The Construction of a High-Energy Collider: Interviews with Witten, Glashow and Weinberg

by Hong-Jian He*

Interview: Edward Witten Discusses Future High Energy Colliders

Edward Witten is currently the Charles Simonyi Professor at Institute for Advanced Study, Princeton. He recently visited Beijing, where he attended the international conference of String-2016 hosted by Tsinghua University, and right before this meeting he and Professor David Gross (Laureate of Nobel Physics Prize 2004) received Honorary Doctorate from the University of Chinese Academy of Sciences, as conferred by the president of Chinese Academy of Sciences, Chunli Bai. Professor Witten visited China many times before, and on February 23, 2014, he was invited to join the Panel Discussion Meeting on "After the Higgs Boson Discovery: Where is Fundamental Physics Going" together with Nobel laureates David Gross and Gerard 't Hooft *et al.*, held at Tsinghua University, Beijing. For the public in China, most people only know Professor Witten as the Fields Medalist in mathematics and a leading string theorist. But, for the physics community, he is a prominent theoretical physicist who made wide range of important contributions to fundamental physics. In fact, besides the Fields Medal (1990), he has won numerous prestigious prizes in physics, including Dirac Medal (1985), Einstein Medal (1985), National Medal of Science (Physics, 2002), Henri Poincaré Prize (2006), Lorentz Medal (2010), Isaac Newton Medal (2010) Fundamental Physics Breakthrough Prize (2012), Albert Einstein World Award of Science (2016), and APS Medal for Exceptional Achievement in Research (American Physics Society, 2016), to just name a few. The physics community has highly admired his exceptional creativities and contributions, just as the citation of Isaac Newton Medal stated, "for his many profound contributions that have transformed areas of particle theory, quantum field theory and general relativity." The news media often describes him as the successor of Einstein. Even though he is very modest, many fellow physicists would argue that his style also shares deep similarity with Newton because Newton is a truly unique master of modern science who is known as both a great physicist and a great mathematician.

Hong-Jian He: Professor Witten, we just met you in August at String-2016 held at Tsinghua University. It is our great pleasure to have this chance for an interview with you. Our first question is to ask you for your comment on the vital importance of the interface between physics and mathematics, which had revolutionized the progress of physics many times in the past, including Newtonian Mechanics, Special and General Relativity, Quantum Mechanics, and Quantum Field Theory and Gauge Theory. Since you won both the Newton Medal and the Poincaré Prize, it is interesting to note that in the past of modern science, Newton is best known for discovering Newton's Laws in physics, but he invented Calculus for mathematics with the motivation of solving physics problem. On the other hand, Poincaré was a mathematician by birth, but he made fundamental contributions to physics. (David Hilbert is probably another such

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great figure after Poincaré.) Would you like to also comment on both of them from your own experience?

Edward Witten: It took a long time for people to understand that to understand the natural world, one should aim for a precise, mathematical description of basic phenomena. The ancient Greeks, for example, were very interested in mathematics and also in the natural world. But in their study of the natural world, they mainly aimed for qualitative descriptions of everything, rather than looking for precise mathematical descriptions of selected things.

My colleague Steve Weinberg recently wrote an outstanding book on the history of science explaining the long process by which people learned to look for precise mathematical explanations of simple phenomena and not just qualitative descriptions of everything. Newton's laws of motion were one of the big milestones here.

Still, it is a bit of a mystery why mathematics is quite so powerful in understanding the physical world. The best I can say is that physical laws, when we understand them better, turn out to be subtle and elegant. Mathematics is the study of things that are subtle and elegant and can be described and studied without reference to any particular cultural context. We might think that this explains, in part, the utility of mathematics. Alternatively, but with tongue in cheek, we might remark that perhaps the Universe was created by a mathematician—or at least, by a lover of mathematics.

He: Regarding the lessons of Superconducting Super Collider (SSC) in USA, perhaps, may you have seen an article "The Crisis of Big Science" [1] written by Steven Weinberg in 2012 for the New York Review of Books? Lately we recommended the media in China to publish its Chinese translation. 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 seems to have made vital negative impacts on American HEP in particular and in its whole fundamental science in general. On the one hand, SSC was designed to collide proton-proton beams at a center of mass energy of 40 TeV, which is nearly a factor 3 larger than the current Run-2 colliding energy (13 TeV) of Large Hadron Collider (LHC) at CERN, Geneva (the European particle physics laboratory. The energy of the LHC (13 TeV, possibly eventually reaching 14 TeV) is just one-third the energy that the SSC would have had. In designing the LHC, the Europeans tried to compensate for the lower energy by designing a machine with high luminosity (very intense particle beams) but there is only so far that one can go that way.

Since you have witnessed the termination of the SSC and the subsequent developments of the LHC so far, we hope you can share your views with Chinese community regarding the lessons of the SSC and LHC.

Witten: It is a real shame that we in the United States did not complete building the SSC, and if this had occurred, our understanding of fundamental physics might have been quite advanced compared to where we are now. The United States would certainly have maintained its leadership position in high energy physics if we had built the SSC.

I think that one of the lessons from the failure of the SSC project and the success of the LHC is that for a successful project of this kind, it is very valuable to have the capability to make long term plans. The European countries are able to make multi-year commitments to CERN, and on the basis of this, it was possible for the LHC to go ahead. Unfortunately, in the U.S., Congress reconsiders the budget for a project every year and even if a project is approved and funded one year, or multiple years in a row, it might still eventually be canceled.

I can also see one advantage of the way we do things in the U.S. In this country, the budget for a big project would always have some sort of contingency allowance for unexpected costs. In Europe, there was a multiyear plan to build the LHC, but with no contingency plan for even a minor cost overrun. Hence when the LHC did run into a cost overrun, quite small in the scheme of things (less than 10% of the cost of project cost if one takes into account the CERN laboratory resources that were directed to the project), it caused political difficulties and led to a delay in the project of a couple of years.

Based on this, my advice for a country that wants to think big is that it is important to make a multiyear plan with a realistic allowance for reasonable contingencies.

He: You have known the current Chinese plan of the "Great Collider" project, 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. We are glad to tell you that this proposal has been officially ranked as the "First Priority HEP Project" at the recent "Strategic Plan Meeting for Future High Energy Physics" of the Chinese Particle Physics Association, held last month on August 20–21, in Hefei. In fact, CERN is also taking very active studies on a similar proposal, called FCC (Future Circular Colliders), despite that CERN will be mainly occupied by the LHC Run-2 and the subsequent LHC Upgrade over the next 15–20 years. Many colleagues worldwide think that this is a truly promising direction for the next step forward of the high energy physics. We recall that you and Professor David Gross (the laureate of Nobel Physics Prize 2004) wrote a joint article "China's Great Scientific Leap Forward" [2] to support this project in last September (published by The Wall Street Journal). Would you like to share your current views on this subject with Chinese publics? Also, please feel free to comment on the status and achievements of Chinese HEP community, including the past and on-going major experiments, such as the BEPC e^+e^- collider, the neutrino experiments Daya Bay and JUNO, and the dark matter experiment PandaX at Jinping deep underground lab, etc.

Witten: First I want to express my appreciation for the progress that China has made in fundamental physics. In particular, the pioneering measurement that was made at Daya Bay added an important ingredient in our understanding of neutrinos, which are mysterious elementary particles whose study has led to many surprises. The work at BEPC is also much appreciated and we look forward to results from other current and upcoming experiments.

During my visits to China—I have been in the country five times, including three visits in the last several years—I see that the country is advancing rapidly in many areas. Given the changes I see, it is not hard to believe that before too long, China might be the leading country in many areas of theoretical and experimental science.

The CEPC and the 100 TeV collider are very exciting projects and truly worthy of a country that aspires to leadership. For the future development of physics, it is very important to make a deep study of the Higgs particle, as CEPC can do, and to probe beyond the electroweak scale of energies, as can be done at the 100 TeV collider.

He: You probably have heard about the on-going public debate in the Chinese community on whether such a Great Collider should be built in China at all. This debate was provoked early this month by the 94-year-old Chinese American theoretical physicist C. N. Yang, who has been strongly against any collider project in China, including the current CEPC-SPPC project led by IHEP director Yifang Wang. It is clear that his major objection is that the project would cost too much for China, and he stressed the cost of the second phase of pp collision that would be built starting in the 2040's. (As one may recall, the funds of the LEP and LHC at CERN were approved separately and in sequence.) It will be extremely helpful for the Chinese community to learn your independent viewpoint and advice from international side. We recall that you discussed this issue in your joint article with David Gross last fall and also in a recent press-interview at Tsinghua during String-2016. It is a pity that these were barely known to the Chinese publics and were largely forgotten so far.

Witten: Ultimately, China will have to decide what sort of position in the world and in the world of science you aspire to.

Yes, China must continue economic development. China's success in lifting hundreds of millions of people out of poverty is one of the inspiring changes that have occurred in the world in my lifetime. This process still has a long way to go.

Moreover, China's continued economic development is really a precondition for being able to afford to build CEPC in the 2020's and to build the 100 TeV collider starting in the 2040's.

As it develops, China must decide its priorities and its ambitions. For what it is worth, I personally think that China is entirely capable of assuming leadership in many areas of science and that this is a worthy goal that befits your cultural traditions and can benefit your society. And I believe that leadership in high energy physics and multiple other areas can be entirely affordable as China continues to develop.

With that said, the scope of your ambitions is something that must ultimately be decided by the Chinese people.

References

[1] Steven Weinberg, "The Crisis of Big Science", in *The New York Review of Books*, May 10, 2012. For Chinese translation of this article, see: http://chuansong.me/n/678905846230.

[2] David Gross and Edward Witten, "China's Great Scientific Leap Forward," in *The Wall Street Journal*, September, 2015. For Chinese translation of this article, see: http://www.ihep.cas.cn/xwdt/cmsm/2015/201509/t20150926_4431136.html.

Interview: Nobel Laureate Sheldon Glashow Discusses Future High Energy Colliders

Sheldon Lee Glashow is a renowned theoretical physicist, the Higgins Professor of Physics at Harvard University until 2000, and currently the Metcalf Professor of Mathematics and Physics at Boston University. He is a major founder of the Standard Model of particle physics, and one of the masters in modern physics. He was awarded the Nobel Prize in Physics in 1979 (together with Abdus Salam and Steven Weinberg) "for their contributions to the theory of the unified weak and electromagnetic interaction between elementary particles, including, inter alia, the prediction of the weak neutral current". He made many other profound contributions to particle physics, including the GIM (Glashow–Iliopoulos–Maiani) mechanism, the prediction of the 4th quark—the Charm Quark, and the Georgi–Glashow SU(5) Grand Unification Model, to just name a few. Professor Glashow has been elected to the National Academy of Sciences of

USA and to the American Academy of Arts and Sciences.

Hong-Jian He: Professor Glashow, it is our great pleasure to have this interview with you, and to discuss with you about the future plans of particle physics, especially, the recent Chinese proposal of CEPC/SPPC and related public debate which you may have heard. Would you be glad to share your insights with Chinese people?

Sheldon Glashow: I am astounded by the surprising and outspoken opposition expressed by our much-respected colleague Yang to China's ambitious and thrilling proposal to initiate its CEPC/SPPC project. Perhaps Planck was right when he said that science progresses one funeral at a time [1].

He: Lately I re-read your article "Particle Physics in The United States, A Personal View" [2]. It is very thoughtful, though it was intended for the prospects of the high energy physics (HEP) in USA. You said that you hope your country (and your university) will continue its active and effective engagement in the Large Hadron Collider (LHC) at CERN. This is also our plan in China. The LHC Run-2 has been performing well to collide proton-proton beams at an energy of 13 TeV. It has collected about 28/fb integrated luminosity in each detector so far, which amounts to about 10% of the planned full data collection at the Run-2. Although no new physics was announced at the ICHEP conference in August, would you like to share with us about your views on possible new findings (or not) at the on-going LHC?

Glashow: Very disappointingly and to my great surprise, LHC as yet finds no indication, nor even a plausible hint, of physics beyond the standard model. Yet, the enthusiasm and commitment of high-energy experimenters and particle theorists have not been impacted. With only 10% of run-2 data in hand, future surprises remain plausible to anticipate, if not in run-2, then in future work at higher luminosity and/or energy. LHC will remain the front of discovery potential in particle physics for decades to come, for Chinese and American scientists as well as those of CERN's member states. The very big problem is what happens afterward.

He: We are glad that you stressed the importance of precision measurements in your article [2]. You also said [2], "Although the world's next great collider is unlikely to be built in the US, I hope that we will be eager participants in any sensible future multinational efforts." Given the fact that the LHC with pp collisions is unable to precisely measure the Higgs boson properties, would you feel crucial to build up a Higgs factory (e^+e^- collider) such as the CEPC [3] proposed in China? We recall that you visited China a number of times before, including one in this fall. What is your viewpoints on the Chinese proposal CEPC?

Glashow: I have visited China several times, and experienced its very rapid progress. Last month I was in Chengdu: a pleasant town 25 years ago when my children saw it, but now an immense city of 14 million! I was delighted by the panda breeding station, but wasn't then aware of the very promising PandaX project that I should have seen. YES! We need a Higgs factory to verify that the properties of the particle found at LHC are just those expected in a one-Higgs standard model. The Chinese hosted CEPC and the Japanese hosted ILC would have similar energies and luminosities: at least one such machine is truly necessary for the health and survival of particle physics. Each of these machines could also provide useful precision data about Z decay modes, and at higher energy, the WZ coupling. Perhaps new particles would be found that have eluded discovery at LHC... But, most importantly, CEPC is an obligatory precursor to the magnificent SPPC project.

He: Regarding the lessons of Superconducting Super Collider (SSC) in USA, perhaps, may you have seen an article "The Crisis of Big Science" [4] written by your colleague Steven Weinberg in 2012 for the New York Review of Books? Early this month, we recommended the Chinese edition of this article [4] to the Chinese publics. The cancellation of SSC by US congress in 1993 was a great loss for the high energy physics (HEP) community in USA and worldwide, although the proposed Space Station Project in the same state Texas (with about ten times more cost) got approved at the same time [4]. It seems to have made vital negative impacts on American HEP in particular and in its whole fundamental science in general. On the one hand, SSC was designed to collide proton-proton beams at a center of mass energy of 40 TeV, which is a factor 3 larger than the current Run-2 energy (13 TeV) of the LHC at CERN, Geneva. It is thus not so unexpected and disappointed that the LHC Run-2 has not found any new physics beyond the Standard Model (SM) so far, because we all know that the SSC with 40 TeV colliding energy was designed to ensure a much more solid new physics discovery reach at TeV scales. As many physicists expected, if the SSC had not been canceled in 1993, it would probably have already made a revolutionary new physics discovery pointing to a new direction of the fundamental physics in the 21st century. Since you witnessed the history of the SSC and the subsequent developments of the LHC, would you like to share your thoughts with the Chinese community regarding the lessons of the SSC and LHC?

Glashow: The energy of the SSC was carefully considered by many outstanding physicists, both experimenters and theorists. The center of mass energy was agreed to be at least 40 TeV so as to guarantee post-standard model discoveries. As I recall, when SSC was aborted by Congress, CERN soon and very fortunately came up with an initially proposed 20 TeV LHC, which gradually fell from 1/2 to 1/3 of the SSC dream energy. Not to diminish the triumph of CERN's Higgs discovery, it is not all that surprising that new physics has so far escaped detection at LHC: It simply was not a machine designed to push beyond the standard model envelope. The consequences of SSC termination for American high-energy physics have been disastrous. We had dominated high-energy physics from 1953 (with the commissioning of the Cosmotron) to 2011 (with the shutdown of the Tevatron). We have no plan to construct a new forefront particle accelerator at any time in the foreseeable future. I hope that the capricious and unpredictable nature of our Congress will not preclude American participation in large and long-term multinational scientific projects, such as the much desired CEPC and SPPC.

He: Perhaps, you already heard about the current Chinese plan of the "Great Collider" project [3], 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 (called SPPC). We are glad to tell you that this proposal has been officially ranked as the "First Priority HEP Project" at the recent "Strategy Plan Meeting for Future High Energy Physics" of the Chinese Particle Physics Association, held last month on August 20–21. In fact, CERN is also taking active studies on a similar proposal, called FCC (Future Circular Colliders), despite that CERN has been mainly occupied by the LHC Run-2 and the subsequent LHC Upgrade over the next 15–20 years. Most colleagues worldwide think that this is a truly promising direction for the next step forward in the high energy physics.—Would you like to share your views on the CEPC Project with the Chinese community? Also, please feel free to comment on the status and achievements of the Chinese HEP developments, including the past and on-going major experiments, such as the BEPC e^+e^- collider, the neutrino experiments Daya Bay and JUNO, and the dark matter experiment PandaX at Jinping deep underground lab in south China.

Glashow: I am reasonably well informed about the Chinese Great Collider project, and I am delighted that the project is the First Priority of Chinese particle physicists. I strongly support the first stage, the CEPC Project, and even more, the culminating deployment of the Great SPPC Collider. I am also aware that CERN has also been considering future circular colliders. I believe that China is in a much better financial situation than Europe to initiate such a project. I would hope that many countries, including but not limited to the EU and America, would partner with China to complete and exploit the CEPC and SPPC endeavors.

China has been taking giant steps in particle physics! The multinational Daya Bay experiment was the first to measure $\theta 13$ despite many unsuccessful prior attempts elsewhere, such as by the French. This was a truly important discovery. Further precision studies of neutrino oscillations will soon be carried out at the JUNO facility. These are expected to resolve the important question of neutrino hierarchy. China has also entered the dark matter sweepstakes with Panda-X, a series of Xenon detectors with increasing sensitivity at the world's deepest underground lab. As of this year (2016), PandaX-II has established the world's most stringent dark matter constraint [5]. Significant future increases in sensitivity are planned. Lastly, BEPC II has achieved world record luminosity for e^+e^- collisions in the energy range 2–4 GeV. With its new BES III detector, it has obtained several exciting results, such as the discovery of the Z_c (3900) particle, with more new states soon to follow. All in all, Chinese particle physics has experienced a remarkable growth spurt, as is both befitting and essential if China is to host the Great Collider.

He: You may have heard about the on-going public debate [6] in the Chinese community on whether such a new collider should be built in China at all. This debate was provoked early this month by the 94-year-old Chinese-American theoretical physicist C. N. Yang, who has been strongly against any collider project in China, including the current CEPC-SPPC project [3] led by IHEP director Yifang Wang. Attached below is an English translation of Yang's recent public article, and a summary of the debate between Yang and Wang (published in "China Daily"). It's clear that Yang's major objection is that this collider would cost too much for China. (CEPC was actually estimated by the Chinese team at IHEP to be about 6 billion US dollar over a ten-year period of construction.) A misconception of Yang was to stress the cost of the potential second phase of pp collider SPPC that would be built from 2040s. (As anyone may recall, the funds of the LEP and LHC at CERN were approved separately and in sequence.) It will be extremely helpful for the Chinese community to learn your opinions and advice from the international side. Would you think that the fund invested for CEPC worthwhile? and what would this contribute to the world through the international collaborations? and to the society of China?

Glashow: Needless to say, I disagree with Yang.

1) China can easily afford to build and operate the proposed facilities.

2) China has won only one Nobel Prize is science. It wants more. Many Nobels have gone to particle physicists in the past, as they will in the future. CEPC and SPPC will make China the world hub of particle physics.
3) It is our duty to try to understand the world we are born to. Others have become reluctant, so it is now the opportunity and the obligation of the Chinese people to take up the challenge.
4) Fundamental physics affects society: One third of the global GDP depends on quantum mechanics; the world-wide-web was developed by and for particle physicists. Of medical scanners: CAT won Nobels for two particle physicists, MRI (as well as nuclear power) spun off nuclear physics, PET uses positrons. Industrial and medical accelerators are a multi-billion dollar business... need I go on?
5) Science has always been international. Both CEPC and SPPC, as multinational efforts hosted by China would continue the tradition of such international efforts as CERN, LIGO, HUBBLE, etc.

References

[1] Note: Max Planck's original statement was in German: "*Eine neue wissenschaftliche Wahrheit pflegt sich nicht in der Weise durchzusetzen, daß ihre Gegner überzeugt werden und sich als belehrt erklären, sondern vielmehr dadurch, daß ihre Gegner allmählich aussterben und daß die heranwachsende Generation von vornherein mit der Wahrheit vertraut gemacht ist.*" (English translation: "A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up who is familiar with it.") Professor Glashow's translation has highlighted its essence.

[2] Sheldon Lee Glashow, "Particle Physics in The United States, A Personal View", arXiv:1305.5482, in the *Proceedings of Community Summer Study 2013: Snowmass on the Mississippi* (CSS2013), Minneapolis, MN, USA, July 29–August 6, 2013.

[3] Circular Electron Positron Collider (CEPC) and Super pp Collider (SPPC), http://cepc.ihep.ac.cn/. See the recent science book for introducing this subject: S. Nadis and S. T. Yau, *From the Great Wall to the Great Collider: China and the Quest to Uncover the Inner Workings of the Universe*, 2015, International Press of Boston, Inc., Massachesetts, USA.

[4] Steven Weinberg, "The Crisis of Big Science", in *The New York Review of Books*, May 10, 2012. For Chinese edition of this article (translated by Zhong-Zhi Xianyu), see: http://chuansong.me/n/678905846230.

[5] A. Tan et al. [PandaX-II Collaboration], *Phys. Rev. Lett.* 117 (2016) 121303 [arXiv:1607.07400 [hep-ex]], which is highlighted as the Cover Article of the issue no. 12.

[6] For a summary report of Yifang Wang's refutation against C. N. Yang, by China Daily, "It is Suitable Now for China to Build Large Collider".

[7] David Gross and Edward Witten, "China's Great Scientific Leap Forward", in *The Wall Street Journal*, September, 2015. For Chinese translation of this article, see: http://www.ihep.cas.cn/xwdt/cmsm/2015/201509/t20150926_4431136.html.

Interview: Nobel Laureate Steven Weinberg Discusses High Energy Colliders

Steven Weinberg is a renowned theoretical physicist and a great master of modern physics. He is currently the Josey Regental Chair Professor in Science at the University of Texas at Austin, where he is on faculty of the Physics Department and Astronomy Department. He is a major founder of the Standard Model of particle physics, and was awarded the Nobel Prize in Physics in 1979 (together with Sheldon Glashow and Abdus Salam) "for their contributions to the theory of the unified weak and electromagnetic interaction between elementary particles, including, inter alia, the prediction of the weak neutral current". His research on elementary particles and cosmology has been honored with numerous other prizes and awards, including National Medal of Science (1991), J. R. Oppenheimer Prize (1973), Heineman Prize of APS (1977), Elliott Cresson Medal of Franklin Institute (1979), James Madison Medal of Princeton University (1991), and Benjamin Franklin Medal of American Philosophical Society (2004). He has been elected to American National Academy of Sciences and Britain's Royal Society, as well as to the American Academy of Arts and Sciences. He has also served as consultant at the US Arms Control and Disarmament Agency, and the JASON group of defense consultants. He taught at Columbia, Berkeley, MIT, and Harvard where he was Higgins Professor of Physics, before coming to Texas in 1982.

He is the author of over 300 articles on elementary particle physics. His books include *Gravitation and Cosmology* (1972); *The First Three Minutes* (1977); The Discovery of Subatomic Particles (1983, 2003); *Elementary Particles and The Laws of Physics* (with R. P. Feynman) (1987); *Dreams of a Final Theory: The Search for the Fundamental Laws of Nature* (1993); a trilogy, *The Quantum Theory of Fields* (1995, 1996, 2000); *Facing Up: Science and its Cultural Adversaries* (2002); *Glory and Terror: The Growing Nuclear Danger* (2004); *Cosmology* (2008); *Lake Views: This World and the Universe* (2010); and *To Explain the World: The Discovery of Modern Science* (2015), etc. His book *Dreams of a Final Theory* was written for the support of building the Superconducting Super Collider (SSC) in USA. His article "Big Crisis of Big Science" was written in 2012 in which he discussed the importance of big projects for the sciences and high energy physics as well as the lessons of the SSC.

Hong-Jian He: Professor Weinberg, it is our great pleasure to have this interview with you. I recently reread your article "Particle Physics, from Rutherford to the LHC", first published in Physics Today

[1], where you explained why the new physics is required to go beyond the Standard Model (SM) of particle physics for which you were a major founder, "It is clearly necessary to go beyond the standard model. There is a mysterious spectrum of quark and lepton masses and mixing angles that we have been staring at for decades, as if they were symbols in an unknown language, without our being able to interpret them. Also, something beyond the standard model is needed to account for cosmological dark matter." These are indeed what the particle physics community has been striving for over the past thirty years, through the major high energy colliders including Tevatron in USA and LEP & LHC in Europe. The LHC Run-2 has been performing very well to collide proton-proton beams at an energy of 13 TeV, which has collected about 10% of the planned full data sample of the Run-2 so far. Even though no new physics is announced at the ICHEP conference in August, would you like to comment on your expectation of possible new findings at the on-going LHC?

Weinberg: It is impossible for anyone to know whether there are significant new discoveries that will be within the capabilities of the LHC. From the beginning, there had been strong reasons to anticipate that the LHC would be able to discover the mechanism by which the symmetry governing weak and electromagnetic forces is broken—either elementary scalar fields, as in the original electroweak theory, or new strong forces, as in technicolor theories. In either case, the observed strength of weak forces gave a powerful indication that new scalar particles or new strong forces would be observable at the LHC, as turned out to be the case. Indeed, this provided a guide in planning the LHC.

But, although there are several other phenomena of great importance that might be discovered at the LHC, including dark matter particles and superpartners of known particles, we have no strong reason to suppose, even if they exist, that they would be within the reach of the LHC. We will just have to wait and see.

He: We know you was the major supporter of the SSC [2]. Early last month, we recommended the Chinese translation of your review article "The Crisis of Big Science" (2012) [3] to the Chinese publics. 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 seems to have made vital negative impacts on American HEP in particular and in its whole fundamental science in general. On the one hand, SSC was designed to collide proton-proton beams at a center of mass energy of 40 TeV, which is a factor 3 larger than the current Run-2 colliding energy (13 TeV) of Large Hadron Collider (LHC) at CERN, Geneva. Perhaps, it should not be so unexpected and disappointed that the LHC Run-2 has not found any new physics beyond the Standard Model (SM) so far, because we all know that the SSC with 40 TeV colliding energy was designed to ensure a much more solid new physics discovery reach at TeV scales. As expected by many physicists, if the SSC had not been canceled in 1993, it would probably have already made revolutionary discovery of new physics beyond the SM and thus have pointed to a new direction for fundamental physics in 21st century. Since you have witnessed the full history of the SSC and the subsequent developments of the LHC so far, would you like to share your views with the Chinese community regarding the lessons of the SSC and LHC?

Weinberg: Even after the SSC program had been approved by the US government, it continued to meet opposition from several directions. Part of the opposition came from those who generally prefer small government and low taxes, and therefore tend to oppose all large government projects, especially those projects for which there is no large number of immediate beneficiaries. The project would obviously provide economic benefits to people in its neighborhood, but these persons would be limited in number. One US senator commented to me that at that moment, before the site of the SSC had been decided, all 100 members of the Senate were in favor of it, but that once the site was chosen, the number of senators in favor would shrink to two, the two senators from the state of the chosen site. Even before the final site had been determined, one member of the House of Representatives who had favored the SSC turned against it once it was clear that the SSC would not go to his own district. All this was standard politics, perhaps of a sort that is not restricted to the US.

Much more disturbing was opposition from within the scientific community. No one argued that the SSC would not do important scientific research, but some urged that the money needed for the SSC would be better spent in other fields, such as their own. (It did not provide much consolation when the SSC was cancelled that the funds saved did not go into other areas of science.)

There was implicit opposition to the SSC from advocates of the LHC, who pointed to the financial savings from their use of an existing tunnel. The smaller circumference of this tunnel limited the LHC energy to only about one third of that possible for the SSC, but proponents of the LHC argued that the LHC could make up for its lower energy by operating at higher intensity, though this higher luminosity obviously carried its own difficulties, due to the several particle collisions in each intersection of bunches.

One explanation that is sometimes given for the cancellation of the SSC is that its projected costs kept increasing. This was certainly charged by some of the opponents of the SSC, but I don't believe it was a fair criticism. The only real increase in the cost of the project was approximately ten percent, made necessary by a calculation of the aperture needed for the SSC beam. Whatever increased cost there was beyond that came from the slowdown of funding from Congress, which required an extension of the time for construction, and hence an extended time in which construction personnel had to be employed.

The SSC project was killed chiefly by competition from a program that masqueraded as science, the International Space Station. This was to be administered at the Johnson Manned Space Flight Center, in Houston, Texas. It was not politically possible to support two large technological projects in Texas, and the Space Station was chosen. In the end, it cost ten times what the SSC would have cost, and has not led to any important scientific research. (The one possible exception, the Alpha Magnetic Spectrometer, could have been operated as well or better, and much more cheaply, on an unmanned satellite.)

The LHC has been a great success, with the discovery of the Higgs boson. Whatever the LHC's chances for further important discoveries, it is clear that the much greater energy of the SSC would have provided a better chance for the future.

He: Perhaps, you already heard about the current Chinese plan of the "Great Collider" project [4], 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 (SPPC). This proposal was officially ranked as the "First Priority HEP Project" at the recent "Strategy Plan Meeting for Future High Energy Physics" of the Chinese Particle Physics Association, held on August 20–21, 2016. This plan has received worldwide supports of the international HEP community since its inception [5].—You probably have heard about the on-going public debate in the Chinese community on whether this Collider should be built in China at all [6], [7], [8]. This debate was provoked by the Chinese-American theoretical physicist C. N. Yang in September, 2016 [7], who has been strongly against any collider project in China, including the current CEPC-SPPC project led by IHEP director Yifang Wang. Attached below are English translations of Yang's recent public article [7], and Yifang Wang's refutation [8]. 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. (The IHEP team estimated [8] 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. It will be extremely helpful for the Chinese community to learn your viewpoints and advice from international side.

Weinberg: I have tremendous respect for the scientific research carried out by C. N. Yang, but I do not agree with his arguments against the proposed CEPC. Some of them are familiar, being used again and again against large scientific projects.

Yes, society has many other needs, including environment, health, education, and so on. It always does. But it also has needs for arts and sciences that make its civilization worthy of respect.

Yes, no immediate practical applications are likely to follow from discoveries made at particle accelerators. But the projects themselves have important practical consequences in the form of technological spin-offs. Frequently cited examples include synchrotron radiation, used to study the properties of materials, and the World Wide Web.

A less frequently cited spin-off is intellectual. The fundamental character of elementary particle physics makes it very attractive to bright young men and women, who then provide a technically sophisticated cadre available to deal with problems of society. In World War II microwave radar, cipher-breaking computers, and nuclear weapons were developed by scientists who before the war had been concerned with problems of fundamental scientific importance rather than of military value. One of the best graduate students who started work with me on elementary particle theory later became interested in more practical problems, and has developed what may become the leading approach to isotope separation. The country that pursues only research of direct practical importance is likely to become unable to make not only discoveries of fundamental importance but also those of practical value.

One of Professor Yang's arguments is that progress can be made without new accelerators by the search for beautiful geometric structures. This reminds me of a position taken after World War II by another great theoretical physicist, Werner Heisenberg. He argued against German spending on particle accelerators, on the grounds that progress could be made through theoretical studies of certain field theories [9]. It is true that without the impetus of new experimental results, in trying to understand the strong forces Yang and Mills did develop a field theory of a class that later turned out to be realized in nature. But the correct Yang-Mills theory of strong forces could not be guessed until accelerator experiments revealed a weakening of strong forces at high energy, and the relevance of Yang-Mills theories++ to the weak and electromagnetic interactions could not be confirmed without new accelerator experiments that discovered weak neutral currents. Theory can only go so far without experiment.

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[4] Circular Electron Positron Collider (CEPC) and Super pp Collider (SPPC), (http://cepc.ihep.ac.cn). See the recent science book for introduction of this subject: S. Nadis and S.-T. Yau, *From the Great Wall to the Great Collider: China and the Quest to Uncover the Inner Workings of the Universe*, 2015, International Press of Boston, Inc., Massachusetts, USA (https://thegreatcollider.com).

[5] David Gross and Edward Witten, "China's Great Scientific Leap Forward", in *The Wall Street Journal*, September, 2015. For Chinese translation of this article, see: http://www.ihep.cas.cn/xwdt/cmsm/2015/201509/t20150926_4431136.html.

[6] Shing-Tung Yau, "Comments on the Construction of a High-Energy Collider in China and Reply to Media's Questions", August 30, 2016.

[7] C. N. Yang, "China should not build a super-collider now", September 4, 2016.

[8] Yifang Wang, "It is Suitable Now for China to Build Large Collider", September 5, 2016.

[9] Note: This refers to, after the German atomic bomb project failed in World War II, Heisenberg's works on certain unified field theory of elementary particles since 1953 in which he tried to derive the so-called "world equation". As is well-known, his attempts ended up in vain and have been forgotten thereafter. See: Werner Heisenberg Biography (https://nobelprize.org); David C. Cassidy, *Uncertainty: The Life and Science of Werner Heisenberg*, Freeman (1992), cf. Appendix A.