Europe’s next big particle collider?

The Large Hadron Collider (LHC) at Europe’s particle physics centre CERN is the largest and one of the most successful scientific instruments ever built. The discovery of the Higgs boson in 2012 at the LHC resulted in the award of a Nobel Prize to the British theoretical physicist Peter Higgs and in the ensuing years the machine has produced an astonishing wealth of new data about the mechanisms which shaped the very early universe. It is likely that these investigations will continue for several more decades. Nevertheless, we are now at a cross-roads on the journey to selecting our next big particle collider project, the successor to the LHC , but we are probably not quite ready to make such a momentous decision. How did we get to this point and why is it proving to be so difficult to resolve this matter?

In the past six decades enormous progress has been made in the understanding of the basic building blocks of matter and the nature of the forces that act between them. Experiment and theory have taken turns to lead us to the next step along the road to discovery and deeper knowledge. Increasingly larger and more powerful accelerators and colliders have been the engines of this enterprise.

In the 1950’s and early 1960’s there was lots of experimental data but not much theoretical understanding. Many new particles were discovered but no underlying and unifying principle existed which could make sense of the plethora of new types of matter. That all changed in 1964 with the introduction of the quark model of matter and the subsequent birth of the Standard Model of particle physics in which the constituents of matter are the leptons (e.g. electrons and neutrinos) and quarks which interact via the exchange of force carrying particles (e.g. photons and W, Z bosons). For the next 30 years, using a series of grander and more ambitious accelerators and colliders, just about every prediction of the Standard Model was tested and experimentally verified. Indeed, the only missing piece of the jig-saw was the observation of the Higgs boson, a particle related to the mechanism whereby the quarks and some of the other fundamental particles acquire their mass, which was eventually discovered at CERN.

The case for constructing a machine as large and expensive as the LHC was partly built around the discovery of the Higgs boson and the measurement of its properties. It was also motivated by the possibility of finding new physics which would provide a deeper understanding of why the Standard Model is the way that it is. Esoteric questions such as the relative lightness of the Higgs boson, the origin of neutrino mass, the matter-antimatter asymmetry of the universe and the nature of the dark matter which is thought to account for nearly one quarter of the universe’s energy are not explained by the Standard Model.

When the LHC began construction the vast majority of the worldwide theoretical particle physics community was in agreement that the discovery of new (beyond the Standard Model) physics was likely to be observed at the LHC. They even went so far as to expect that a “supersymmetric extension” of the Standard Model would be discovered.
“Supersymmetry” was at the time a new and appealing way of arranging the “bosons” (photon, W, Z and Higgs bosons) and the “fermions” (leptons and quarks) together in symmetric patterns which reflected a mathematically beautiful way of understanding

physical law. The energy of the colliding beams in the LHC was considered enough for these patterns to emerge as “supersymmetric” partners of leptons and bosons would be produced. Furthermore these “partner” particles would explain the dark matter that astronomers’ observations had demonstrated is required

One of the important outcomes of research at the LHC to date has however been the consistency of the validity of the Standard Model with the experimental results. The physics embodied in it seems to be followed well in that its predictions for the way in which the sub-nuclear world works at the LHC are borne out. New physics beyond the Standard Model of any form, regardless of whether it is of the supersymmetric kind or otherwise, seems not to be necessary. Indeed, the accuracy with which the predictions of Standard Model physics have been confirmed at this time at the LHC is a remarkable discovery in itself

This remarkable outcome of LHC experimentation to date will be taken to new levels of sensitivity during the next run of the LHC which begins sometime in 2021 or very soon thereafter. Whether evidence for new physics beyond the Standard Model is discovered or not is thus becoming a matter of yet more experimental dedication and persistence, much as is always the case in science. After all, though first postulated in 1964, it took nearly fifty years to discover the Higgs boson and in that time the discovery of the sixth and heaviest quark took twenty years. And throughout this half century worldwide half a dozen increasingly more energetic particle colliders were built in mankind’s search for the fundamental theory of matter in the Universe. The cutting edge of science is always like this, and particle physics is no exception.

It could well now be the case that in our search to understand the nature of matter and energy in the Universe that, as in the 1960s, experimentatal discovery will again lead the way. Whether or not this discovery will be at the LHC, and the machine could well operate for another 15 to 20 years, is unknown (as is always the way with fundamental research). But in the meantime, because of the scale of any such project, what should be the world’s next big particle collider and what other machines will also be necessary, and all at what cost, becomes an important issue in sub-nuclear science.

This is the junction which the world’s particle physicists now face in the absence of hints about the nature of the new physics from the LHC. In May of this year about six hundred physicists from Europe’s research centres in particle physics met in Granada to discuss an update to the European Strategy on Particle Physics which is refreshed every seven years. The process that they initiated will take about twelve months to complete and the new strategy will emerge as a consensus by the middle of 2020.

This consensus follows in the first instance a number of indisputable facts. Following the discovery of the Higgs boson, the measurement of the parameters of the Standard Model of particle physics with the highest possible precision is a major imperative. While the energy scale at which new physics may emerge is presently unknown, the search for possible deviations to the Standard Model’s predictions is a reliable road towards breakthroughs in the field. Determining precisely the properties of the Higgs boson is a major objective of this strategy because of its pivotal role in the Standard Model. The best possible sensitivity to such deviations at the LHC requires the highest possible rate of production of Higgs bosons which in turn requires the highest possible intensity of proton-proton collisions at the energy of the stored beams in the LHC. We say that the “luminosity” of the LHC must be maximised. An upgrade in luminosity is scheduled for completion by 2026. It will yield a five-fold increase in particle collisions per second. Thus, the main debate at the Open Symposium in Granada concerned future colliders at which experimental measurements could bring a substantial improvement in our understanding.

The history of collider physics includes machines in which high energy beams of protons and of electrons collide at the feasibly highest energy possible. Electron-positron colliders (SPEAR, DORIS, ADONE, PETRA, PEP, SLC, LEP), in which beams of electrons and their antimatter equivalents (positrons) collide with each other head-on, hadron colliders (ISR, SppS, Tevatron, RHIC, LHC) in which two beams of protons (the nuclei of hydrogen atoms) or of a beam of protons and their anti-matter anti-protons, or even of other atomic nuclei, collide head-on, and an electron-hadron collider (HERA) have all contributed to the establishment of the Standard Model. Almost all of these machines have been circular in layout with counter-rotating beams colliding at fixed points around the rings where the particle detectors are located. In the future electron-positron colliders are more likely to be linear in layout (to avoid extremely expensive energy losses from the beams) unless enormously large (100 Km) circumference rings are used to house them which would be nearly four times longer than the LHC tunnel. Presently it is not unreasonable to envisage that electron-positron and hadron colliders will continue to be the preferred tools of choice, although a third but extremely ambitious alternative could emerge in the form of a muon collider, a roughly circular machine with counter-rotating beams of muons (heavier cousins of the electron) and their antimatter equivalent. Electrons and muons are members of the lepton family of particles, so it is natural to refer to lepton colliders when both types are under consideration.

Within a broad consensus on the central physics goals, the global particle physics community is presently confronted with choices among potential future colliders. Although the LHC will collide protons until the late 2030s, the scale of the technological challenges make it essential to begin to plan now for the future. R&D in accelerator technology, strategic planning and organisation of the availability and the allocation of resources over a long construction phase must be carefully considered in global collaboration. The technologies which are necessary for such a project require substantial R&D phases, for example, advanced high-field radio frequency particle acceleration. For collisions of electrons and positrons in a “next generation” collider, the Compact Linear Collider (CLIC) at CERN would use room-temperature radio frequency structures with a two-beam acceleration scheme (i.e. a low energy drive beam transferring energy to a high energy accelerated beam). The 100 Km circumference Future Circular Collider (FCC-ee), also at CERN, would require superconducting radio frequency devices to achieve its target performance at several interesting energies, including the Z boson, Higgs boson, W boson pair threshold and for the top quark. Experiments at such electron-positron colliders would enable a “step-up improvement” compared with present experiments at the LHC. There is also a linear collider project, the International Linear Collider (ILC), which awaits approval for construction in Japan if enough international investment could be made available.

High-energy hadron colliders beyond the LHC rely on larger tunnels and higher-field superconducting magnets. A major contender is the hadronic version of the Future Circular Collider (FCC-hh), built from bending magnets that are twice as powerful as current magnets, in the same 100 km tunnel attached to the present CERN complex as the FCC-ee. After the FCC-ee, this could provide exploratory proton-proton collisions at seven times the LHC collision energy, a comprehensive programme of first electron-positron and then proton-proton head on collisions whose cost and operation would span many decades.

Very similar 80-100 Km circular layouts for electron-positron and hadron colliders are also being discussed in China, a nation with huge ambitions for science and technological development in the coming decades. Should China decide to commit to the construction of such a high profile facility in the next few years (initially for electron positron collisions and later for hadrons), it would certainly seek substantial foreign contributions to offset some of the very high cost associated with such an enterprise. However, China would also have to make huge investments in engineering and technology to bring the large national commitment to fruition, and it has plans to do so. If China declared nationally its intention to pursue such a course, it would ultimately open the way to its global leadership of the field in decades to come.

So, how should European particle physicists decide between electron-positron and hadron collider options, or even a muon collider? Previous experience suggests that hadron colliders are generally instruments of discovery whilst electron-positron machines investigate in detail the nature of new discoveries. This approach worked very well for the W and Z bosons, discovered in the early 1980’s at CERN’s previous hadron collider and then studied with extraordinary precision at CERN in the Large Electron Positron collider, LEP. This principle could apply for the detailed investigation of the properties of the Higgs boson, thereby spawning the idea of a “Higgs factory”, an electron-positron collider of sufficient energy to copiously produce Higgs bosons. This could be realised with the ILC concept in Japan, a low energy version of CLIC or with one of the circular collider options, either in China or at CERN. The muon collider could also function as a Higgs factory but the technological challenges of building such a machine are much greater than for the more familiar alternatives.

However, the conventional wisdom of building a hadron collider followed by a lepton collider may no longer be completely applicable in the 21st century. The LHC will, after the luminosity upgrade, produce very large numbers of Higgs bosons, whilst the ATLAS and CMS detectors are extremely powerful instruments for studying their properties. In some types of Higgs boson decay into lighter particles the Higgs factories will be no more effective than the LHC at measuring the relevant decay parameters. There are exceptions to this, where the factories would yield superior results but overall the performance differences may not be sufficient to warrant the expenditure that would be incurred in building such a new machine. Thus, it might be premature to commit to a Higgs factory at the moment. It may be better to wait to see if the LHC, with its much higher energy reach, reveals evidence of new physics. If and when that happens, but possibly not until the 2030’s, it might then become very clear what type of electron-positron collider is required to further investigate the new phenomena. If nothing new is found at the LHC then it may make more sense to pursue the FCC-hh option and simply focus on even higher hadron collider energies rather than the inherently superior precision (but lower energy reach) of electron-positron colliders. That being said, once the decision to excavate a 100Km tunnel is made, it would be very natural to install the FCC-ee option first to improve the measurement of the Higgs boson decay parameters that are best measured at a lepton collider. After that, further exploration of the new high energy frontier (seven times the LHC energy) could commence.

It is therefore tempting to conclude that now is not the ideal time for Europe to decide what type of large particle collider it should operate after the LHC has done its job. We simply don't have enough experimental guidance yet to really point the way forward with confidence. Perhaps the particle physics community ought to postpone this decision until the 2027 European Strategy refresh? Alas, the problem with that approach is that it will take many years to develop the technologies required for some of these new machines. For example, the high field superconducting magnets for FCC-hh might take 10-15 years to fully perfect and then another few years to mass produce and install in the new machine. That may not matter too much if the plan was to precede the FCC-hh option with 5-10 years of data collection with a FCC-ee machine. Nevertheless, the magnet development programme must be initiated fairly soon and very substantial financial resources would have to be committed to it. Ultimately, CERN probably has to strive to keep open as many options as possible but the development of new accelerator technology is an expensive business and some hard choices lie ahead.

Ultimately, we know what we can and could do if we had sufficient resources. We must continue to grow investment for R&D in a number of technologies which also have a direct synergy with many other uses of accelerators. This investment must continue to include pivotal solutions to unparalleled issues in technologies of automated control and data management which particle physics poses and which also as they emerge and with their solutions will continue to enable societal benefit. While in parallel, the immediate consequences of this R&D will underpin the full exploitation of science at the energy scale of the LHC, will focus the options for the choice of a future collider, and will enable hitherto unrealised and innovative new experimental approaches whose possibility may well provide the clues concerning how to pursue the next conceptual paradigm in this fundamental science. The future for this vibrant field of research remains bright and whether the next major breakthrough occurs in a few years from now or in several decades, the one thing we can be certain of is that extraordinary discoveries and profound new insights will eventually emerge from what is one of humanity’s greatest endeavours.

Peter Ratoff
Director, Cockcroft Institute
Daresbury, UK.

December 2019