2.9 Appendix: EF Community Input

- Comment # 1: Despite the fact that we live in an exciting time for experimental particle physics, there is a general malaise that seems to be setting in due to the projected long timescales for a new collider at the energy frontier. I think reinvigorating the field with bold new ideas is going to be necessary, and we may wish to focus some of our energies toward accelerator technologies to make them a reality. A cutting-edge e+e- machine would be a good shorter-term goal, while also making strides toward a mu+mu- machine on the Fermilab campus for a medium-to-long-term goal.

- Comment # 2: From the outside (i.e. the European community) I would be fired with enthusiasm if the Americas/the U.S. comes actively back into collider physics. A second EF lab besides CERN would benefit the whole field in terms of competence and competition, education and diversity. Within the next 20 years, to me the only realistic goal is a cutting-edge (quoting comment #1) e+e- collider that covers Higgs, top and the electroweak sector and that is extendable into the TeV region, to give us guideline on higher energy scales and complement the discovery potential of hadron colliders in the electroweak sector. Beyond that, R&D towards a (multi-)TeV-scale muon collider and high-field magnets for the next proton collider are of great importance. This has to be complemented by a thriving field of theoretical physics in precision modelling and model building as well as scientific computing.

- Comment # 3: The EF has a compelling short- term program at the LHC. Run 3 is about to begin, and later the HL-LHC will provide new discoveries and opportunities. The excitement is high in the community, and the US has leading roles in all phases of research. Continued funding of HL-LHC to ensure the construction of the upgrades will allow full realization of our investment.

  In the longer term, there are compelling arguments for a Higgs factory medium term and for exploring higher energies longer term. To keep the transfer of knowledge from one generation to the next and to continue with the exciting science, at least one Higgs factory option should come on line no more than 5 years after the end of HL-LHC. This means construction should start during the HL-LHC run. (As an active member of FCC-ee, I want to add as an aside here that with the help of the US, this is possible with FCC-ee, which uses technology developed for Belle-II, has substantial funding in the CERN current budget to develop the technical details, has already identified sources for a substantial portion of its construction funding, has strong existing infrastructure, and a well established international community working on LHC)

  All costed Higgs Factory options are expensive. We therefore need maximal flexibility to work with the world community to achieve this goal, and should not present a vision to P5 that urges constraints that will hinder this. Instead, we should present the case for a Higgs factory and higher energy exploration in as machine-independent manner as possible, while urging haste in working with the world community to come to consensus, as time is short. We know that work now is being hindered by the too prescriptive language in the last P5 report. A crucial goal is a stable funding mechanism for big science similar to the extremely successful mechanism that funds CERN, and a laboratory that will feel that success of our field - no matter where the hardware is located - is crucial to their success. To accomplish this, a charge to a cabinet-level position is probably necessary. Without a well-organized and stable funding structure, a future machine cannot occur on the required time scale. Until a global decision is made, the US should engage actively in all the feasibility studies ongoing, including FCC-ee, ILC prelab, C3, muon collider, FCC-hh etc especially in areas that are germane to all efforts. Sufficient funding needs to be made available to allow the US to have credible participation in these efforts. Indeed, already the US is falling behind in higgs factor detector development and physics feasibility studies. There need to be at least several full time scientists at each lab and several in the University community, including several research scientists.

A vibrant accelerator community is essential. Novel accelerator techniques, like muon colliders, C^3, high field magnets for FCC-hh will find applications both for our field and in industry. Some critical mass R&D should continue in all these areas. And it is essential that US accelerator physicists become actively involved in the FCC-ee feasibility study and be allowed to travel to FCC-ee workshops before it is too late.

Finally, a strong theory community drives our vision towards possibilities to explore. Funding is essential. The program should be diverse while containing a substantial component dedicated to the precision calculations needed to get the most from the increasingly precise future large data sets.

We should minimize mandates for effort not related to the core HEP mission to prevent continuing diffusion of limited funding.

- Comment # 4: The #1 problem in particle physics is the nature of the Higgs boson. The description of the Higgs boson given by the Standard Model of particle physics can parametrize our current data on elementary particles. But this description does not answer the "why" questions, either for the spectrum of particle masses and mixings, or for origin of dark matter, or for the baryon-antibaryon asymmetry, or for the origin of neutrino masses, or for the fundamental breaking of electroweak symmetry required in all explanations. Without understanding the Higgs boson, we cannot make progress on any of the most important fundamental questions.

  To make progress, we need new information from experiment. There are many possible directions, including machines that access higher energy or, alternatively, higher precision. The #1 problem for the Energy Frontier is to choose a single direction as next major accelerator project after the LHC. We in the US need to work with our global partners to develop a plan. Given that it takes 5 years to create an engineered design and 10 years to construct a collider, we need to formulate a plan now to have a next collider that will operate within the careers of our current students and postdoctoral fellows.

  Given this timeline, there is a unique choice for the next collider. It should be an e+e- Higgs factory operating the CM energy range of 240-600 GeV, with sufficient luminosity to measure the couplings of the Higgs boson to 1% precision. The physics case for such a machine is very strong. There are designs for both linear and circular colliders that meet this goal. The required technologies are either ready now or achievable with straightforward programs of R&D.

  At this time, there are a number of proposals for Higgs factory colliders being considered in different regions of the world. These include the ILC in Japan, the FCC-ee at CERN, and the CEPC in China. All three of these machines require very substantial support from governments, and today none of them has a clear path to funding. As US participants in the Energy Frontier, we need to state clearly that we consider the physics goals important independent of the technology. All three of these machines can meet our goals, and we will participate in whichever project goes forward.

  In view of the uncertainties in all three cases, we also support exploring US hosting of a Higgs factory collider. This might be based on any of the three approaches currently on the table. We also encourage R&D toward a smaller and less costly design. Most likely, a US-hosted machine proposal will need to argue that it has the minimal cost needed to meet the physics goals.

  The detector requirements and designs are very similar for all proposed Higgs factories. It is essential that US particle experimenters are supported today to develop technologies for Higgs factory detectors in order for the US to play a leading role in the eventual global project.

  The precision study of the Higgs boson is not the end of the exploration of the Energy Frontier. The leading models of the Higgs field available today include particles at energies well beyond those of the LHC. In almost all cases, the study of their key features requires parton-parton CM energies above 10 TeV. Thus, the Energy Frontier must continue to expand the capabilities of accelerators toward higher energies. Today, there is no accelerator technology that offers a cost-effective design for a collider at the 10 TeV scale. We need to continue to develop advanced technologies that will bring us to higher energies. But this should not keep us from taking the steps we can take now to explore the Higgs boson and gain insights that will point the way forward.

- Comment # 5: I think the Physics results that can be done are very compelling. However, I think this project needs to be presented with some level of interesting risk that would enable the next generation to get excited and motivated to solve the problem. The current efforts on the detector technology and physics process appear as incremental updates from technologies developed during the time of LEP. Building something affordable, like C^3 goes a long way into helping motivate next generations, provided there is enough freedom to be creative and take risks. Ideas in this direction would be :

  1. Going further a field from the ILC/Other detector design, higher beam frequencies, higher material budget, newer detector technology 2. Leveraging C^3 technology to explore high energies, through muons or electrons 3. Understanding the complementarity of HEP with C^3 technology Free electron lasers with the same tech

  More importantly, I don't think the younger generation will feel ownership of such a project unless they have the opportunity to try something risky or novel. While there has been a lot of thought put into a future lepton collider, this thought has largely happened before the current generation of physicists(young faculty, postdocs) were able to contribute. While much of this is optics, C^3 might be a way to re-think things. More generally, building some ownership, through ingenuity amongst the younger generation would be good. A key point to consider is that the younger generation was raised on the LHC, and bringing about similar challenges to the LHC would help to get people excited. Part of the excitement from Muon colliders originates from this same motivation.

- Comment # 6: For the immediate future (next several years) it is clear that completing the HL-LHC upgrades and exploiting the data from the LHC needs to remain the top priority of the energy frontier for the short/medium term goals.

  The next goal must be having all the tools in place to make the decision on what projects for the future we should invest in and ultimately construct. With our current knowledge, this is a machine to investigate and test the EW sector at the precision scale with technology that is viable currently. An e+e- Higgs factory of either circular (FCC-ee, CPEC, ..) or linear ILC collider present similar physics potential and should clearly be the next goal beyond the LHC. The decision between these machines appears to be more dominated by political will than fundamental physics and as a community we should support whatever path that will reach us this goal in the intermediate time scale.

  Beyond that we must also make clear investments into the best and most cost effective way to reach the many TeV scale directly. Currently this means investment in both R&D into very large hadron colliders at the 100 TeV scale and novel ideas like muon colliders at the 3,10, or 30 TeV scale. A reasonable ask is on the order of 10-20 million for R&D of dedicated funding per year for these efforts so that we are in a good position in the next 10 years to decide which of these projects is the best route to that scale.

- Comment # 7: A clear vision for the future starts with what we've learned from the LHC. The Higgs is the most important data point given to us from the LHC thus far and our vision must be centered on trying to answer the numerous questions we are left with from the Higgs. Additionally, a lack of confirmed BSM physics in direct and indirect searches means we must be prepared to make a jump beyond the TeV scale.

  With the lessons of the LHC in mind, this clearly argues for an e+e- Higgs factory and an Energy Frontier machine probing the 10+ TeV scale. The Higgs factory for making new measurements that the LHC is unable to do, and the Energy Frontier machine for probing a different set of Higgs related questions that remain unanswered without energy, as well as exploring the unknown. It is also imperative to bring back a collider program to the US for the health of the field and to strengthen the US role within it. This starts with collider R&D being supported by this snowmass/P5 which could prepare a vision for a US based collider by the next P5 as the strongest post DUNE future for US HEP.

This particular vision could take many forms, with e+e-, pp, or muon colliders built in the US, CERN or elsewhere. However, for US HEP developing new technologies and methods is the key to sustaining excellence, and bringing excitement and the next generation of physicists into our field. With this in mind I highly support a US vision which supports muon collider research towards a 10+ TeV program, since it ticks that box but also enables synergies in HEP outside the EF as well.

- Comment # 8: The discovery of the Higgs boson without any accompanying new particles discovered at the moment just makes electroweak symmetry breaking even more mysterious. The underlying dynamics that is responsible for generating the Mexican-hat Higgs potential remains unknown and continues to be one of the deepest questions in fundamental physics. Only energy frontier is able to address this question and it could not be replaced by experiments at other frontiers.

  Direct searches for new physics at the LHC suggests that the next energy scale could be above the TeV scale that was expected before the LHC and leaves relatively less room for a sizable deviation to be observed in precision measurements, compared to the early LHC days. With that said, precision measurements are definitely important and the e+e- machines could be technologically more ready for near-term projects. Yet since our ultimate goal is to directly find new particles and identify new particle mechanisms (whether it could be achieved in our own lifetimes or not), I hope that the community could keep an open mind for other possible avenue beyond the traditional precision to high energy route, such as a high-energy muon collider, which could potentially achieve precision measurements and directly search for new particles. The hope is that the muon collider program could get some support for its R&D development to keep it in the list of possible future colliders. On both the experimental and theoretical sides, a muon collider provides many opportunities for the current and future generations of particle physicists.

- Comment # 9: The goal of Snowmass reports should be to present a menu of options for P5 to pick from. As with any menu, every dish should be presented in as attractive a light as possible, and there should be a variety of dishes, including appetizers, main courses, desserts, etc. P5 will have to figure out how to maximize the physics within budget constraints year to year, as projects sunset and others start. It is difficult to anticipate how this will work in advance, especially without a list of the "menu" items presented by the other frontiers. For this reason, it is good to give P5 a range of options, from expensive to less expensive options, those yielding near-term results to those preparing the way for the long-term future, etc.

  Given this, it would be good for the vision presented in the EF report to include the fact that the energy frontier has long been the driving force behind progress in particle physics, and there is no reason this should be any different in the coming years. There continue to be well-known and highly-motivated large projects, but there are also new ideas and innovations that show the vitality of the EF, including the FPF, C^3, renewed interest in the muon collider, etc. All this leads to great ideas in all categories, including long-term projects certain to produce important results in the long term, projects requiring near-term investment and certain to produce near-term results, and projects requiring near-term investments to prepare the way for potential long-term breakthroughs.

- Comment # 10: For the future of collider physics continuity between the HL-LHC program and the next Higgs factory is beyond critical. Maintaining and growing expertise on both accelerator and detector technologies will require a clear roadmap that would ensure R&D funding. Having a well defined path beyond HL-LHC will help retain and attract talent and form the next generations of particle physicists. The current uncertainty on what's next beyond HL-LHC is causing a gradual loss of interest in pursuing in our community detector R&D and more generally in the collider physics program. A clear commitment to a Higgs factory now, will re-vitalize not just the R&D investigations in the US but inject enthusiasm and excitement in the community. When thinking about the next Higgs factory, prioritizing flexibility to explore high energy would inject for sure additional excitement. Ideally that would build technical expertise in the US that we will be able to leverage to design the roadmap for the next discovery machine targeting 10 TeV energy scale.

- Comment # 11: All the old ideas in support of weak scale supersymmetry remain valid and it is hard to fathom the existence of the spin-0 Higgs boson without its protective supersymmetry. And supersymmetry is supported by a variety of data combined with virtual effects. Our best understanding of string theory these days points to a landscape of vacua which favors a very SM-like Higgs boson with mass around 125 GeV and sparticle masses beyond present LHC search limits. Given this situation, it is important to support HL-LHC in the short term, an e+e- collider such as ILC which can ultimately extend to 600-700 GeV in the medium term, and support FCC-hh in the long term. A big new tunnel at CERN could support a 50 TeV collider using conventional, reliable magnet technology. And if advances in reliable magnet technology are made, then perhaps we may move to 75-100 TeV or beyond in the far future.

- Comment # 12: To address the important questions unanswered by the standard model and unveil the related mysteries, the future high-energy project must be versatile, with a scope as broad and powerful as possible, and with a dual capacity of unprecedented precision/sensitivity and energy reach. Without reducing the inclusiveness of the excellent comments from Michael and Sarah above, the following remarks can be made.

  The Future Circular Colliders offer unique opportunities in both directions, with a strategic operation in two stages, providing together a powerful long-term vision with complementary and synergistic physics programmes. The e+e– machine provides ideal conditions for the study of the four heavy particles of the standard model, with a flurry of opportunities for precision measurements in the Higgs and EW sectors, searches for rare or forbidden processes, and the possible discovery of elusive feebly coupled particles. The very-high-statistics operation at the Z pole, complemented by the runs at the W and top-pair-production thresholds, will refine to an unparalleled level the exploration of new physics through its quantum effects on EW observables. The possible successive 100 TeV pp collider, whose feasibility and success requires the e+e- machine as a first step, would synergistically complement the precision measurements in the Higgs and EW sector, and greatly extend the discovery reach at high mass.

  More pragmatically, the FCC project leverages the CERN existing accelerator complex, its available infrastructures, its organisational and administrative services, its stable budget, and decades of worldwide collaboration. Such a research infrastructure, coupled to a long-term strategic programme serving the worldwide community, is the grass roots to the successful implementation of any large-scale project. In addition to this favourable situation, and as stressed by the 2020 European Strategy update, intellectual and technological contribution from the worldwide high-energy physics community will be key for the FCC-ee project to be approved by the CERN Council following the next update of the European Strategy for Particle Physics.

  Any of the ambitious projects on the table at Snowmass will require immense resources, firm commitment of the host governments, and global international support. These political considerations will carry large weight in the ultimate choice, and may end up being the determining factor. As proponents, we are engaged in making the scientific case of our favourite project as strong as possible. As a community, we must ensure that the political process following Snowmass remains open to evaluate all top options for future colliders. In addition to the support of renewed R&D efforts to pursue the high-energy frontier with colliders in the US, we believe that a strong and positive statement from Snowmass regarding the extraordinary scientific value and reach of the FCC will prove a necessary and precious springboard for the continuation of the adventure. Today, US physicists play an important and visible role in LHC. Their leadership role in shaping and exploiting the FCC is both desirable and necessary. Now is the time to get involved.

- Comment # 13: Value and challenges of precision measurements: On 8 April 2022, the CDF collaboration reported the W mass to be 80433.5 9.4 MeV, to be compared to the prediction from EW precision observables (within the standard model) of 80357 6 MeV. Taken at face value, this large difference would only be accommodated by the existence of new physics at a relatively low energy scale. A considerable excitement arises from this result, which in itself demonstrates the interest and passion around precision measurements. Amazingly long stories about these findings have been broadcasted on several news channels in Europe. The large difference between this measurement and the current world average (80379 12 MeV), however, points to subtle systematic effects and therefore calls for some caution before jumping to conclusions.

  This confused (and confusing) situation will only be resolved if the community decides to proceed with substantially more precise and more accurate measurements. As a matter of fact, the FCC-ee offers the best prospects for an improved and systematic-uncertainty safe W boson mass measurement, with several 108 W pairs delivered to up to four different detectors, two different methods (direct reconstruction and threshold cross section), and rigorous detector calibration based on Z pole data. Uncertainties such as lepton momentum scale or pdfs will simply not exist. The projected combined sensitivity of 0.4 MeV or better, thanks to the very precise beam energy calibration with resonant depolarization, is about 25 times better than that of the CDF measurement.

  The FCC-ee also features an extraordinary Z factory, with several Tera Z (51012 Z), which will bring "decisive improvement on the many electroweak precision observable (EWPO) measurements. The prediction of the W mass from these EWPO measurements (and from the precise top-quark mass measured with the million top pairs produced at threshold), will therefore also drastically improve down to a precision of 0.3 MeV or less, about 20 times better than the current world average, dominated by the previous high-energy e+e- circular collider, LEP. The comparison between the direct measurement and the indirect prediction of the W mass to the refined level of precision expected at FCC-ee will bring powerful answers to the present puzzle. With respect to today's result, the expected significance of this difference will be multiplied by 20: increased precision will then equate discovery potential.

  Obviously, the FCC-ee precision program is not limited to the W mass measurements. As mentioned above, it provides many other observables with precisions improved by one to three orders of magnitude, which will be expressed in units of keV or ppm! At the Z pole, the Z mass and width, the effective weak mixing angle, the leptonic and heavy flavour partial widths and left-right asymmetries, the invisible partial width (N_v), the QED and QCD coupling constants at the Z mass scale, the tau lifetime, mass and branching ratios, and many heavy flavour observables and rare decays; and at higher energies, the W branching ratios and anomalous couplings, the neutrino neutral current couplings; the higgs boson mass, width and couplings to the Z, W, b, tau, charm, gluon, possibly electron and neutrino, and maybe even its self coupling; the top quark mass and its electroweak couplings. The list is only waiting to be augmented with new ideas. This perspective requires a considerable improvement in experimental systematic errors and theoretical precision to match the FCC-ee statistical uncertainties, on a large set of measured observables. The multidimensional approach will help eliminate spurious deviations; possibly reveal a pattern of deviations, guiding the theoretical interpretation; and enlarge the phase space of sensitivity to new physics by orders of magnitude.

  The predictive precision of models for new physics will also need to be adapted to the improved experimental precision. Whether or not the direct W mass measurement and its indirect standard-model prediction from EWPOs finally agree, the precision expected with FCC-ee will then allow a multitude of these new physics models to be rejected, thus strongly limiting the field of possible new physics interpretations, and providing a clearer vision of what to look for, either at the 10 TeV energy scale (or beyond), or for light particles with much weaker couplings. In this regard, and independently of its ultimate fate, the recent CDF measurement serves as a timely wake-up call to remind us of the physics case depth of an e+e- collider with high luminosities from the Z pole to the top-pair threshold, expanding well beyond that of a (remarkable) Higgs factory.

This opportunity must not be missed.

- Comment # 14: One question we as a community should ask ourselves: do we want to keep moving into a direction where there is one single lab (CERN) that is leading the development at the energy frontier. Currently, I do not see any other entity that will build the next energy-frontier machine: the effort of CEPC/SppC has unfortunate political constraints making me doubt the US/world will support it, and the ILC has been on the verge of being decided on for (over) a decade so that it is unclear that there will be a positive decision by the Japanese government. While it is clear that the US is now focusing on building DUNE (which is an important project), we need to make the decision now if the US wants to reinvigorate efforts to be a leader at the energy frontier. Let us not forget that the Tevatron and LEP were running in parallel. I believe that having two parallel efforts (e.g. CERN's effort on the FCC-hh and another lab towards a lepton collider) will be beneficial not only for the field but also both entities (as one says: competition is good for business). Especially building a machine in quick succession of the HL-LHC would keep the field vibrant and I am sure would also motivate a lot of students in choosing the energy frontier as their subject of study. The energy frontier at Snowmass should be confident in itself to have a vision where the US could build a new machine regardless of CERN's plans on the FCC. I also believe that this could fit into an encompassing US strategy including the current strong focus on neutrino physics: the energy frontier will first need to put together the R&D effort for the next machine while in parallel the construction of DUNE is ongoing - once DUNE is operational and the insights of its data are being harvested, one could consider to start construction of that new machine. On a personal note, I would be especially excited if the community would take a bold decision towards a forward looking technology that has great potential to go beyond being "just a Higgs-factory": I became interested in a muon collider as there are challenges that I as a (still) young scientist will have to solve while the machine itself has the potential to take our field to a completely new level of having elementary particle collisions in the multi-TeV range

- Comment # 16: After the discovery of the Higgs boson, we still have little insight into the origin of electroweak symmetry breaking. This key question in particle physics could have an answer at the multi-TeV scale. For example, the simplest explanation of the 125 GeV Higgs mass in the context of supersymmetry involves squarks at around 10 TeV. Similar statements apply to composite Higgs models. The Standard Model-like Higgs at the LHC, the 125 GeV Higgs mass, and the lack of physics beyond the Standard Model so far in precision flavor or CP-violating physics all point toward multi-TeV scales for new physics. Independent of any particular models, a leap forward of an order of magnitude in energy is always a promising strategy for discovering new physics.

  It is imperative that our plans for the future of particle physics aim to reach energy scales well above that of the LHC as soon as possible. This requires either a hadron collider at much higher than LHC energies, such as FCC-hh, or a lepton collider operating at the multi-TeV scale. A 10 TeV muon collider or e+e- collider would have comparable physics reach to a 100 TeV proton-proton collider. Any of these options would have major discovery potential and could help us to build the next Standard Model.

  A Higgs factory, which does not reach a new energy frontier, can enable many precise Standard Model measurements, could potentially discover light hidden sectors (if they are there to be discovered) in rare Z or Higgs decays, and has some chance of finding deviations from the Standard Model. However, the argument that we should rely on such deviations to point to the next energy scale is dangerous. In many cases, Higgs factory measurements probe an energy range similar to the LHC. For example, the simplest "natural SUSY" spectra would be probed up to stop masses of around 1 TeV; much of this parameter space is already excluded by the LHC. New physics at the 10 TeV scale, which could shed light on the origin of the electroweak scale, could remain invisible to Higgs factories. We must not make the case for building a true energy frontier collider contingent on Higgs factory measurements. This could doom the field to never have another collider, if Higgs factories only confirm the Standard Model. I find it very plausible that this is all a Higgs factory would achieve. Even if a percent-level deviation in Higgs properties is observed, the natural next step would be to build a collider reaching the 10 TeV scale to understand it. We should not wait for such hints (though we should welcome them if we find them). We should be planning, now, for colliders that reach the 10 TeV scale.

We should also consider the possibility that our clues to higher energy scales could come from non-collider experiments, which are powerful complementary probes of particle physics that deserve very strong support. Electron EDM measurements are making extraordinarily rapid progress and are sensitive to a broad range of CP-violating electroweak physics at multi-TeV scales (potentially even the PeV scale, within the next decade). Charged lepton flavor violating (CLFV) experiments are also making enormous strides toward the multi-TeV scale. A discovery from such an experiment would be exciting, but would carry minimal information about the nature of new physics. Only a collider can directly reach high energies and allow us to build the next Standard Model.

In light of these exciting precision developments, our plans for the future should be more nimble. We should plan for reaching high energies sooner, rather than later. If EDM or CLFV experiments decisively show that the Standard Model breaks down, it would be absurd to continue pursuing precision Standard Model physics for decades, rather than aiming directly for the new physics itself at high-energy experiments.

It is crucial that the Snowmass process supports R&D toward advanced, high-energy colliders. These include not only high-energy hadron colliders (with the associated requirements for R&D in high-field magnets), but also novel technologies such as muon colliders or strategies for reaching much higher energies at electron-positron colliders. In particular, the construction of a 10 TeV muon collider appears to be within the realm of plausibility. A major R&D effort in this direction is well-justified. It could allow the US to be an active participant in energy-frontier physics, complementing other colliders in Europe or Asia. Young accelerator and collider physicists are excited by the possibility of working toward a qualitatively new collider, with unprecedented physics reach, within their lifetimes. Such a collider is naturally where we would want to turn if we learn about new physics from precision experiments. It would offer an exciting hybrid of high precision and high energy. It is premature to argue that we must build such a collider in the US, but it is an absolute necessity for any reasonable future of the field that major R&D is carried out to assess the option.

- Comment # 17: If possible, 'Vision for EF for the next 20 years, and also beyond' can include proposals for research programs offered to young scientists. Training networks for PhD students and postdocs (with fellowships), in alliance with commercial companies (IT, industry etc). Networks among universities, research institutes, also at the international and intercontinental levels. Future colliders define research projects which are attractive for young generations of talented people, which may seem to be paradoxical concerning their long-term goals. However, already established projects (grants, networks) with challenging EF tasks attract young researchers. At least this is my experience with FCC-ee PhD and postdoc projects. So I think that this strategy for EF studies could be intensified. As we want to test many big issues in particle physics like vacuum structure and possibly elementary particles substructures or issues between particle physics and cosmology, programs to engage young researchers is a must. I think that Snowmass can indicate proper directions for such programs.

- Comment # 18: Having been invited to express our vision for the future of HEP in the US, we obviously can only give our impression as outsiders with all due caveats.

The discovery of the HIggs boson has opened a new era of exploration. There is the HIggs boson itself... and there is the big unknown: where in the space of masses and couplings lie the new particles or phenomena that would explain the mysteries left unanswered by the Standard Model. The FCC is an extremely attractive solution to this multidimensional problem, thanks to the remarkable synergies and complementarities offered by the realistic combination of the long term ambition of a laboratory (a 100 TeV hadron collider FCC-hh), with a high-precision Higgs, Electroweak and top factory (the FCC-ee). That these two machines can fit exactly in the same tunnel is remarkable. The Financial and Technical Feasibility study of this staged project (ee first, then hh) is now undertaken with significant resources at CERN, and the studies are moving along. This is an opportunity that should not be missed.

So if asked "what should the US HEP community do?", the response is rather easy – they should participate, contribute, and take leading roles in the preparation of the FCC – this comprises many challenges addressed to a vast palette of skills (see [1]). Right now, the FCC is the solution that gives more physics for the money and the deepest perspective, and we should go for it. However, when participating in the Snowmass meetings, which should be praised for their openness, it could be clearly felt that there is a concern that the High Energy Frontier has left the US, and that working at CERN would become unescapable. This would be ignoring the extraordinary vitality, talent and creativity of the US particle physics and accelerator community. It is natural to believe that great inventions will continue to come out of the US academic world. For all its virtues, FCC will not satisfy all wishes that HEP will ever encounter. Additional technologies (high-efficiency circular colliders, ERL, linear collider, muon collider, in increasing order of energy) might at some point become of urgent nature. The study of the alternatives should continue, not only with the purpose of providing a plan B, should the FCC turn out not to be feasible, but perhaps more importantly to provide the required new tools should new physics opportunities show up along the way.

Physics will guide the choice of the machine that would best complement the FCC. As an example, one can develop the case of muon storage rings. The guiding principle of the FCC, synergy and complementarity, could be helpful here. Let's start with complementarity. What the FCC plan (ee then hh) does not, is to provide high energy (¿365 GeV) lepton collisions with a well defined center-of-mass. Muon colliders are the most efficient from the wall plug-power point of view, at least for center-of-mass energy exceeding about 2 TeV. Contrary to electrons, they do not suffer from beamstrahlung, and the center-of-mass energy spread remains small up to very high energies. A muon collider, in the standard scheme, starts with a high intensity proton source. Muons are spin-polarized at production in pion decay, and the spin precession in the ring provides a remarkable precision on the centre-of-mass energy and its spread. Should FCC-ee or FCC-hh provide evidence that a new particle exist in the 0.5-10 TeV region, a muon collider might be a tool of choice to study its properties, in a way similar to the e+e- factories being a necessary complement to the LHC discovery of the Higgs boson.

What about synergy? Fermilab, already host of the g-2 storage ring, will be investing in a high power proton (PIPII) machine able to serve the neutrino program (LBNF) and more, with significant international contributions. This is presently the flagship program. The world-wide long-baseline neutrino program would greatly benefit from the experimental knowledge of electron and muon (anti) neutrino cross-sections and especially their ratios. A small muon storage ring neutrino factory, with top energies ranging from a few hundred MeV up to several GeV, can provide this with sub-percent accuracy (the Nustorm project). Building, and operating such a facility around the clock, with the necessary control of beam parameters (e.g.intensity, beam sizes, energy and polarization) will constitute great hands-on experience and a first step towards the realization of a more ambitious high energy muon collider.

Other projects might offer similar possibilities. With enough creativity, and exploiting synergies and complementarities globally, one should be confident that the US can find a path back to the Energy Frontier.

- Comment # 19: We can explore elementary particle physics in many different ways. Still, I hope we all agree that the energy frontier, particularly colliders, provides an utterly transparent, decisive, and broad scope probe. One can conduct thousands of different searches covering the whole spectrum of physics accessible.

  However, the energy frontier is losing energy. Without a concrete future plan laid ahead, which we have some hope to see its operation and results, people are discouraged from exploring further, both theoretically and experimentally. We will lose the next generation of theorists and experimentalists who understand collider physics, accelerator physics, and the many facets of elementary particle physics. And maybe the field will cease to exist.

  Building a future collider project is admittedly a huge commitment, so we are planning and want to reach a good plan. The community needs to stay together with a good plan, which is exciting in physics (this is a very basic requirement, people cannot pretend their interests), and full steam ahead to execute. Snowmass is such an opportunity, and we should future realize the need and the danger of falling apart.

  In my opinion, if there is some EF collider project that we can make it through, we should! And at the same time, do a lot of R&Ds for future accelerator projects to ensure new opportunities have a chance to emerge.

- Comment #20:

  1. The next global accelerator should be an e+e- Higgs factory. The physics case for this machine is very strong and is established within the particle physics community. Several options for realization - linear and circular - are under consideration. Snowmass should recommend that an e+e- Higgs factory should be constructed to operate on a time scale relevant to young people now engaged in experimental particle physics.
  2. In order for the US to have leadership in the experimental program of a Higgs factory, Snowmass should recommend that the DOE and NSF establish a platform for detector R&D specific to this class of colliders. Research activities should cover the range of proposed accelerator facilities and should initially emphasize areas that are applicable across facilities.
  3. A number of accelerator options for a Higgs factory are now under consideration, using different accelerator technologies. The International Linear Collider in Japan, based on superconducting RF technology, is the most advanced of these, already having an engineering design and the engagement of governments. CERN is putting forward the Future Circular Collider-ee, based on a synchrotron with nanobeam focussing. Snowmass should signal US support for each of these initiatives stating, in both cases, that there should be a plan for timely realization of the collider.
  4. In view of the uncertainties both for ILC and FCC-ee, Snowmass should recommend planning toward a US-sited proposal for a Higgs factory. It is likely that cost will be a significant factor for a US proposal, and that the approach of minimal cost will be a compact high-gradient copper linear collider. The new C3 technology is the most promising approach along this lines; this technology also has applications to X-ray free electron lasers and medical accelerators that make it a capability important to the DOE. Snowmass should recommend funding of an R&D program on C3 technology that could lead to a Higgs factory technical design within this decade.