Shiing-Shen Chern. Yang's work in the 1950s and 1960s on statistical mechanics and high-energy physics are all admirable – none more influential than his generalization of Weyl's gauge theory to the non-Abelian case. The Standard Model of high-energy physics was constructed by many Western physicists in the 1970s, which may well be the most successful physics theory ever devised in human history, and its construction makes use of the non-Abelian theory.

Over the past fifty years, many important experimental results have come from the world's top high-energy colliders, each of them revealing a fundamental aspect of nature. The ultimate question that humans can ask concerns how the Universe was born. The experimental breakthroughs realized at these incredible machines represent important steps toward answering this question.

The theories underlying these experiments all involve Mr. Yang's theory, and each breakthrough achieved makes his theory more impressive. So it is puzzling that Mr. Yang would be against the collider proposed for China, which would open new frontiers in high-energy physics, creating possibilities that extend far beyond the grasp of a run-of-the-mill businessman like Mengyuan Wang.

I have to doubt whether it is true that C. N. Yang is really against further development of this most important field of research, as journalists have maintained. After all, I have had a long association with Mr. Yang during which time I have never heard that he was against building a new, state-of-the-art collider. So I am skeptical of the journalists' message.

Advancement in science rests on the contributions of many scientists and does not depend on any one individual. Fundamental truths about nature are only accepted after close scrutiny. As Aristotle once said of his teacher: "Plato is dear to me, but dearer still is truth." Such a wise and courageous attitude is needed to press forward in our quest to uncover the deepest truths about the universe. In Western countries, scientists and governments have been making great efforts, willingly and unconditionally, to explore the most profound mysteries of nature. To this end, large sums of money have been invested in basic research that appears to have no obvious practical applications. These investments have paid off over the long run, however, helping to establish the basis of today's Western civilization.

China today is no longer the China of the past. Shouldn't China make a contribution toward answering the ultimate question about the Universe? Should we be satisfied with the minor benefits that may come from further developments in computer games, real estate, and the Internet, while steering clear of bigger, more far-reaching issues? So far as I can tell, no great country in the history of humanity ever operated in such a shortsighted and unambitious fashion.

Let us ask ourselves: Cannot China manage to build a collider, given all of its national wealth and power? Can the peaceful "Rise of China," as advocated by Chinese leaders, not have the broad scope and vision necessary to enhance our understanding of the Universe? Among the scientists who oppose the collider project in China, who among them are real experts in experimental high-energy physics? And does it make sense to ignore the opinions of the most experienced scientific experts in the world?

The importance of the collider project to both the international science community and to China itself was spelled out in a book, *From the Great Wall to the Great Collider*, written by Steve Nadis and myself. I hope that people can evaluate this endeavor with objectivity and rationality, and not be swayed by the spurious views being promulgated by irresponsible members of the press.

29 August 2016

It is Suitable Now for China to Build a Large Collider

by Yifang Wang[1]

Today (September 4th) "The Intellectuals" published Prof. Chen-Ning Yang's article "It's not suitable now for China to construct the large collider". As an experimentalist and the current Director of Institute of High Energy Physics, Chinese Academy of Sciences (IHEP, CAS), I cannot agree with him. Prof. Yang is a respected scientist, but I have even more respect for science and rationality. I apologize in advance if the following discussion would cause any impoliteness.

The first point of Prof. Yang is that the construction of large colliders is a bottomless money sink. During the construction of the Superconducting Super Collider (SSC) in the US, the prices went so high that this project had to be given up halfway, leading to a waste of 3 billion US dollars. The construction of LHC cost 10 billion US dollars. The large collider of China won't be cheaper than 20 billion US dollars, and may become a bottomless money sink.

Concerning this point, there are actually three questions: First, why did SSC fail? Secondly, what's the cost of China's large collider? Thirdly, is our estimation reliable? Is it another bottomless money sink? Let me discuss them one by one, as follows:

1. Why did the SSC fail? Are all large colliders bottomless money sinks?

The SSC of the US failed for multiple reasons, including the deficit in the government budget, the competition of funding with the space station project, the political struggle between the Democratic and the Republican party, the competition between Texas and other states for the hosting of the SSC, mismanagement, mistakes in the budget, soaring cost, and not sufficient international cooperation, etc. A detailed analysis and historic documents can be found in the reference [2, 3]. In fact, the budget overrun was definitely not the main reason for its failure. Rather it

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was quite accidental in special circumstances, mainly due to political reasons.

For the US, the cancellation of the SSC project was a big mistake. Consequently, the high energy physics community of the US lost the chance to find the Higgs boson, lost its foundation and opportunity for further development, and lost its leading position in the international particle physics community. This decision had extremely negative impact to big science projects at the US. It dampened the ambition and courage of an entire generation. The opposition to the SSC in the US at that time shares many common arguments with the criticism of the Chinese large collider project today. In fact, the cancellation of the SSC project didn't lead to any increase of funding to any disciplines. Of course, the construction of the SSC didn't lead to any decrease of funding to any other field either. Many people who opposed the SSC came to regret their opposition to the project.

After that, the Europeans built the Large Hadron Collider (LHC) with great success. Even though there was a small budget overrun, it was not significant, showing that large colliders are not necessarily bottomless money sinks, and can be successful.

The decision making process and political system in China is very different from that of the US, and is actually advantageous for large construction projects. It has less uncertainty. China today has already done a number of things which the US would not or could not do. In the future, more achievements of this kind will happen. The failure of SSC doesn't means that we are not able to build large colliders in China. Of course, we should learn from the experience of the SSC and be better prepared for the project, including better international cooperation, management and budget estimate.

2. What's the cost of this collider?

The large collider we proposed has two phases. The first phase is a Circular Electron Positron Collider (CEPC), which could be constructed during 2022–2030. Assuming a tunnel circumference of 100 km, the construction cost is roughly 40 billion CNY (not including the cost of land, and the supporting infrastructure such as road, water, internet, power supplies, etc.). If we succeed in the CEPC project and there are hints of physics beyond the SM, and if the novel technology of super conducting materials will have matured so that their cost is reduced to an acceptable level (say, $\sim$20 CNY/kA*m), we can start the second phase, the Super Proton Proton Collider (SPPC). The cost of this phase could be controlled to within 100 billion CNY, and the construction could happen in 2040–2050. As an international project, we expect 30% of the total cost to be covered by international partners. **Therefore, the Chinese government needs to invest 30 billion CNY (3 billion per year for 10 years) for the first phase and 70 billion CNY (7 billion/year) for the second phase (without taking into account inflation).** The fact that there is a possible second phase gives this proposed project, CEPC-SPPC, a much longer lifetime. It could stimulate the development of corresponding technologies such as high-Tc superconducting materials. These two phases are highly complementary to each other, in both scientific goals and technology impacts. At this stage, the purpose to discuss SPPC is to make sure that our design, such as tunnel length and cross section, does not limit the option for the future upgrade.

3. Is this estimate reliable? Would it repeat the failure of SSC?

In the past half a century, many accelerators have been successfully constructed all over the world (LEP, LHC, PEPII, KEKB/SuperKEKB, et al.). There were also some not-so-successful accelerator projects (ISABELLE, SSC, et al.). In this list, all failed projects are proton colliders, with no failed example of electron-positron colliders. The reason is that the proton collider is technologically much more complicated; it's usually very hard to predict the advancement of the super-conducting technology and it is not easy to properly balance between the cost, technology, and the specification. If the spec is too high, it would result in cost overrun. On the other hand, the choice of low spec would appear to be too conservative.

There had been many successful examples of large construction projects in China. Since its founding 40 years ago, IHEP has carried out many big scientific projects with cost higher than 100 M CNY, including the Beijing Electron Positron Collider (BEPC), the Daya Bay Neutrino experiment, the China Spallation Neutron Sources and the ADS Injector. All these projects have been completed on time, and up to the spec. The actual cost has never exceeded the budget by more than 5%. We have a mature system and deep experience on budget estimate, construction and project management.

In fact, we employed two methods to estimate the cost of CEPC: (1) decomposing the project into a list of equipment, components and sub-systems; and (2) making analogies and comparing with similar projects around the world. Both at the sub-system level and for the total cost, these cost estimates are consistent within 20%. In fact, once we finished the Preliminary Conceptual Design Report (Pre-CDR) [1] for the CEPC (the first phase), we generated a list of more than 1000 items, based on which the cost estimate was done, and reviewed by domestic and international experts. If Prof. Yang has any doubt about this cost estimate, another review can be organized.

For the SPPC, we only used the comparison and analogy method, since it is not the main objective now, but only a future possibility. It is meaningless to talk about its construction cost now. We have to wait until the technology is mature enough. How could it become a bottomless money sink?

The second point of Prof. Yang is that, China is still a developing country, and there are still livelihood issues to be solved. A Large collider is not that urgent and should not be considered now.

For any country, especially one with the size of China, it is essential to balance the short-term needs and the long-term plans. The livelihood is certainly an essential issue and it is in fact the main part of the government budget. Meanwhile, we also need to invest in long-term needs, with a reasonable fraction of GDP on basic science, in order to ensure the potential of long term development, and stimulate becoming a leader of the world. A terrible example is the Qing Dynasty. At that time, China had the largest GDP of the world, capable of buying anything from abroad. However, China was not developed in science. Though China bought lots of advanced weapons from abroad, it was still defeated miserably and its livelihood fell all the way down to the bottom of the world.

For centuries, the studies of microscopic structure of matter, from molecules, atoms to nucleus and elementary particles, led the development of science to a large extent. Nowadays, such research takes the form of particle physics, which aims to reveal the fundamental building blocks of matter and their interactions. Particle physics adopted and stimulated the development of technologies such as accelerators, detectors, cryogenics, superconducting materials and cavities, micro wave equipment, vacuum systems, power supplies, precision machinery, automation, computing and networks. Because of its huge impact to science and the boost to the technology development, high energy physics is a very significant and unique field. **By constructing the CEPC, China will play a leading role in this important flagship field. In addition, Chinese industries could manufacture related high-technology products and lead the world. Meanwhile, the CEPC will also attract, and train thousands of top scientists and engineers, forming a science and technology center. The CEPC is indeed an urgently priority for China.**

In fact, the impression of China to the world is rich and at same time too practical. A big country without contribution to the civilization cannot have big impact and influence in the world. This will in turn affect China's interests. On the other hand, as a fraction of GDP, the cost of the large collider (CEPC and even SPPC) didn't exceed that of BEPC, and is lower than that of other constructed and planned facilities (LEP, LHC, SSC and ILC) in the world.

The CEPC provides a unique opportunity for China to assume the leadership in the field of high energy physics in the world. First, the Higgs boson discovered at LHC has a mass that is perfectly suited to allow a circular electron positron collider to be a Higgs factory. Meanwhile, this collider could be upgraded to a proton collider, providing a science program that could last for 50 years. Secondly, we have a time window of roughly 20 years with relatively mild international competition, since Europe, the US and Japan are all occupied by other particle physics projects. Thirdly, with the BEPC, an electron-positron collider, we accumulated sufficient experience and a well-trained team which are just right for CEPC. This window of opportunity will last for only about 10 years. It is hard to predict when would be the next time if we miss it. Meanwhile, China has excellent experiences in large underground projects, and the economy is still in rapid growth. During this restructuring period, there are needs to invest on science. Therefore, CEPC is a well suited project for China now.

The third point of Prof. Yang is that the construction of CEPC will largely squeeze the funding for other disciplines of basic science.

Currently in China, the funding for basic science is roughly 5% of the total R&D spending, while that fraction for developed countries in the world is typically 15%. As a large developing country moving towards a developed one, I think China should gradually increase this ratio to 10% and eventually to 15%. In terms of numbers, there is still a big room for the funding of basic science to increase (roughly 100 B CNY per year). Therefore, construction of the CEPC would not crowd out the funding for other disciplines.

On the other hand, how should we spend the funding from such an increase? It is well known that a significant percentage of our funding is spent on purchasing equipment, especially from abroad. If we suddenly increase the funding uniformly to all disciplines, or toward some disciplines that strongly rely on the international apparatus, it is very likely that such an increase will also boost the GDP of foreign countries. To the contrary, if we invest on the large accelerator for 10 years with a total budget of 30B CNY, more than 90% of the money will be spent in China. Such a spending will stimulate our companies to have technology progresses and more market sharing, train thousands of scientists and engineers that could design and manufacture the needed apparatus, and help the development of other disciplines. In fact, such an investment will not change significantly the balance among different fields. In the long run, it will rebalance the funding distribution to a level comparable to the norm in the world (currently particle physics and nuclear physics are significant low in China relative to the rest of the world). The Chinese government is now calling for proposals to host large international scientific projects. CEPC is an excellent candidate, and not in conflict with other disciplines of basic science.

The fourth point of Prof. Yang is that SUSY particles and Quantum Gravity have not yet been discovered, and it is hopeless for the CEPC to discover such hypothetical particles.

The science goal of CEPC is not what Prof. Yang described. In fact, we described clearly the physics motivation in the "Preliminary Conceptual Design Report of CEPC-SPPC" [1] which I delivered to Prof. Yang in person. In short, the Standard Model (SM) of the particle physics is only an effective theory at low energies. We aim at discovering the fundamental physics principles that underlying the SM. Although there are some experimental evidences for new physics beyond the SM, we still need more data to guide the direction for the future. Currently, most of the problems of the SM are related to the Higgs boson. Therefore, clues of new physics at deeper level shall come from the Higgs boson. CEPC can measure the Higgs boson to an accuracy of 1% level, which is a factor of 10 better than that of the LHC. Such a precision would allow us to determine the properties of the Higgs, and to check its consistency with the prediction of the SM. Meanwhile, CEPC may measure for the first time the self-coupling of the Higgs boson (indirectly) to determine the type of the electroweak phase transition, which is essential for the understanding of the early evolution of the Universe. In short, no matter if LHC discovers new physics or not, CEPC is badly needed and cannot be skipped in the advance of particle physics.

If there is any deviation from the SM observed at the CEPC, for example new coupling and/or new partners of Higgs boson, substructure of the Higgs boson, we could upgrade CEPC to SPPC to directly look for the cause of such deviation, which might be SUSY particles or any other new particles. For experimentalists, we care about theoretical predictions, but we never rely on them. It is too assertive to claim what particles can or cannot be discovered at the future collider. Most people from the international high energy physics community do not think that way either.

The fifth point of Prof. Yang is that major achievement of particle physics in the last 70 years did not offer direct benefit to human life, and it won't have any in the future.

For 70 years, high energy physics had a lot of achievements. It developed lots of technologies that are closely related to people's daily life. Without particle physics, there will be no synchrotron light source (coming from electron positron circular collider), free electron laser (coming from electron positron linear collider) and spallation neutron source, which are essential tools for the study of biology, geology, environment, material science, and condensed matter physics. Without particle physics, many medical apparatus such as MRI, PET and radiotherapy would not exist, would not be so advanced, or be invented much later. Many people would have a shorter life span, or their life quality would be severely reduced. Without particle physics, there would be no (or much delayed) touching screen, and therefore no smartphones; there will be no World-Wide-Web (WWW) and we would not be able to surf the web. There would be no e-commerce of course. In fact, the WWW has profoundly changed the world, and its economic outcome has been much more than all the investment in high energy physics before that.

In terms of the CEPC, how would it affect our daily life? With the 30-billion dollar CNY investment (3 billion per year, for 10 years beginning in 2022), we could promote the following technologies in our domestic companies to a world leading position:

a) High Quality Super Conducting Cavity (used in almost all the accelerators)
b) High Efficiency, High power microwave power source(used in radar, broadcasting, communication and accelerators)
c) Large scale cryogenic systems (used in other fundamental researches, rocket engine, medical apparatus)
d) High speed, radiation-hard silicon detectors, electronics and ASICs

In the meantime, we can also lead the world in technologies such as precision machinery, microwave, vacuum, automation, data acquisition and processing, computing and networks. We can train thousands of top-level physicists and engineers, as well as attract thousands top-level scientists and engineers worldwide to form an international center of science. If SPPC is going forward, 7 Billion CNY will be investigated each year starting from 2040, it can promote the application of our technologies of high-Tc superconducting material and superconducting magnets which will be leading the world. The volume of this industry would be much larger than 70 Billion CNY(cost of SPPC). On top of that, there might be unexpected new discoveries and new technologies. The direct application of high energy physics discoveries cannot be predicted now. Indeed there should be no need to ask this question since the importance of the study of the structure of matter and elementary particles cannot be emphasized more. The Chinese may have laughed at the Greeks and the Europeans for their "useless" studies on atoms, gravity, quantum mechanics and the Higgs boson, but there is always a price to pay (which has been paid).

The sixth point of Prof. Yang is that the Institute of High Energy Physics (IHEP) did not have great achievements in the last 30 years. Over 90% of the works for the large collider will be dominated by non-Chinese and the possible Nobel laureates would not be Chinese.

It has been more than 40 years since the establishment of the IHEP. Benefiting from the construction of the Beijing Electron Positron Collider (BEPC), IHEP had been developed significantly with focus on particle physics, astrophysics, multi-discipline research and applications. For particle physics, a major investment to facility at IHEP is the Beijing Electron-Positron Collider (240 M CNY, 1984), its upgrade (640 M CNY, 2004) and the Daya Bay neutrino experiments (170 M CNY, 2007). The total is about 1 Billion CNY. Comparing to other disciplines, for example biology, condensed matter physics and astrophysics as mentioned by Prof. Yang, the funding level of particle physics is not higher (in total or per person). Meanwhile, the output of particle physics, partially measured by national and international awards and honors, is no less than other disciplines. Though such an investment is orders of magnitude lower than that of leading countries, the scientific output is somehow comparable. At least IHEP is one of the four leading particle physics laboratories in the world (CERN, Fermilab, KEK, IHEP).

In the year of 2012, Chinese scientists first independently proposed the CEPC-SPPC project. The international community of particle physics responded strongly to this proposal and gave strong support. We launched the conceptual design afterwards and completed mainly by ourselves, with some international participation, the "Preliminary Conceptual Design Report" (pre CDR) [1] in 2015. Hence we believe that in the future, more than 70% of the works for the large collider will be completed by Chinese, at least the same as the fraction of the Chinese investment. If Prof. Yang still has no confidence, please consult with leaders of major particle physics labs in the world.

In fact, IHEP has over 30 years' experiences with the electron positron collider. CEPC is proposed after much deliberation. Those who participated the design and construction of BEPC in 80's agree that it was much more difficult to construct the BEPC in 80's than to construct the CEPC today. We believe that the younger generation today would do even better and we have the confidence, capacity and courage to accomplish the CEPC by ourselves. On the other hand, we should encourage international participations for this project.

Concerning to the second phase of the hadron collider (SPPC), we admit that we don't have much experience and need more effort. However, we still have 20 more years, and should be able meet the minimum target of "accomplish works proportional to the funding contribution". According to our record of progress in the last 30 years, this goal should be achievable.

About the possibility of Nobel Prize to Chinese, I think it is not predictable. It is not the motivation of the investment to basic science by our country, nor that of the individuals who do the research. We ultimately try to understand and reveal fundamental principles of the nature. The Higgs boson is discovered at CERN and its discovery granted the Nobel Prize to Mr. Higgs from the University of Edinburg. We hope that China can host a research institute with the similar scale, scientific output and technology capabilities like CERN. It is not important whether we have our University of Edinburg and Mr. Higgs to win a Nobel Prize.

The seventh point of Prof. Yang is that the future of particle physics lies in the direction of "new concept of acceleration" and "theory of geometry", not in colliders.

The "new concept of acceleration" (such as plasma acceleration) is indeed promising for the future accelerators. Given enough time, maybe in several decades, such technologies might be applicable to fixed target experiments or other experiments that do not require high quality beams. For high energy colliders, both beam quality and energy efficiency of these novel technologies still have a long way to go. In the meantime, high energy physics should not halt to wait for the maturity of these technologies. And about the "theory of geometry" or "string theory", they are too far away from being able to be tested by experiments, it is not an issue for us (experimental particle physicists) to consider now.

People always have different opinions about the future direction of particle physics. China does not have Nobel Laureates in physics now, but there are many in the world. Obviously Prof. Yang holds a view different from the majority of them, not only now, but also in the past. Prof. Yang has been pessimistic about particle physics since 1960s, and missed the major discoveries of the Standard Model of particle physics. In 1970s, Prof. Yang opposed the construction of high-energy accelerators in China [4]. Fortunately, Mr. Deng Xiaoping took the suggestions of Prof. T. D. Lee and other prominent scientists. As a result, it became possible to have today's IHEP, BEPC, Daya Bay, and their great results, as well as large science facilities such as synchrotron light sources and the spallation neutron source serving the science community of the whole country. Facing the future, we should listen more to the young scientists working on the front line of the research, who will make our science flourish and grow into a leading position in the world.

5 September 2016

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Why China Should Build the Great Collider: A Response to C. N. Yang

by David Gross

Professor C. N. Yang is one of the great figures in physics of the last century. However, I disagree with all the objections to building the "Great Collider" in China that he has recently voiced.

Before addressing Professor Yang's points, it is perhaps worth explaining why I feel compelled to voice my opinions on this matter.

First and foremost, I am very excited by the scientific potential of the Chinese collider project, and as a friend of Chinese science and a foreign member of the Chinese Academy of Science I am very excited about the many benefits that this project will produce for China.

Particle physics has entered a new epoch in the 21st century, driven by deep paradoxes that strike at the foundations of our understanding of the 20th century revolutions of dynamical space-time and quantum mechanics. Some of the deepest of these mysteries revolve around the Higgs boson, a particle unlike any we have discovered before. The answer to a very basic question about the Higgs – is it point-like, or does it have substructure? – will force fundamental physics down radically different paths in the coming decades. The LHC will not answer this question; a new particle accelerator is needed to decisively settle the issue. This is precisely what the Chinese collider project will do.

I am also moved to comment for a second reason, as an American physicist who witnessed the cancellation of the Superconducting Super Collider (SSC) project in the early 1990s. The most prominent detractors of the SSC put forward many of the same arguments made today by Professor Yang. But the cancellation of the SSC is now almost universally regarded, by supporters and detractors of the project alike, as a disaster for fundamental physics in the US, one with lasting negative effects that have proven difficult to recover from. The US had spent decades as the unquestioned world leader in particle physics, yet quickly ceded this mantle to Europe, and with it, a critical capacity to "think big" and pull off major, ambitious, long-term projects.

Today, China has a golden opportunity not only to do something great for physics, but also to catapult itself to world leadership in fundamental physics at an especially crucial juncture in the history of the subject. It would be a tragedy if the same calamitous errors made by the US in cancelling the SSC, fueled by similarly faulty arguments, were to derail the Chinese collider effort.

Before addressing Professor Yang's remarks, it is important to clarify the collider projects under consideration. The machine currently being proposed by Chinese physicists is the "CEPC", a large electron-positron collider, between 50–100 km in circumference. The CEPC will function as a "Higgs factory" and settle outstanding questions about this deeply mysterious particle. This is the only machine under discussion for the next two decades.

Further into the future, the same 50 to 100 km tunnel used for the CEPC could be used for a second machine, the "SPPC", that would collide protons at energies over 7 times higher than the LHC, securing the experimental future of fundamental particle physics on the 50-year timescale. Of course, the prospect of the SPPC following the CEPC adds significantly to the excitement and scientific potential of the CEPC project, but any concrete decisions about proceeding to the SPPC cannot be responsibly made till over a decade from now.

With these preliminaries aside, let us examine each of Professor Yang's criticisms in turn.

1. Are accelerators a bottomless money sink?

No! The example of the SSC is way out of date and much has changed since. Indeed, the cancellation of the SSC forced the international particle physics community to learn some hard lessons, and subsequently every major accelerator project completed in the past twenty years, chief amongst them the Large Hadron Collider, has been completed essentially on time and on budget. Chinese particle physicists have made a detailed cost estimate for the CEPC project, which is on the order of \$6 billion, not \$20 billion. And they have an impressive record over the past decades, from BEPC to Daya Bay to the neutron spallation project.

Comparing to the cost of the LHC is not relevant, since electron-positron colliders like the CEPC are well-known to be far cheaper than proton-proton colliders like the LHC. As we have already stressed, beyond this it is not possible to make responsible estimates for the cost of the SPPC, which depend on the development of various new technologies in the coming ten to twenty years.

2. Can China, a developing country, afford to build the Collider?

Yes! Professor Yang argues that while China is wealthy in absolute terms, its low GDP per capita is not yet comparable to those of wealthy nations. But the size of the GDP per capita would only be relevant if China were planning to build a number of colliders in proportion to its population, whereas only a single facility is being discussed. Indeed, using the same logic one could argue that the cost of the collider per capita is significantly smaller in China than anywhere else in world!

China's ambitions on the world stage are high, more in line with its GDP than its GDP per capita. The GDP per capita has not prevented China from aiming to go to the moon, or completing outstanding engineering projects like the Three Gorges Dam. Pursuing bold initiatives in the basic sciences seems to be perfectly in line with these big ambitions.

But more importantly, I believe, and history has proved, that it is precisely such long-term investments at the frontiers of science that have stimulated the technological advances that lift developing countries to economic superpowers.

3. Will funding for the CEPC hinder the development of other parts of Chinese science?

No! While I have no precise insight into how Chinese funding of the collider project will work, the scale of funds will obviously require new money to flow into the support of basic science, in accord with the stated goals of the Chinese government to rapidly increase the proportion of GDP spent on basic research. Other fields should not have to pay for the collider.

Furthermore, the SSC saga taught us that thinking in terms of a "zero-sum-game" for science funding is a losing proposition; other areas in physics did not get anticipated big extra funds after the SSC was cancelled. Instead, the overall ambition of the US to pursue big scientific goals diminished palpably, to the detriment of all.

The CEPC will stimulate the growth of science in another way; by becoming a magnet for international talent in physics and engineering, thus helping to create and sustain an intellectual infrastructure that will spur the development of many other technical fields in China.

4. Is the purpose of the Collider to discover Supersymmetry, an unproven hypothesis?

No! Professor Yang argues that the chief scientific purpose of these machines is to discover supersymmetry, a new symmetry of space-time that is, at the moment, a hypothesis with no experimental support.

Alas, it appears that Professor Yang has not read any of the scientific documents that have discussed the physics case for the CEPC/SPPC at some length. The central physics case for both the CEPC and the SPPC have little to do with speculations about supersymmetry and much to do with deeply understanding the mysteries of a particle we know exists – the Higgs. The Higgs is the first seemingly elementary particle of "spin zero" we have ever seen, and is associated with deep theoretical mysteries – in fact these mysteries are made deeper and more pressing by the absence of something like supersymmetry at the LHC.

The CEPC will put the Higgs under a powerful microscope, and probe its size to resolution 10 to 30 times better than the LHC. The CEPC has a rich and detailed experimental program that will either reveal substructure for the Higgs, or allow us to conclusively decide that the Higgs is an elementary particle on the same footing as quarks and leptons. The guaranteed physics of the SPPC is similarly centered on the Higgs: it will determine whether the Higgs looks point-like to itself. It will do this by establishing the existence of the most fundamental interactions elementary particles can have, where three identical particles meet at a common point in space-time. We have never seen this most basic of all possible interactions in Nature before, and the LHC will not be able to conclusively establish its presence. The SPPC will not only discover this self-interaction, but will measure it to an accuracy of a few percent!

Of course the SPPC will also explore much higher energies, and will have the power to produce new particles that are nearly ten times heavier than can be produced at the LHC. It will certainly continue the search for supersymmetry (amongst other things), precisely because supersymmetry is currently a hypothesis. Indeed, all the great ideas of physics, including the Yang–Mills idea of local gauge theories, as well the proposal of Quantum Chromodynamics (the theory of the nuclear force), were "just a hypothesis" until their predictions were tested by experiment.

But to repeat: the guaranteed physics of these machines is centered on revealing the deeper nature of the Higgs and discovering new interactions associated with it.

5. Has high-energy physics produced any "tangible benefits" to society?

Yes! Even taking an extremely narrow view of this question, the technologies directly springing from particle physics have spawned huge industries that generate revenues far exceeding the magnitude of the investment in basic science. The multi-billion dollar accelerator industry, operating thousands of small-scale particle accelerators around the world ranging from light sources, to medical accelerators for cancer-fighting radiation therapies, owes its existence to particle physics. And the need for powerful magnets at proton colliders necessitated the development of superconducting magnet technology, itself a billion dollar industry, which are the critical component for MRI machines, a five billion dollar industry.

But of course, *much* more importantly, scientific research at the forefronts of knowledge is most powerfully driven by our fundamental curiosity to understand and master how Nature works. Time and again, this mastery has led to revolutionary technological developments that have transformed our lives. Often these have arisen as "spin-offs", not directly associated with the central thrust of the scientific questions, but arising inevitably in tackling and solving hard scientific problems. As one famous example, without the laws of quantum mechanics, none of the modern electronics industry would be possible; one can justifiably say that the understanding of quantum mechanics is responsible for 2/3 of the world's GDP. Another famous example is the invention of the World-Wide-Web at CERN, which was developed to cope with the challenge of experimental particle physicists needing to share vast quantities of information with each other.

Why has the pursuit of science for its own sake had such a remarkable track record in generating transformative new technology? The reason must be that Nature poses deeper and more challenging questions than humans can do, and the struggle to understand Nature forces us to invent better and deeper ideas than we would if left to our devices.

6. The Chinese particle physics community is not strong enough to undertake this project.

No! I strongly disagree with Professor Yang's assessment of the strength of the Chinese particle physics community. High-energy physics in China has a rich history going back to the construction of the Beijing Electron-Positron Collider (BEPC) in the 1980s. The BEPC put China on the world map in particle physics, and the ensuing decades have seen a continual rise in its strength.

A spectacular recent example was the Daya Bay experiment that beat many groups around the world who were chasing the most elusive of all neutrino-mixing phenomenon, making a beautiful and incisive measurement. The international community, through a number of prestigious prizes, has already recognized this achievement. Yifang Wang, who lead the Daya Bay effort, wants to build on this success by pursuing the much more ambitious goal of the CEPC/SPPC, a program that would immediately make China the world leader in high energy physics.

Professor Yang also worries that the project would be intellectually dominated by foreigners and that Chinese physicists won't get credit for its discoveries. I am surprised by his lack of confidence in the potential and brilliance of Chinese physicists! Of course the Chinese particle physics community will be stimulated to grow by these projects, tapping an ocean of talent and drawing many brilliant young minds into physics. Indeed, the engagement of this huge new pool of Chinese talent is one of the greatest contributions this project will make to physics itself! In 10 to 20 years, there is no doubt that Chinese physicists will be a major intellectual force leading the CEPC effort.

Finally, will Chinese physicists win a Nobel Prize for any discoveries made by these machines? Probably, maybe, who knows? And who cares! Grand scientific projects on this scale, now more than ever, transcend prizes and the pursuit of personal recognition and glory. What is certain is that with the CEPC, China will become the world center of activity in fundamental physics for the next twenty years, and extending to fifty years if the SPPC is built.

7. There are other, cheaper avenues to pursue in fundamental physics.

No! On the experimental side, Professor Yang suggests developing novel accelerator technologies. This is certainly an important direction, and has been developing for a number of decades, with concrete ideas currently on the table for generating much larger particle accelerations, for instance using laser technology. But none of these methods can accelerate particles coherently enough; to produce the large number of collisions needed to study high-energy physics. Ways around these difficulties may be found, but it is impossible to predict the timescale for progress, which could well be several decades.

Amusingly, a similar argument was made by prominent opponents of the SSC, condensed-matter physicists who argued that the SSC should be delayed till high-temperature superconductors were developed to greatly reduce the cost of the magnets needed for the machine. Nearly three decades later we are still waiting for these practical high-temperature superconductors to materialize. In the meanwhile, the use of established superconducting magnets in particle colliders has continued to lead to major discoveries, from the top quark to the Higgs particle.

Professor Yang also suggests further theoretical investigations into beautiful geometric structures in physics. Obviously, as a theoretical physicist, I strongly believe in the power of theoretical physics to generate deep new hypotheses that might propel our understanding of the laws of Nature. The exploration of geometric structures is one of many avenues of this sort that have been and continue to be actively explored by theorists. But physics is most fundamentally an experimental science, and experiments have always played a critical role in the discovery of our deepest theories. The story of the Standard Model of particle physics perfectly illustrates this point. The leap from the profound classical Yang–Mills generalization of Maxwell's equations to the powerful quantum theory that actually describes Nature required major new theoretical ideas. And the development of these crucial concepts was largely forced on theorists by a wide array of surprising experimental results.

The need for new experiments in fundamental physics is just as pressing today as it has always been. Indeed if anything, especially when confronting the mysteries associated with the Higgs particle, the lessons of the LHC have left us in a greater state of theoretical confusion than we have seen in decades. What we desperately need are incisive new inputs from experiment, of exactly the sort we will get from the CEPC.

23 September 2016

Interview: Nobel Laureate Gerard 't Hooft Discusses High-Energy Colliders

by Hong-Jian He

Professor Gerard 't Hooft is a renowned theoretical physicist at Utrecht University, the Netherlands. He is among the founders of the standard model of particle physics, and was awarded Nobel Prize in Physics "for elucidating the quantum structure of electroweak interactions" (together with Martinus Veltman) in 1999. He has also received numerous other prestigious prizes and awards, including Heineman Prize of American Physical Society (1979), Wolf Prize (1981), Pius XI Medal (1983), Lorentz Medal (1986), Spinoza Prize (1995), Franklin Medal (1995), Gian Carlo Wick Commemorative Medal (1997), HEP Prize of European Physical Society (1999), Ettore Majorana Prize (2011), Lomonosov Gold Medal (2011), and 1st Prize of Gravity Research Foundation (2015). He is the author of many popular science books, including In Search of the Ultimate Building Blocks, *and, more recently*, Playing with Planets *and* Time in Powers of Ten.

Question: Professor 't Hooft, it is our great pleasure to have this interview with you. I newly read your very thoughtful article "Imagining the Future, or How the Standard Model May Survive the Attacks" [1]. In particular, you discussed new thinking about the Higgs boson and hierarchy problem. You also commented on the possible hint from the LHC. The LHC Run-2 has been performing well to collide proton beams at 13 TeV energy, and has collected about 10% of the planned full Run-2 data. Even though no new physics is announced at the ICHEP conference in this summer, would you like to comment on your expectation of possible new findings at the on-going LHC?

Answer: In one way, LHC did what was expected: it found the Higgs particle, often regarded as the last missing link in the Standard Model, but then it did something unexpected as well: it showed that there seem to be no other particles with such properties, while most theoreticians did expect them, and so there was a surprise after all. Then, many of us expected the Standard Model soon to require modifications in the form of new particles. We had several kinds of theories for that, of which the supersymmetry theory was the most advanced and detailed of all. To the contrary, there seem to be no new particles at all.

Will this be the new world at the TeV scale? We did not expect that. LHC is like the Michelson-Morley experiment, which, by giving no result at all, led to Eistein's relativity theory. Now, I am considering that possibility seriously again: new theories that should explain the non-existence of heavy particles. I hope that this will turn out to be wrong again, since new particles will be giving us much more information, information that may reveal new principles of nature.

Question: Since you mentioned [1] that the Higgs boson (125 GeV) is an important clue and given the fact that the LHC with pp collisions could not measure the Higgs boson precisely, would you feel crucial to build up an e^+e^- Higgs Factory such as the CEPC [2]? You visited China many times before, and on February 23, 2014, you joined the Panel Discussion Meeting on "After the Higgs Boson Discovery: Where is Fundamental Physics Going", held at Tsinghua University, Beijing. What is your viewpoints on this subject now?

Answer: My viewpoints have not changed much. The value of 125 GeV is special because it is close to what one could have expected from theories based on conformal invariance, a theory that might one day explain to us the absence of heavy fundamental particles. If they are indeed absent, we need other clues to find the truth, and one of these clues could be obtained from precision physics. An e^+e^- Higgs factory would be quite suitable for obtaining precision data that would be more difficult to produce in other machines.

Question: Regarding the lessons of Superconducting Super Collider (SSC) in USA, perhaps, you may have seen an article "The Crisis of Big Science" [3] by Steven Weinberg in 2012? The cancellation of SSC by US congress in 1993 was a great loss for the high energy physics (HEP) community in USA and worldwide; it made vital negative impacts on American HEP in particular and in its whole fundamental science in general. Would you like to share your views with the public regarding the lessons of SSC and LHC?

Answer: I do not quite share Weinberg's interpretation of recent history of our science. His rather gloomy mood on how big science failed applies to some unfortunate events such as the cancellation of the American Superconducting Super Collider, which has turned out to be too large and too costly to be operated by a single nation. However, many other big projects were extremely successful. LIGO has spectacular successes, various space probes and telescopes found lots of exciting things in the universe, such as gigantic black holes colliding at cosmic distances, and less far away asteroids, dwarf planets, comets, and thousands of exoplanets. Of course I see the LHC as a great example of how big science can still be successful, and clearly nobody can be blamed for the nonexistence of particles at the TeV scale. We still do not understand why this should be, so we strongly applaud initiatives for the next, greater machine.

Question: Perhaps, you already heard about the current Chinese plan of the "Great Collider" project [2], whose first phase is called CEPC, an electron-positron collider of energy 250 GeV, running in a circular tunnel of circumference about 100 km long. It has a potential second phase for a proton-proton collider with energy up to 100 TeV. Many colleagues worldwide think that this is a truly promising direction for the next step forward in HEP [4]. – Would you like to share your views on the CEPC Project with the Chinese community?

Answer: We do have to live with the fact that science, no matter how big, evolves and its focus will change along with this evolution. If large particle accelerators and other large projects such as ITER will eventually not be further pursued, then this must be for sound scientific reasons. Perhaps we will find other ways to find answers to our questions. But today I do think we are not ready yet to give up hopes that higher energy machines will lead to important insights. It's far too early to abandon that direction, but we do have to be united in our searches. The SSC might have been too ambitious at its time, and it might be too preposterous for us to ask China to succeed where the USA failed. But I would actually be pleased if China and Europe went into a friendly competition for building and operating the most powerful scientific instrument in the world – in that case, we scientists would all prosper from it. On the other hand, perhaps CERN's present success is telling us that international collaboration, safeguarded by very strict regulations, is the way to go.

Question: You probably have heard the on-going public debate in the Chinese community on whether this Collider should be built in China at all [5–7]. This debate was provoked by the Chinese-American theoretical physicist C. N. Yang in this fall [6], who has been strongly against any collider project in China, including the current CEPC-SPPC project led by IHEP director Yifang Wang. It's clear that Yang's major objection is that this collider would cost too much for China, and a misconception of him was to stress the cost of the potential second phase SPPC. (As Yifang Wang showed in his refutation [7], the IHEP team estimated the CEPC cost to be about 6 billion US dollars invested over 10 years and its 25% will come from international collaboration. The SPPC would be built during 2040s if the required technologies become mature by then. As anyone may recall, the funds of the LEP and LHC at CERN were approved separately and in sequence.) – Would you like to share your opinion with the Chinese public?

Answer: It is important to have this discussion in China. I am sure that Prof. Yang understands China's domestic and foreign political attitudes and problems, as well as its enormous potential as a world power, so he should be listened to. Yet I don't quite follow his arguments. In planning the SSC, I suspect the scientists in the USA miscalculated the support they would receive from politicians, congress, and fellow scientists, at home as well as abroad. Maybe it was just a tiny miscalculation, but it was enough to topple the project. This does not have to mean that China will make the same mistakes. Instead, the Chinese should carefully study what went wrong with the SSC, and ensure a sufficiently stable political and financial basis for the realization of its ambitious plans. Then decide whether the plans can be realised. As for their benefit for humanity in general and China in particular, we should indeed not make too grand promises in that a giant new accelerator will bring many elementary breakthroughs, let alone new applications of big science that will boost China's prosperity. That is not the main justification of these enterprises. What should be expected is that this accelerator, together with a number of other big science projects, will lead to joint investigations all over the world of humanity's basic questions. Chinese scientists will take part in these discussions, bringing in their own observations and results. China will be part of a scientific intelligentsia discussing not only basic questions in physics, but in all sciences and problems faced by humanity.

Will it be worth-while to spend such amounts of money on a project whose purposes are obscure to a big majority of the population? This, the Chinese scientists and politicians must decide for themselves. I should warn the scientists in particular that, in my experience, this isn't a zero-sum game. Money saved by cancelling this machine, will not be used for other branches of science, but most likely disappear into completely different activities, which you may or you may not agree about. Therefore, in my humble opinion all scientists should be in favor of reserving money for projects like this, just because it is money to be spent on fundamental science. If indeed China decides to go into this direction, other, totally different big science projects might follow.

I presume Prof. Yang observed that, while the LHC was built in a region that already had all the necessary infrastructure present, which will certainly have suppressed its costs, the new Chinese machine must be built from scratch. This will make it cost more, but then, such money is well-spent. A new city may arise, where scientists from all over the world pay frequent visits and discuss the world's problems. If China could still be looked upon as a developing country now, it won't be that anymore.

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[6] C. N. Yang, "China should not build a super-collider now", September 4, 2016. For a Chinese-language translation, see http://news.sina.com.cn/pl/2016-09-04/doc-ifxvqctu6167027.shtml.

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23 November 2016

Professor Stephen Hawking on the Future of Particle Physics and a Chinese Great Collider

Stephen W. Hawking is probably the only living scientist today who needs no introduction to the public. He has contributed the following statement by the invitation of the editors.

Particle physics is definitely not a dying field. It is however an entirely different enterprise than it was in 1980. Since then, the standard model looks to be essentially confirmed and this may give the impression that the field is complete. However, that is far from being true. There are phenomena that are just not included in the standard model. Some are CP violation, neutrino oscillations, dark matter. In theory, the problems are immense: how to include gravity, the recently discovered dualities of quantum field theories, quark confinement, dark energy, black holes, early-universe cosmology. It is a different world but one that offers huge challenges to ambitious young people interested in how our Universe works. China has an incredible opportunity to become the world leader here – don't waste it. A good example is to build the Great Collider that can lead high energy physics for the next fifty years.[2]

24 November 2016

Edmond L. Berger on the Chinese Proposal of CEPC/SPPC Colliders

by Edmond L. Berger

Edmond L. Berger obtained his Bachelor of Science degree at MIT and PhD degree at Princeton University. He is a Senior Physicist (since 1976) and Distinguished Fellow (since 1995) at Argonne National Laboratory, USA. He is an elected Fellow of American Physical Society since 1975. He is a distinguished theorist in collider physics of the international high energy physics community and has visited China several times.

In my view, the reasons for the colliders in China are simple and compelling. First, higher energy is mandatory for getting to shorter distances. Shorter distances will reveal whether there is an even smaller substructure than quarks. Second, high energy physics is a proven driver of new technology. It will bring a very high return on investment to industry in China. Third, the international cooperation, collaboration and partnership required for success of the colliders will demonstrate China's leadership far beyond science and be a force for international peace and stability, earning not only a Nobel Prize in science for China but also a Nobel Prize for Peace.[3]

8 October 2016

[2] A Chinese-language translation (by Zhong-Zhi Xianyu & Hong-Jian He) of Prof. Hawking's statement, published in *Mathematics, Science, History & Culture* magazine, is available at https://goo.gl/nme06o.

[3] A Chinese-language translation (by Ying-Zhang Chen & Hong-Jian He) of Prof. Berger's statement, published in *Mathematics, Science, History & Culture* magazine, is available at https://goo.gl/NKRf74.

Supplement to the "Giant Collider in China" Debate

by Henry Tye

Background on Prof. C. N. Yang's opinion on high-energy physics (HEP), sometimes referred to as elementary particle physics, or just particle physics:

I have expressed my personal view supporting the construction of the giant collider in China. Prof. S. T. Yau and Prof. Yifang Wang have already explained clearly the rationale behind this collider proposal and addressed Professor Yang's opposition point by point. This note is a supplement to that discussion, providing some background to the debate, which actually started in the 1970s. Towards the end, I have added a comment expressing my own belief that China can afford the collider project.

The ultimate decision whether to build the giant collider in China or not has to be made by the Chinese leaders. However, I strongly believe that open discussions in the public domain are a healthy phenomenon. When there is an open debate, the public has a chance to learn more about the project, as well as the bigger picture where China is going in terms of science and technology in the 21st century. Naturally, the public will also gain a better understanding of China's position and role in the world. Being a particle physicist, I have a vested personal interest in the collider, though I do not expect to live long enough to see the fruits of this project.

Professor C. N. Yang is a giant in the history of physics. His contributions in the 1950s were a corner stone of the foundation of particle physics. However, since early 1970s, he had moved away from particle physics, so his opinion on particle physics is scarce in the public domain. Based on various sources, direct or indirect, his opinion expressed in a rare interview with Professor Kerson Huang is believed to be genuine. So I would like to include it here. Based on this interview, we can fairly say that Professor Yang was not prescient of the future of high-energy physics even back in the 1980s. I have also added some background comments to provide context for the quotes from the interview.

Below is a part of an interview conducted in 2001 by Kerson Huang with C. N. Yang, in which Yang referred to his opinion on high-energy physics expressed in 1980, an opinion which he continues to hold. Kerson was Professor of physics at MIT, specializing in statistical mechanics, quantum field theory and high energy physics. He collaborated with Prof. Yang in a number of papers on statistical physics.

(Incidentally, our friend Kerson passed away on 1 September 2016 at age 88.)

Kerson Huang's Interview with C. N. Yang, for the C. N. Yang Archive at the Chinese University of Hong Kong[4]

Huang: This is Kerson Huang. It is July 29, 2000. We are in Professor Yang Chen-Ning's office at the Chinese University of Hong Kong. I am interviewing him on the subject of statistical mechanics.[5]

Yang: The truth is, elementary particle physics made great progress in the last century, or in the last 50 years, but its dominance of the publicity of physics is coming to an end.

I don't know whether you know this story. I think you were not there. In 1980, I think, Marshak organized an international conference at VPI. I think you were not there. Marshak specially organized it, partly because Zhou Guangzhao was visiting for a year, or a year and a half, and Marshak was a great admirer of Zhou Guangzhao. So he organized the conference, and many people were there. The last day, Saturday morning, was devoted to a panel discussion about the future of highenergy physics. Did you hear this story?

Huang: No.

Yang: Before that day, I had been asked to participate in the panel. I refused. I said I didn't think I had reasonable things to say. So I was sitting in the audience, and there was a panel on stage. Who was on the panel? Ten people: Marshak, T. D. Lee, Martin Perl, Gursey, Weinberg, maybe Glashow. Zhou Guangzhou? Oh yes, Nambu, and also some Europeans. There were two camps. One camp said W and Z would be discovered, and the other camp said W and Z would not be discovered, mostly in the tone that it's better for them not to be discovered, so you have some puzzle as to what's going on.

They talked for about an hour, and were near the end of the panel, when suddenly Gursey spotted me sitting in the front row.

He said, "Professor Yang is in the audience. We would like to hear his opinion."

I said, "No, no, I already declined to be on the panel."

But then everybody said they wanted me to say something. So, on the spur of the moment, I said to Marshak,

"Yes, I will say something, if you promise not to publish it".

He said OK, and he stuck to his word later.

So I said, "In the next ten years, I think the title of the panel was either the future or the next ten years of high-energy physics," I said, "In the next ten years, the most important discovery in high-energy physics is that 'the party's over'."

[4] See [1], pp. 22–23.

[5] See [1], p. 22.

After I said that, there was general silence. Nobody said a word, and then Marshak declared the panel was finished. I remember immediately afterwards several young people surrounded me, in particular Henry Tye. Do you know Henry Tye?

Huang: Yes.

Yang: So Henry got into an argument with me, and I said, "I won't argue with you; but please remember, what I said to you is more important for your future than mine." (Laughter)

Huang: That's very true; but some people still believe it's not over.

[*The interview goes back to statistical physics*…]

Background

The 1980 conference mentioned was a small meeting with less than a hundred participants. It was at Virginia Polytechnic Institute, or Virginia Tech. Marshak was a well-respected senior theorist there.

The panel was a prestigious one: Lee (Nobel 1957), Weinberg and Glashow (Nobel 1979) just won the Prize, while Perl (Nobel 1995) and Nambu (Nobel 2008) got the Prize years later. Gursey is a professor from Yale University. We all know Zhou.

I could not agree with what Yang said that day, so we argued. Yang thought that HEP was going to fade away quickly and all of us should do something else. Yang himself has certainly moved away from the field by 1980. In the 1970s, Yang had been advising young folks like me to move into another field. (I must state that a few young talented particle physicists had taken Yang's advice to move to other areas and have very successful careers).

So far, history tells us that Yang is not a good predictor of the future of high energy physics. In 1980, the W and Z bosons have not yet been discovered (in fact, Glashow and others have suggested an alternative which does not have the Z boson), so the standard model that unifies the electromagnetic and the weak forces is still nothing but a theoretical idea. Over time, many (some beautiful) theoretical ideas fell to the wayside as data became available. At the time, CERN was getting ready the proton-anti-proton collider to search for them, which was discovered soon after in 1983 (Nobel 1984).

The top quark was discovered in 1990s and then the Higgs boson was discovered in 2012. In the mean time, we learned a lot about the neutrino sector. This completed the proof of all the key ingredients of the unification of the electromagnetic and the weak forces. Together with quantum chromodynamics (QCD) for the nuclear (strong) force, we can now claim to fully understand all forces and matter observable today. Collectively, all these discoveries rest on what we call the standard model. Most of them were made in experiments using the colliders, at Fermilab or at CERN.

Since 1984, string theory has been intensely studied by many of the best minds in the world. The success of the standard model allows us to go beyond, pushing cosmology to the new frontier. Since the 1970s, the field of high-energy physics has grown substantially; now it is truly an international community. The center of gravity has shifted from USA to Europe. Many countries have built up teams to participate at the CERN Large Hadron Collider (LHC) and other experiments. Asia's role has grown substantially. Where will high-energy physics be 20 years from now is the big question. A next generation collider is a must if there is an answer to this question and a necessity to maintain the vitality of the field.

If China decides to build the giant collider, then the center of gravity for high-energy physics will shift to China. If Europe decides to go ahead with their giant collider project, then Europe will remain as the center for the rest of the 21st century. Because of the ongoing LHC project, Europe has to wait for another 5 to 10 years before they have the resources to move forward. That is the window of opportunity for China, since the HEP community cannot and will not support 2 giant colliders. If China decides to build it, the decision itself will bring instant prestige in science to China, much like the announcement of AIIB which brought instant recognition and financial clout to China.

It is true that such discoveries require expensive colliders and large teams working together, which may contradict the inviolable spirit of individualism in research. However, because of the nature of the problem, it is unavoidable. In terms of cost, support for individual HEP scientist is comparable to those in many other scientific fields, except that in this case, high-energy physicists must pool all their resources together to form huge collaborations. Chinese physicists have to work with physicists coming from all corners of the world, and such an international project takes decades to complete, not years.

Human civilization can develop because humans pool their resources together to advance. If everyone has to hunt or farm for his/her family, he/she will have no time for any scholarly activity. Philosophers and scholars can function when others take care of their daily needs. Over time, as intellectual pursuit becomes more sophisticated, we need larger and larger teams (building jet planes, fusion experiment, international space station etc.) to reach new heights. High-energy physics has led theway in basic science. When heights have to be reached, other fields are moving in that direction also (gravitational wave detection by LIGO is a team of 1000). Genomics, Brain initiative etc. are moving along this pathtoo. So far, such initiatives are intellectually driven, above the issues of race, religion, gender, nationality and cultural differences. Scientists will work harmoniously with others from different backgrounds for a common goal. This is an ideal mode for human civilization. Such projects will surely do more for world peace than an expensive weapon.

I have said earlier that Yang did not embrace the ways others apply his 1954's idea to construct the models for the electromagnetic, weak and strong forces in the early 1970s. This involves a rather deep philosophical issue concerning symmetry. Think of a face. It should be symmetric between the left side and the right side. No one wants a face that is asymmetric, i.e., where the left-right symmetry is broken. Symmetry had played a central role in physics, and in Yang's career. The Yang–Mills theory proposed by him with Robert Mills in 1954 follows directly from a deep beautiful symmetry. (I should say that Prof. Yang's papers are gems: the clarity, the elegance and the insight they exude.) However, in using Yang's theory to build the model that unifies the electromagnetic and the weak forces, the symmetry is spontaneously broken. This might be why Yang did not readily accept it. For the nuclear force in quantum chromodynamics (QCD), Yang's idea is realized in a way where the beautiful (color) symmetry is hidden. What good is a symmetry that one can never see? In any case, Yang had not worked in these directions that most of the particle physicists have been deeply involved in since the early 1970s, with fantastic results.

For someone like Yang who likes to put symmetry front and center, it is a bit ironic that his breakthrough 1956 work with T. D. Lee (Nobel 1957) is to point out that the left-right (parity) symmetry is actually broken in nature.

Earlier, I said that we can now claim to fully understand all forces and matter observable today. Why do we need a new collider? The reason is this is not the end of fundamental physics. For one, we now know that our observable matter in the universe constitutes only 5% of the content of our universe. The rest are dark matter and dark energy. So we know there are more things for us to discover and to understand. There are plenty of theoretical ideas, however only experiments can determine the truth. Also, there are puzzles that we'd like to understand better:

– the mass hierarchy issue
– supersymmetric particles?
– signatures for string theory
– then there are the unknown

Some of the questions may be addressed by astrophysical or cosmological observations and underground experiments, but nothing can replace a giant collider that will probe energy scales beyond the present collider. Our understanding of nature can move to the next level only with a combination of efforts on all fronts.

Is the giant collider too expensive for China to undertake? This is a question for China is decide. Certainly, the worst scenario is to approve a long term project and then cancel it later, wasting substantial resources, like the SSC (Superconducting SuperCollider in USA) in 1990s, or to a lesser extent, LISA (Laser Interferometer Space Antenna in USA) a few years ago.

I certainly agree that there are many other areas of research and development (RD) that deserve very strong support from the government. I believe that the giant collider should not and will not squeeze out other areas of research. Sometime, it is easy for one to lose track of the tremendous progress China's RD has made in the past 30 some years. Look at the chart provided by National Science Foundation (Science and Engineering Indicator 2016) [2], which includes data up to 2013. The numbers are based on PPP, i.e., purchasing power parity, which is considered to be more reliable in measuring the actual funding level. China's RD has grown close to 20% a year for the past decades. It is equal to USA's RD budget in 2016 (about 0.5 Trillion in US dollars) if this has not already happened. This growth is unprecedented in human history. It will most probably more than double within the next 5 to 10 years reaching 1 Trillion US dollars per year. Now the giant collider requires about 0.5 Billion (US) a year on average. That is less than 0.1% of China's annual RD budget in the coming years. The collider project is not going to squeeze out other areas of research.

We can also look at where Europe is in the same chart. When they decided to build the LHC at CERN in the 1980s, the CERN budget was quite substantial compared to the European RD budget. Yet, other areas of RD in Europe have grown rapidly at the same time. Not only other RD areas have not suffered from the budget demand of the LHC project, I believe their rapid growth was in part spurred by the confidence Europe gained from the success of CERN. We see that Europe continues to support CERN strongly for the conceivable future. Clearly they have decided that money was well spent in this expensive international project.

References

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21 September 2016

Mister Yang and Mister Sci: Viewpoint on the Big Collider[6]

by George W. S. Hou, National Taiwan University[7]

Everything else aside, after 150 years of disgrace and misery, the single most important achievement since the founding of the People's Republic is perhaps that China finally recovered the ability to build anything. This in fact rose out of rather difficult predicaments, and the products were usually not cutting edge, but China finally became self-reliant again. With the pent up energy released by the economic reform, in a few decades China has risen to become the world's second largest economy, returning as such to the international arena. The financial crisis in America and Europe only helped to elevate the influence of China.

So, China has restored itself, but restore to what?

Big Country Considerations

Professor Chen-Ning Yang was in Taipei in March 2015 to receive an honorary degree from National Taiwan University. During the second half of the banquet hosted by NTU President Yang, I asked Prof. Yang about his opinion on the CEPC. Prof. Yang, as in the past, expressed his disapproval, and recounted with such clarity his opposition of the construction of the Beijing Electron-Positron Collider (BEPC). His disapproval back then was not so different, where, aside from concern for the dismal livelihoods of the Chinese populace, the main emphasis was to pursue "useful science". Curiously, he himself brought up what Deng Xiaoping said at the decision to go ahead: "We already know Prof. Yang's opinion, so one need not ask again." The arguments for disapproval were certainly much stronger back then, since China had so much misery and issues to deal with.[8] So I asked, since Hefei[9] no longer suffers from power shortage, while economic issues would always exist, shouldn't China think also from the Big Country perspective? I brought up the 1950's slogan "Nukes even if no pants", which he agreed that it was the correct decision. I concurred that it concerned the survival of the Chinese nation, hence was more grave a matter. But, after 200 years of fallen status, what position would China restore itself to? The banquet was winding down with well-wishes and all, so the discussion came to an end.

To build Super Colliders such as the CEPC and its possible successor, a Super Proton-Proton Collider, the SPPC, construction costs are in units of 10 billion US dollars, let there be no doubt about it. But the expensive nature is not the theme of our discussion. When the Chinese economy is still growing at fast pace, this is by no means unaffordable. Furthermore, as the economy seeks to transform into the next phase, major investment towards the long term future could reap dividends in both sci-tech development and economic growth. Considering that the body of Chinese High Energy Physics (HEP) is underinvested compared with many other frontier fields, and the orders of magnitude difference with international HEP bodies, China really needs to catch up. These aspects have been discussed well by Director Wang. The combination of CEPC/SPPC offers a great opportunity for China to demonstrate the resolution to become a new world-leading center for a most frontier domain of human civilization. This is not the "indigenous steelmaking" for a renewed "Surpass England, Catch-up America" movement,[10] but to invite the whole world to join forces in developing human civilization for the next 60 years. Execution and internationalization would evidently be core challenges, but only by resolutely plunging forth with perseverance will true progress materialize.

Mr. Tech and Mr. Yee

What Prof. Yang is concerned with, I would call "Mr. Tech" and "Mr. Yee" (Economy), the first rhyming with iron in Chinese, the second rhyming with clothes. But I am not actually worried about these, because China has always been a self-sustaining and affluent economy for millennia, contributing much to technology and civilization. So, how did China lose so miserably against the West? Let us not put the blame politically by calling it Imperialism, the core issue is "Mr. Sci" (rhymes with "game" as in a competition), which was hotly debated in China a century ago. This new development of mankind sprang forth in Western Europe, with pursuit of fundamental truths (curiosity, and "face of God") plus empirical and pragmatic verification methods, ultimately brought "Mr. Tech" and "Mr. Yee" to a new level, such that the World civilization became basically a European civilization, and brought Europeans into the world, including China. The inability for China to respond resulted in over a hundred years of misery, which was aggravated by the successful modernization of Japan.

[6] A Chinese-language version of this essay is available at: https://goo.gl/hGieaM.

[7] Author's disclosure: I am an IAC member of the Circular Electron-Positron Collider (CEPC), and so already hold specific views. This article is written from the perspective of Taiwan, and also as the Chair of the AsiaHEP Forum. It mostly concerns the articles written by Professor Cheng-Ning Yang and IHEP Director Yifang Wang in early September 2016.

[8] The Cultural Revolution had ended only a few years earlier.

[9] Prof. Yang's birth place, the capital of Anhui province.

[10] Both were slogans even before the Cultural Revolution.

It is true that, by account of per capita GDP, "China is still a developing country" (words from Prof. Yang), but lifting a billion souls out of poverty in the past few decades was no mean feat. With China regaining its usual footprint, on one hand Big Country considerations should differ from countries like Brazil with relatively smaller population, on the other hand, China should learn the lesson from how it "lost" against the West. After being absent for a few hundred years, China should restore itself as a main contributor to human civilization and culture, rather than returning to its material and pragmatic civilization of old; the key is the ability for genuine innovation that can renew itself. "Mr. Sci" seeks true knowledge by pursuing the most fundamental, without asking about applications. But as has been demonstrated time and again, the "totally useless" knowledge often brings about the most transformational applications: from the 19th to 20th centuries, fundamental science such as discoveries of electromagneticism, atomic and nuclear structure, DNA, etc. all demonstrate that the greatest impact arise from pursuit of the truly fundamental.

SSC and CERN

Like many, Prof. Yang raises the bitter US experience of the SSC as the first reason against building a big collider in China. In response, Director Wang stresses that it was "a very wrong decision... of the US", making the US "lose the chance to discover the Higgs boson... the foundation for future development,... and international leadership", and that US HEP "has not recovered to this date", which are very well said. When in 1983 the European center CERN discovered the electroweak bosons, it stimulated the US community to plan for building the SSC to discover the Higgs, but it ended in failure, and Director Wang has listed many reasons. The initial effort at keeping the budget low resulted in an ever escalating budget after construction start, is a not unimportant but relatively superfluous reason for the eventual cancellation of the SSC. The main backdrop is the end of the Cold War, and the US "military-industrial-politic complex" was readjusting. President Bush the elder's "100 hour war", or early ending of the first Gulf war, lead to economic recession and Bush losing reelection; the Clinton Democrat government did not see the SSC as its own. Compounding to that is the pitfall of yearly budget approvals by Congress, such that support for the SSC collapsed in face of competition from, for example, the International Space Station. Another flaw in the US system is reflected in the fact that, before the site was fixed, every state supported it, but after Texas was selected, and when the Texas-based Bush lost, Congress could turn around and kill the project. From hindsight, this blocked the Big Country future in HEP for the US. The SSC had a circumference of over 80 kilometers. Had it been built, where would the 27 km CERN LHC stand? And how could China discuss now a Super Collider? The past is a mirror for one to reflect on the present, and the loss for the US opened up opportunities for others.

The first to gain was CERN. On one hand, the pressure from SSC evaporated and the LHC project thrived; on the other hand, US physicists came *en masse* to Europe to help the development, even as the Tevatron continued to run for more than 15 years at 1/7 the energy of the LHC. Today, a majority of high energy physicists worldwide converge on CERN, bringing in funding and resources and making CERN the most successful and internationalized center for Big Science research and technology development. It was in such an environment, with the Big Science/Data frontier nature of HEP, which incubated the invention of WWW that changed human civilization and lifestyle. With its nonprofit orientation toward scientific research, CERN did not possess WWW, but gave it to humankind, which is really commendable.

CERN is a product of the devastation of two great European wars. Based on multilateral international treaties, it enjoys a much more stable structure for growth than the US system based on states. Oppenheimer had once told a French diplomat friend (François de Rose), "... in the future, research is going to require industrial, technical and financial resources that will be beyond the means of individual European countries. You will therefore need to join forces to pool all your resources. It would be fundamentally unhealthy if European scientists were obliged to go to the US or the Soviet Union to conduct their research.", which became an impetus for the establishment of CERN. Now, it is the Americans, people from the former Soviet Union and from all over the world who descend on CERN to do research! The discovery of the Higgs boson certainly brought human understanding of Nature to a new level.

The LEP/LHC Inspiration

The LHC was built in the same tunnel of the Large Electron Project, LEP, which offers inspiration for CEPC/SPPC. Let us elaborate. LEP had in fact searched for the Higgs boson, reaching the mass range of 114 GeV/c^2, which falls short from the 125 discovery by only 11 GeV/c^2. What it means is that, if a somewhat larger ring could be built to reduce the waste from synchrotron radiation loss, one could have a Higgs factory to mass produce the new boson and utilize the clean electron-positron collider environment for precision measurements of Higgs properties, to check whether they agree with predictions of the Standard Model of particle physics. The precision would certainly surpass the LHC, and the Higgs boson is the key to understand Nature's arrangement beyond the scale of electroweak symmetry breaking, hence the key to unlock the secrets of the origin of the Universe. CEPC is an improved version of LEP, its design and construction fits the current abilities of China quite well, and can serve as a platform to train and build the team that befits the economic footprint of China. What is more, analogous to conversion to the LHC after completion of LEP's physics goals, taking the prospects for high energy frontier into consideration, and keeping in mind the pressure from SSC that the LHC had felt, in preparation for a future SPPC, it is advisable to have a larger, rather than smaller, ring size. Put differently: Why should it be smaller than the SSC that the US had planned over 30 years ago? Drilling tunnels is not difficult at all for China. Although we have no guarantee at present for discovery at 100 TeV collision energy (7 times LHC energy), the humankind would have to cross this line sooner or later. Why can it not be led by China for a change? It is time. And if the LHC makes further discovery in the next 20 years, it would not be able to explore it fully, and would only enhance the reason for having an SPPC. Of course, it is more difficult to build a high energy proton collider, which needs high field superconducting magnets and the ability to mass produce thousands of them. While the technologies have yet to mature, it makes clear the good sense of having a CEPC preceding the SPPC, since the construction and operation of the former allows for more than 20 years of R&D and manpower growth towards the latter. It is for such reasons that CERN is highly concerned, hence pushing eagerly the study of the similar FCCee/hh. CERN worries about being overtaken by China. But Europe and CERN have an "Achilles heel": because building the LHC was not cheap and the Phase I Upgrade is just being completed, from the management point of view, the much more expensive Phase II Upgrade must also be done, the running of which would bring us to 2035. In addition, making a 70 km or longer tunnel near Geneva cannot be easier than something similar in China, which is to the advantage of China.

In all, CEPC is a collider with definite science target, but "traditional" enough hence quite doable, while SPPC provides the longer term perspective for development. When I heard about the CEPC/SPPC initiative in early Fall of 2012, not long after the Higgs discovery, I marveled at both its sensibility, and the element of cunning.

Why Should China Do This?

While we emphasized that the failure of the American effort to build the SSC offers China the opportunity, a question that often arises is: "If the Americans won't do this, why should China?" Prof. Nima Arkani-Hamed (who everybody calls Nima), Director of Center for Future High Energy Physics of IHEP, Beijing, and a member of the Institute for Advanced Study in Princeton, has been a staunch supporter of CEPC/SPPC. He admits that he gets asked this question often in China. Let me analyze it.

The Large Hadron Collider Physics LHCP conference in 2014 was held at Columbia University in New York, where a panel discussion titled "The Road to Discovery" was organized. The six panelists were all renowned high energy physicists in positions of influence, but all but Sergio Bertolucci, CERN Director of Research, were American.[11] So, in the setting of an international conference, as the discussion went on, it often felt like an American "townhall meeting". Nima, confessing that he'd been asked the above question by Chinese people, which often puts him on the spot, uttered to himself on stage that part of his drive seems to be: "If China builds a Super Collider, perhaps only then could the US be pushed into action." Now, my discussion here is based on impressions and may not be precise, but what he means seem to be the following. The only Super Power, the United States, would not be perturbed if Europe develops further programs beyond the LHC, because Europe is incorporated into NATO; and if Japan would build the International Linear Collider, the ILC, so much the better, as the US would feel no less uncomfortable; except China... I am all for competition, not for confrontation, but what Nima's words reflect is the frustration and sense of powerlessness of the American community towards their own country's self-limitation. This should not be a reason why China should not do it, but highlights rather the good opportunity that it is. The US definitely has the ability to build a Super Collider, but the shadow of the SSC failure lingers.

Japan has been pushing the ILC since a long time. In fact, the international community has agreed upon building the ILC in Japan, but the chicken-and-egg problem of funding and international participation has still not been resolved. The chief reason behind all this is that, since the bubble burst more than 25 years ago, Japan has yet to find a new economic model. In the meantime, however, China restored itself. From the perspective of several millennia, a restored China pursuing its own sense of mission is only natural, while the "two dimensional" China could do things beyond what Japan can do. But Japan lead the way in modernization, and its scientific accomplishments are superb, well worthy of China to emulate. I myself

[11] Hitoshi Murayama, director of the Japanese Kavli Institute for the Physics and Mathematics of the Universe, is also a professor at Berkeley.

built a team during 1994 to 2008 to join the Belle experiment at the KEK "B Factory", with fond memories of excellent collaboration. I sincerely support Japan to build the ILC. More so, being Asian, I truly hope that both the CEPC/SPPC and the ILC could be built. Is it not clear that East Asia lacks in large scientific research facilities? It certainly does not match the status of having the second and third largest economies. If American, European, Russian and people from all over the world gather to East Asia for scientific research, that would be the true ascent of East Asia.

An Asian Renaissance

That is also in part our theme. For over a hundred years, except for Japan, East Asia has been absent in the advancement of human civilization. China restored itself, but if only to be a manufacturing base, or even a self-sustaining economy based on domestic demand, then it would be deficient. China was absent from the critical period of modern scientific development, shouldn't China take on itself as the vanguard for the next stage of human civilization and have a "Renaissance"? The theme of our discussion, the Super Collider, is a banner that China should simply take up with magnanimity, and the time is ripe for generating the next paradigm of large and international frontier science and technology research.

24 November 2016