A Flexible Strategy for the Future of Particle Physics at CERN

Halina Abramowicz[3], Marcel Demarteau[10], Spencer Gessner[2], Jenny List[5], Donatella Lucchesi[8], Patrick Meade[6], Patric Muggli[4], Isobel Ojalvo[7], Simone Pagan Griso[1], J. Ritchie Patterson[11], Michael Peskin[2], and Andrea Wulzer[9]

[1]Lawrence Berkeley National Laboratory
[2]SLAC National Accelerator Laboratory
[3]Tel Aviv University
[4]Max-Planck-Institute for Physics, Munich
[5]DESY
[6]YITP Stony Brook
[7]Princeton University
[8]University of Padova and INFN Padova
[9]IFAE, Universitat Autònoma de Barcelona
[10]Oak Ridge National Laboratory
[11]CLASSE, Cornell University

May 16, 2025

Abstract

This document outlines a strategy to ensure CERN remains at the forefront of particle physics by addressing the most pressing questions of our field in a timely and effective manner. The strategy balances ambition with feasibility—financially, logistically, and environmentally—while ensuring a robust path to exploring fundamental interactions at energies far beyond those of the LHC.

This approach prioritizes rapid progress toward the 10 TeV frontier and beyond, while maintaining a seamless continuity in frontier-physics experiments to maximize scientific output and preserve expertise in experimental operation. The plan also recognizes the need for a next-generation collider with a rich physics program that engages the young scientists currently involved in the LHC era.

This requires a strategic compromise: an optimized near-term solution that is cost-effective yet scientifically compelling, leaving room for future accelerator innovations. The vision leverages decades of breakthroughs in accelerator technology, combining proven methods with new creative advancements to overcome the challenges ahead. By pursuing a flexible and forward-looking program, we aim to meet both the immediate and long-term needs of the global particle physics community in its search for a deeper understanding of nature.

This document outlines a strategy for the CERN laboratory to answer the most pressing questions of our field in the most timely manner. This strategy aims to be flexible, ambitious, viable both financially and in terms of human resources, and environmentally sustainable. It ensures a continuity in running frontier-physics experiments with minimal interruptions. It is meant to complement, and it is heavily based on, detailed plans being submitted to the European Strategy for Particle Physics (ESPP) process by authors of reports on the Linear Collider Vision [1], the Linear Collider Facility (LCF) at CERN [2], the International Muon Collider Collaboration (IMCC) [3], and the Plasma Wakefield Collider [4], the Cool Copper Collider [5], plus potentially others. The strategy is also consistent with the recent US P5 report [6], making it easier to attract broad international support.

The simple, yet powerful, core idea of this strategy is to build a short-term Higgs factory followed by a muon-based or wakefield-based 10 TeV center-of-mass energy lepton collider. This strategy offers well-defined branch points that will stimulate result-driven research. It ensures the ability to quickly adapt the program to new developments, including, most importantly, results from other experiments that might point to specific energy scales that would become a priority to explore. It addresses both the need for a solid, ambitious physics program studying the property of the Higgs boson and the need to explore the highest possible energy scales - or the most relevant ones - in a timely manner.

Motivation

The experimental results from the Large Hadron Collider have now established the Standard Model of particle physics as a description of the fundamental interactions of particle physics up to the energy scale of 1 TeV. On the one hand, this is a remarkable achievement; on the other hand, it leaves particle physicists in a quandary. The Standard Model is manifestly incomplete. It does not explain the hierarchy of masses of the elementary particles and, more generally, the structure of flavor interactions and the origin of CP violation. The Standard Model is based on spontaneous symmetry breaking, but it gives no clue as to the physical origin of that symmetry breaking. In the Standard Model, all of these questions are answered by couplings of the Higgs boson, but all of those couplings are inserted by hand with no apparent explanation. Another major gap in the Standard Model is its lack of any particle that could be a candidate for the dark matter of the universe.

To seek the answers to these questions, we need a major step in energy beyond that of the LHC. The 2019 update of the European Strategy for Particle Physics and the 2023 P5 report in the US emphasized the need for a higher energy collider that could achieve 10 TeV in the center of mass energy of parton collisions. It is important to remember that 10 TeV is not a specific benchmark but only an indication of the order of magnitude in energy that one would need to reach. The particle physics community needs a strategy that robustly meets this goal, with the possibility of higher-energy extensions. And, further, the program of this collider should not only be to reveal deviations from the Standard Model. It should be aimed at discovering and studying the new particles that explain the nature and couplings of the Higgs boson. In the best case, discoveries from other frontiers, from the HL-LHC, or from the Higgs program itself, might strongly suggest an energy scale in the multi-TeV regime that would be crucial to study with a collider to fully understand the underlying structure that these experiments partially reveal.

Today, we have no assured accelerator technology capable of reaching 10 TeV in the parton center of mass. Three different technologies aiming for this goal are being actively pursued. The first is to construct a tunnel to host a 100 TeV proton-proton collider and fill it with high-field dipole magnets. The second is to develop a technology for efficiently "cooling" muons, that is, decreasing the phase space of a cloud of muons by a factor 10^{-6} so that they can be injected into a high-energy collider. The third is to tame the enormous accelerating fields available from plasmas so that these can be the basis of a reproducible and robust linear accelerator. All three approaches are being pursued in

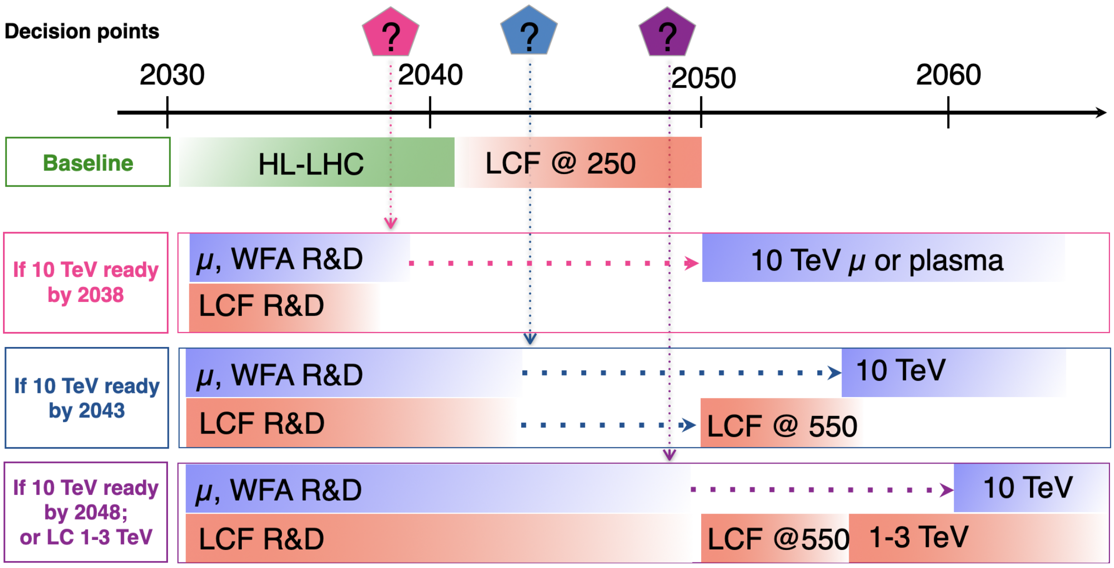

Figure 1: Sketch of the proposed strategy timeline, indicating possible branching points as pentagons; their outcome is shown as different lines above indicating possible paths.

Europe with substantial collaboration from US laboratories. All three approaches aim for significant progress within a decade, assuming that they will be adequately supported at levels significantly beyond what is currently provided.

In addition to all of the above, there is a need to ensure a solid program of running experiments able to produce world-best results and progress towards these goals with minor interruptions.

We need to develop a strategy addressing all of these goals.

Strategy outline and timeline

A rough sketch of the elements of this strategy is depicted in Figure 1.

A linear collider based on Superconducting Radio Frequency Cavities (SRFC) offers the most straightforward path to realize a Higgs factory by colliding (polarised) electron and positron beams with a center-of-mass energy of 250 GeV. It requires a site length of about 20.5 Km and would host one or potentially two interaction points and experiments. This first stage is detailed in the Linear Collider Facility concept submitted by the LCVision document [1]. Compared to that document, however, here we expect only the shorter tunnel to be excavated and to be sufficient for most scenarios presented. The maturity of this technology, well developed and tested for ILC, and the ability to construct it in parallel to a running HL-LHC project, would allow for a timely start of data-taking shortly after the end of the HL-LHC if this strategy is adopted promptly. The physics program of this first stage is expected to last about 8 years, following the "full-power" option outlined by LCVision, and would also include a short run at the Z pole.

An alternative first-stage based on alternative technologies, such as warm or cold copper acceleration, is also possible. Its implications are detailed within the LCVision and C^3 documents.

Such a program allows the community to devolve significant human and financial resources to a strong R&D program to be pursued in parallel, aiming primarily to reaching higher center-of-mass energies, including the realization of demonstrator facilities needed along the way.

The first junction point arrives at around 2038. At that stage, a competitive assessment of the readiness of a muon collider facility hosted on the CERN site (μC) and an upgrade of the linear accelerator complex to be based on wakefield acceleration (WFA) should be performed with the aim to reach energy scales around 10 TeV as quickly as possible.

If either of the two technologies are ready, a go-ahead would enable them to be "queued" to start right away after the physics program of the linear collider at 250 GeV mentioned above, around 2050. While this timeline is aggressive, it is believed to be technically feasible given sufficient R&D funding. It is consistent, for instance, with what outlined by the IMCC for the realization of a 10 TeV muon collider. If this high-energy collider is realized, this would ensure likely 10 to 15 years of data-taking at the energy frontier and the possibility of further energy and/or luminosity upgrades once its technologies are mastered. The huge breadth of the physics reach of such a program is well presented in the various documents referenced here. This would conclude this strategy. Either option would need relatively modest new infrastructure, e.g. a μC option might need "just" a new 11 Km tunnel and a WFA option an increase of the linear collider main tunnel length. In this scenario, the physics case motivating a higher-energy linear collider would be explored directly by the 10 TeV machine.

It might be possible, however, that neither of these technologies is ready for a green light by 2038, requiring more extensive R&D. In such a scenario, an energy upgrade for the linear collider facility can be executed. Such an upgrade could be based on either conventional SRFC, or - preferably - warm or cold copper technologies and would naturally aim for an energy of 550 GeV. This would tremendously extend the physics reach and would provide a 5 to 10 years robust physics program. In the case conventional SRFC can be avoided by using more advanced technologies, no additional excavation would be necessary. The physics program at 550 GeV and above includes qualitatively new Higgs measurements, including the direct precision measurements of the top quark Yukawa coupling and the Higgs self-coupling, as well as the search for Higgs and top quark compositeness through precision measurements of the top quark electroweak couplings.

In the situation above, a new decision point would be set after 5 years ($\approx$ 2043), when the readiness of the μC and WFA technologies will be re-assessed. Depending on the outcome, a further 5 years of running or queuing the technology that is ready earlier would provide the obvious path forward. A further decision point in an additional 5 years ($\approx$ 2048) fits naturally the 10 years physics program of a 550 GeV linear collider.

If at that point no technology is still ready, further energy upgrades are possible up to a few TeV of center-of-mass energy, depending on the technology available, and these might or might not require a longer tunnel.

A more extended linear collider program that considers multiple energies also fits naturally in case the 10 TeV technology is delayed. This is not shown for simplicity.

In addition to this baseline strategy, other options can be considered. For instance, a Higgs factory based on the LHC tunnel is also an option; it is attractive in terms of minimizing financial commitments and new infrastructure, but it has the drawback of lacking a logical upgrade path that makes the strategy above very flexible, and requires to finish the HL-LHC program and de-install LHC before the construction of a Higgs factory could even start. Luminosity would be limited and no beam polarisation available. Another option would be to complement an initial linear Higgs factory with a new pp collider, should technology and science point in that direction instead of a muon or plasma collider at any of the decision points.

Finally, this strategy can easily be adapted depending on specific needs that could arise rather quickly and dynamically if new physics hints become available. For instance, if a 2-3 TeV facility is needed, the community could review the available technologies and either upgrade the linear collider earlier to those energies or build a few-TeV WFA-based or muon collider.

Discussion of merits

There are several advantages in the strategy presented in the previous section.

Physics potential This must be the primary driving force for a healthy field that can continue to attract the most brilliant people. The strategy must ensure that the field is able to innovate and remain open to new ideas, results and unexpected events that can come at any time. In turn, this requirement can be broken down into two main aspects: flagship results and breadth of the physics program. The elements of this strategy, when combined, offer the best potential to investigate as quickly as possible critical key questions of our field and maintain a very broad physics exploration program.

Flexibility The outlined strategy offers maximal flexibility that aims to reach high-energies as quickly as possible, yet adapts to an evolving field and allows contingency options depending on the technology readiness to achieve high-energy colliders of the scale of 10 TeV, or more if needed. That is a core strength of this proposal. Several key elements that build such flexibility are apparent in the previous section.

Financial and Human Resources These large scale projects take a huge toll in terms of human and financial resources of the field. As such, ethically we should dedicate the needed resources only when their expected return to the overall scientific community is very high. The relatively low financial and human cost of the first phase of this strategy allows resources to evolve and focus towards 10 TeV collider technologies with the needed strength, while at the same time maintaining a running flagship facility at CERN.

Continuity This financially conservative strategy allows for continuity of physics without having a substantial gap between the end of the HL-LHC and the start of a Higgs factory. This enables retention of key personnel and the ability to grow the relevant workforce with new students.

Ambition The strategy aims to achieve the highest possible energies, while at the same time preserving key aspects of a low-energy Higgs factory program.

Feasibility The stages of this strategy are put together in such a way that the less-risky options come first. The later stages leave many options available to ensure the ultimate feasibility and realization of this plan.

Timing and Long-term perspective Of course a vibrant and talented community wants a lot, and asap. A timescale compatible with the career length of a person already demands much from its participants. A proposal that goes beyond this introduces aspects that will likely sap the motivation of scientists to engage with it. The above strategy prioritizes high-energy upgrades in the shortest possible time while ensuring a running facility with minimal interruptions. It also ensures that CERN will be at the center of particle physics research for the next decades. Because of its flexibility, it minimizes the risk that CERN will be forced to a option that is less than optimally relevant.

Sustainability While sensibility to this theme varies by country, it is obvious that as time evolves, the sustainability of our research will play a more and more important role in our lives. Given the timescale of these projects, this factor has to be taken into account as a huge risk factor. The options considered have all been carefully optimized to reduce carbon footprint in both construction and operation, as detailed in their submissions.

Training and Community engagement Particle physics has been and should continue to aim to be a huge source of training for young physicists, including and especially to those who then pursue jobs outside academia. Large-data analysis, very advanced technology and the ability to solve complex problems and stay on the forefront of technological innovations is of paramount importance to ultimately secure the level of funding that is needed to aggressively make progress in our understanding of fundamental physics. The presence at the same time of a running flagship facility and a vibrant and ambitious R&D program ensures the best and broadest possible training for the current and next generation of physicists.

Contingencies The large number of options available offer the flexibility to adapt to any contingency. A scenario worth mentioning is that of a large circular Higgs factory being built in China. In that situation, the strategy would adapt by prioritizing directly a 550 GeV linear collider and aiming for further upgrades to be decided at the junction points as before. A slight shift of the junction points and timeline, as well as an increased amount of financial resources might be inevitable in this situation, but it would still ensure a viable flagship path for CERN.

Conclusions

CERN and the global particle physics community need a robust path to explore the fundamental interactions at energies much higher than those of the LHC. At the same time, we need a path to a next collider with a rich physics program that can engage the young scientists now involved with the LHC program. The solution must be a compromise in which the least costly – but still highly motivated – near-term solution is chosen to provide space for the development of the accelerator technologies of the future.

This document outlines a strategy to maximize the potential of the CERN laboratory to remain at the forefront of particle physics. While many details are omitted, we feel we have given enough to clearly explain the core idea. A more detailed plan can be easily and quickly developed, if there is interest, taking into account more concretely constraints and interplay among options, thanks to the large amount of work done by the individual projects and detailed in their submissions.

Over 70 years of progress in the construction of high-energy particle accelerators, we have raised the capabilities of our experiments from the GeV to the TeV energy scale. This has been accomplished in part by the construction of larger accelerators, but mainly through creative ideas such as strong focusing, colliding beams, superconducting magnets, and particle beam cooling that have broken through apparent barriers to progress. In our quest for the next stage in high-energy physics, we will need that creativity again. Our plan for the next and future colliders can offer a rich program based on studies of the Higgs boson while at the same time opening opportunities for transformative advances in accelerator design. Only with such a flexible and open-ended program can we meet both the near- and long-term needs of our community in its search for a truly fundamental understanding of nature.

References

[1] Balazs, C. and others (Linear Collider Vision). A Linear Collider Vision for the Future of Particle Physics. arXiv:2503.19983, 2025.

[2] LCVision Team. The Linear Collider Facility (LCF) at CERN, EPPSU submission, abstract # 40. https://agenda.linearcollider.org/event/10624/.

[3] The International Muon Collider Collaboration. The Muon Collider Input for the European Strategy for Particle Physics. arXiv:2503.XXXXX (to appear), 2025.

[4] Spencer Gessner et al. Design Initiative for a 10 TeV pCM Wakefield Collider. arXiv:2503.20214, 2025.

[5] Emilio Nanni, Caterina Vernieri, et al. Esppu input: C^3 within the "linear collider vision". arXiv:2503.20829, 2025.

[6] Asai, Shoji and others (P5 Collaboration). Exploring the Quantum Universe: Pathways to Innovation and Discovery in Particle Physics. arXiv:2407.19176, 2023.