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Supplement to the "Giant Collider in China" debate: Background on Prof. C. N. Yang's opinion on high-energy physics*

S.-H. Henry Tye
Hong Kong University of Science and Technology, Hong Kong
Cornell University, USA

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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

*High-energy physics (HEP), sometimes referred to as elementary particle physics, or just particle physics.

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 a 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. URL: http://www.mit.edu/people/kerson/Articlestuff/Yang_interview_2001.pdf).

INTERVIEW OF C. N. YANG FOR THE C. N. YANG ARCHIVE
THE CHINESE UNIVERSITY OF HONG KONG
By Kerson Huang (January 3, 2001)

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.

... (Early discussion is on statistical physics. Refer to the relevant part on pp. 22–23.) ...

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 high-energy 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'."

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–antiproton 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 HEP 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 HEP 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 HEP 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 two 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 the way 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 path too. 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 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 to decide. Certainly, the worst scenario is to approve a long-term project and then cancel it later, wasting substantial resources, like the SSC (Superconducting Super Collider 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), 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$) if this has not already happened. This growth is unprecedented in human history. It will most probably more than double within the next five to ten years reaching 1 Trillion US$ 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.

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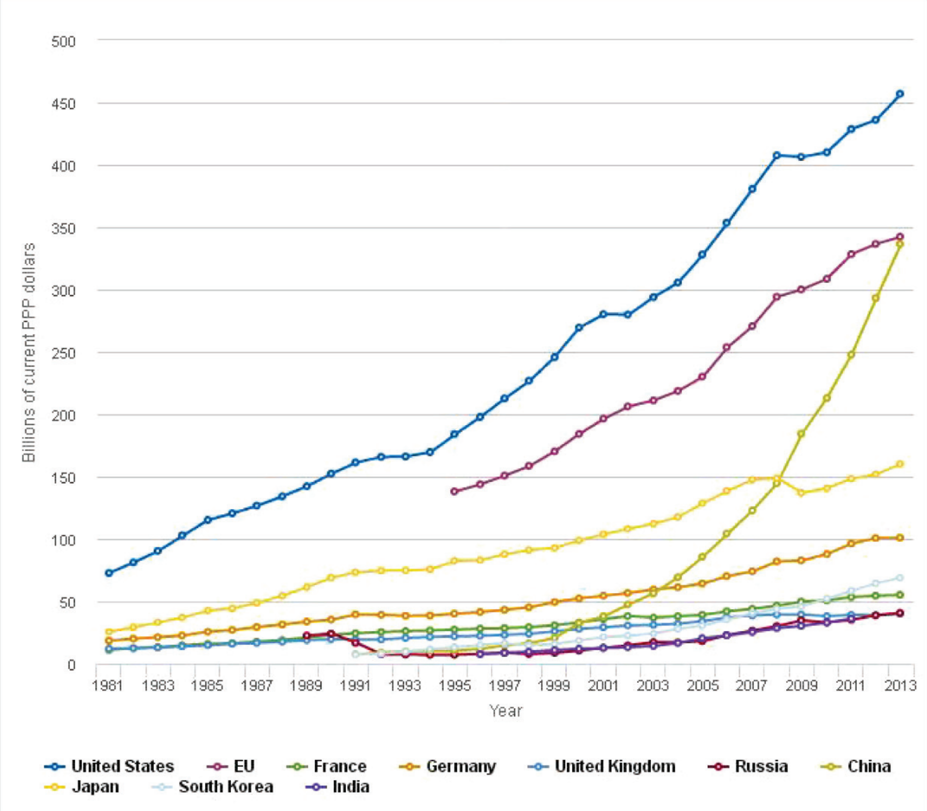
Figure 4-8

Gross domestic expenditures on R&D, by the United States, the EU, and selected other countries: 1981–2013



NA = not available.

EU = European Union; PPP = purchasing power parity.

NOTES: Data are for the top nine R&D-performing countries and the EU. Data are not available for all countries for all years. Data for the United States in this figure reflect international standards for calculating gross expenditures on R&D, which vary slightly from the National Science Foundation's protocol for tallying U.S. total R&D. Data for Japan for 1996 onward may not be consistent with earlier data because of changes in methodology. Data for Germany for 1981–90 are for West Germany.

SOURCES: National Science Foundation, National Center for Science and Engineering Statistics, National Patterns of R&D Resources (annual series); Organisation for Economic Co-operation and Development, *Main Science and Technology Indicators* (2015/1); United Nations Educational, Scientific and Cultural Organization Institute for Statistics Data Centre, http://www.uis.unesco.org/DataCentre/Pages/BrowseScience.aspx, accessed 23 January 2015. See appendix table 4-12.

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