Swiss inputs to the 2026 update of the European Strategy for Particle Physics (ESPP)

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Abstract: This document summarizes the Swiss inputs to the 2026 update of the European Strategy for Particle Physics (ESPP), compiled by CHIPP, the Swiss Institute for Particle Physics. Building on the "CHIPP Community Roadmap 2024," which outlined national research infrastructure needs, a series of workshops were held to develop consensus on the strategy. The resulting responses were finalized and approved by the CHIPP board in March 2025.

March 2025

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The following represents the Swiss inputs to the 2026 update of the European Strategy for Particle Physics (ESPP) that were compiled by the Swiss Institute for Particle Physics (CHIPP) and submitted in March 2025.

CHIPP is the bottom-up organization of Swiss particle and astroparticle physics researchers in Switzerland, and it is a legal entity of Swiss law. CHIPP is tasked with coordinating national efforts in the realm of particle and astroparticle physics. CHIPP consists of two bodies: the CHIPP plenary, consisting of physicists in Swiss institutions with a postgraduate degree (PhD students, postdocs, senior scientists and professors) as well as Swiss nationals with a PhD working at CERN; and the CHIPP board, consisting of professors at Swiss institutions with activities in particle and/or astroparticle physics, as well as the heads of the experimental and theoretical particle physics groups at the Paul Scherer Institute (PSI), and additional members of the plenary with special functions. The CHIPP board elects an executive board consisting of a Chair and three Deputy Chairs.

CHIPP undertook a roadmap update process in January 2024, culminating in December 2024 with a roadmap published by the Swiss Academies of Science titled “CHIPP Community Roadmap 2024”, focusing on the needs for research infrastructures up to 2029-2032 and beyond. This roadmap forms the basis for coordinating research activities in our three research pillars: i) the high-energy, high-intensity, and precision frontier, ii) neutrino physics, and iii) astroparticle physics.

The 2024 roadmap formed a starting point for addressing the Swiss inputs to the ESPP process. In addition, a February 2025 Strategy workshop, with all members of the CHIPP plenary invited, was used to formulate consensus on the specific ESPP answers. CHIPP Early Career Researchers (ECRs) were active in the process, conducting their own workshop and contributing to the strategy discussion. The CHIPP executive board compiled the responses to the ESPP questions, with the CHIPP plenary providing a review. Finally, the CHIPP board approved the following responses in March 2025.

Questions to be considered by countries/regions when forming and submitting their “national input” to the ESPP:

3a. Which is the preferred next major/flagship collider project for CERN?

The Swiss Institute of Particle Physics (CHIPP) community strongly supports the Future Circular Collider (FCC) project as the next major facility at CERN, beginning with FCC-ee and followed by FCC-hh. CHIPP reaffirms that, with the information available at the time of writing, no other prospective facility can match the FCC's unparalleled combination of exceptional physics potential and long-term prospects for the field. This evaluation considers the financial aspects, the technological and human benefits, and the energy sustainability in relation to other projects. The CHIPP community underscores that the FCC represents a tremendous opportunity that Europe should embrace. Realizing the FCC is essential to secure the future of CERN and to ensure the long-term vitality of particle physics in Europe.

The CHIPP and CHART community, encompassing accelerator, experimental, and theoretical physicists, has made significant strides in advancing the FCC project. On the accelerator front, CHART (Swiss Accelerator Research and Technology), founded in 2016 as an umbrella collaboration for accelerator research and technology, is driving cutting-edge developments—including advancements in high-field magnets, advanced beam optics, and state-of-the-art cryogenic systems—that are critical for pushing the performance boundaries of future accelerators. On the experimental side, the recently launched CHEF (Swiss High-Energy research for the FCC) initiative represents a dedicated effort to support FCC projects in experiment and theory. Swiss researchers are involved in detector R&D, and the prospects of the FCC-ee are sparking innovative ideas that extend beyond previous concepts. Moreover, the theoretical precision required to leverage the FCC-ee's experimental capabilities is poised to revolutionize calculations, simulations, and predictions. With an impressive track record in higher-order computations for collider observables and in deciphering subtle signals of new physics, Swiss researchers are eager to tackle these challenges, and to develop new and further refine theoretical tools.

3b. What are the most important elements in the response to a)?

i. Physics potential:

The FCC's physics potential is unrivaled. With a precision physics program followed by high-energy exploration, it provides the most comprehensive, time-honored approach to addressing the fundamental questions of particle physics for decades to come.

The physics potential of the FCC-ee, with its multiple center-of-mass energy configurations, provides a rich and broad program of Higgs and top-quark physics, electroweak & QCD studies, flavor physics, rare decays, and beyond-Standard Model (BSM) searches for heavy neutral leptons, axion-like particles, and other feebly interacting/long-lived particles. It has become clear that independent measurements from Higgs, top, electroweak, and flavor studies provide complementary constraints on new physics beyond what is possible with other machines. Beyond the primary FCC-ee goal of detailed studies of the Higgs boson, it is important to highlight that the greater than $5*10^{12}$ Z bosons produced generates an immense wealth of b and c quarks, tau leptons, and access to never-before measured tiny branching ratios and extremely small couplings.

ii. Long-term perspective:

The FCC is a long-term ambition, encompassing diverse research areas—from fundamental physics to advanced technological developments—attracting interest from various disciplines and offering unparalleled opportunities for training future generations of researchers while inspiring the scientific community. It will catalyze extensive research innovation and foster engagement with the industrial sector, including adding value to innovation in this area. The comprehensive FCC program, including FCC-ee and FCC-hh, provides cutting-edge collider physics opportunities for the rest of the century.

iii. Financial resources:

When evaluating future projects at CERN, it is essential to consider construction costs, operational expenses, and the overall physics potential of the entire program. The staggered development of the FCC-ee and FCC-hh guarantees a long-term return on investment, making the FCC more sustainable than projects with single-use, short-term applications. When comparing the operational costs over the required years of operation, the FCC-ee emerges as a more cost-effective option than other proposals, as reported in arXiv:2412.13130.

The scale of modern facilities has grown tremendously, and the advent of the LHC has spurred an expanding global community. Numerous institutes and states now routinely seek to join experiments and collaborate with CERN, reflecting the LHC program's enduring inspiration, rewarding nature, and significant contributions to education and technology transfer. Although about 30% of young researchers remain directly engaged in particle physics over the long term, the training provided at CERN spans a diverse range of fields. The Swiss community maintains that sustaining this community is vital for human advancement and global economic progress. Any new facility that risks diminishing interest or reducing the community's size could jeopardize decades of effort and the immense potential for training future generations across multiple career paths.

iv & v. Timing & careers & training:

Ensuring the community's robustness is a top priority, and making a prompt decision is essential. In particular, Swiss early-career scientists argue that establishing and approving a clear, timely plan for a future flagship facility for particle physics is far more critical than minimizing the gaps between operational facilities.

The vast amount of data produced by the HL-LHC will require several years to analyze fully, and legacy measurements are expected to continue for 5–10 years after its conclusion. In tandem with FCC development, this extended data analysis period will provide early-career researchers with a wealth of opportunities. While it is ideal for the FCC to commence soon after the HL-LHC, some flexibility is warranted to secure a comprehensive funding model. The Swiss community is deeply invested in the HL-

LHC—particularly in ATLAS, CMS, and LHCb—and is committed to ensuring that the full potential of the HL-LHC is realized. To address any potential FCC-ee funding shortfall, potential levers within the HL-LHC and FCC-ee schedules should be investigated and balanced concerning the overall physics deliverables of these programs.

vi. Sustainability:

For Switzerland as a host state, sustainability is of vital importance. This is particularly important when communicating to the general public. Efforts to minimize both the environmental impact of construction and operation are essential. It has been reported arXiv: 2412.13130 that the energy costs of the FCC-ee are much smaller than other options for the same physics potential. CO_2 emissions are, therefore, proportionally smaller. Comparing options at CERN to other projects elsewhere in the world, CERN benefits from having a significant fraction of its electricity generated by nuclear power plants, which have a much smaller carbon footprint. While we do not yet have all the information to assess all projects with the same level of detail, we plan to evaluate these project inputs carefully.

In summary, we believe the FCC project—when considered in its full scope—is the most cost-effective and sustainable option. It offers an exceptionally broad research program, supports the largest scientific community, and ensures that investments will yield benefits over many decades.

3c. Should CERN/Europe proceed with the preferred option set out in 3a) or should alternative options be considered:

i. if Japan proceeds with the ILC in a timely way?

CERN's flagship should remain the FCC. Exploring complementary scenarios that allow both programs to mutually benefit and maximize scientific returns would be essential.

ii. if China proceeds with the CEPC on the announced timescale?

CERN's flagship should remain the FCC. Even if the Circular Electron-Positron Collider (CEPC) comes online years earlier, investing in the FCC remains crucial. The CEPC cannot accommodate the current CERN community or deliver the training and technology transfer scale that the FCC can offer to Europe. Moreover, the FCC—or any new CERN flagship—is built on decades of international experience and effort, providing a significant advantage over the new CEPC endeavor.

The flexibility of center-of-mass energies of the FCC-ee can allow it also to be complementary to the CEPC in terms of the physics it explores.

Should the CEPC become a reality, China's plan envisions the SppC operating by 2055 to achieve 70-100 TeV collisions. It is essential to factor this into CERN's roadmap and allocate the necessary resources to continue advancing FCC-hh developments.

iii. if the US proceeds with a muon collider?

CERN's flagship should remain the FCC. A muon collider option is an exciting possibility that should continue to be developed as a demonstrator, as the technology needed to realize it has not yet been established.

iv. if there are major new (unexpected) results from the HL-LHC or other HEP experiments?

New, unexpected results from the HL-LHC or other HEP experiments would be a tremendous breakthrough for particle physics and would awaken public awareness and breed new scientists eager to elucidate the discoveries. This would require a new look at the CERN strategy but depends on the nature of the results. Hints of new physics from the HL-LHC may require the more precise FCC-ee to elucidate. Should the HL-LHC observe new states that are best measured at the HL-LHC and require more running time, this can be accommodated by running the HL-LHC longer and then studying the precise effects of this physics with the FCC-ee. To study such new states in more detail, one may consider going directly to an FCC-hh with optimized center-of-mass energies and luminosities. Such a shift from FCC-ee to a more expensive and technically difficult FCC-hh would potentially be possible with increased public interest and funding. However, the challenges of demonstrating capable detector instrumentation and analysis studies to achieve precision physics goals with an FCC-hh should not be underestimated. This shift would be facilitated by then-ongoing tunnel construction for the FCC-ee, thus allowing the FCC program to react much more flexibly to new physics developments than any of its competing projects. If new physics becomes apparent in other experiments besides at the HL-LHC, a particle physics strategy update should also be developed to understand the implications of the new physics for colliders, as well as other dedicated experiments to examine the physics. However, it must be emphasized that all these scenarios are hypothetical and cannot be given significant weight in the planning of the next collider project.

3d. Beyond the preferred option in 3a), what other accelerator R&D topics (e.g. high-field magnets, RF technology, alternative accelerators/colliders) should be pursued in parallel?

Magnets based on high-temperature superconductors reduce energy costs substantially and would be a tremendous development for FCC-hh.

Clearly, high-field magnets bring tremendous benefit to the center-of-mass energy that a future FCC-hh could reach.

As mentioned, the CHART organization is active in these areas.

3e. What is the prioritized list of alternative options if the preferred option set out in 3a) is not feasible (due to cost, timing, international developments, or for other reasons)?

The Swiss community does not see an alternative option to the FCC program that provides the broad physics program, technical achievability, timeline, and maintenance of the physics community. Based on the available information, the FCC program outperforms any alternative option regarding these aspects.

The Swiss community is not providing an alternative option; and it emphasizes that no alternative scenario exists at this stage that wouldn't compromise one or more critical aspects.

All alternative options fall short compared to the FCC-ee. They lack the sufficiently compelling and comprehensive physics program required to sustain the interest of the global particle physics community and inspire the next generation of scientists. We would also forgo the unique synergy the FCC-ee offers: the unprecedented precision measurements that can tightly constrain new physics scenarios, laying the groundwork for a subsequent phase of direct discovery at the FCC-hh, which would operate at the highest achievable center-of-mass energies.

Delaying a commitment to the FCC-ee in favor of exploring other possibilities amounts to merely “kicking the can down the road”—potentially to the following European Strategy Update or even the next generation. Such indecision threatens the community's cohesion and the field's continuity.

From a cost perspective, the FCC project represents the most cost-effective and sustainable path forward. It enables a physics program of extraordinary breadth, capable of supporting the largest possible research community while ensuring a long-term return on investment. Furthermore, flexibility in HL-LHC operation and shutdown schedules, as well as a slight delay in the start of the FCC-ee, could help alleviate any potential FCC-ee funding shortfalls.

3f. What are the most important elements in the response to 3e)? (The set of considerations in 3b should be used).

Based on the information currently available, the reasons we do not offer alternative options are as follows:

i) **Physics potential.** While the International Linear Collider (ILC) and the future Large Electron-Positron Collider (LEP3) might offer competitive programs for Higgs physics, some of the electroweak studies and rare processes, their lower luminosities prevent them from achieving the full potential of the FCC-ee in many crucial measurements and indirect searches. Similarly, the HE-LHC and a lower-energy FCC-hh option would miss opportunities for key SM measurements at the precision frontier. Moreover, a modest energy increase relative to the LHC is not well-motivated without clear insights into the energy scale of new physics—a determination that the FCC-ee could provide. Upgrading to the High-Energy LHC (HE-LHC) would make it difficult to later justify only a small incremental energy boost for a machine like the FCC-hh, thereby jeopardizing the long-term program. In contrast, a lower-energy FCC-hh would limit and delay the prospects compared to the default FCC-hh design. Although the Large Hadron-Electron Collider (LHeC) offers a complementary program, it would not be able to replace the comprehensive richness and potential of the full FCC program.

ii) **Long term perspective.** The FCC's two-stage approach—starting with FCC-ee and progressing to FCC-hh—embodies the ambition necessary for sustained long-term engagement and investment. In contrast, the ILC, LEP3, LHeC and HE-LHC options do not provide a new, longer tunnel required for FCC-hh and fail to offer a clear pathway to the high-energy collider.

iii) **Financial Resources.** While the ILC, LEP3, LHeC, and HE-LHC might offer lower initial construction costs, they do not provide the advantage of a new tunnel. Consequently, these options would require a significantly larger investment later, likely delaying the initiation of the FCC-hh program. For example, the HE-LHC would still demand costly magnets, which may be challenging to deliver on schedule, potentially creating a significant gap between the end of the HL-LHC and the next collider. Similarly, the LEP3 design, with its smaller radius, would incur higher synchrotron radiation power, resulting in increased operational costs and difficulties in reaching the necessary luminosities at higher center-of-mass energies. Moreover, lower luminosity would mean longer operational periods, further escalating electricity costs to achieve comparable physics output. The FCC-hh option entails higher upfront expenses—owing to extensive R&D for magnet development and the construction of a new tunnel and accelerator—but incorporating the FCC-ee stage helps to amortize these investments over time, while also providing a highly synergistic physics program.

iv & v) **Timing, Careers, and Training.** Even with two experiments, the ILC serves only a limited segment of the community, and the size of the experiments at LEP3, HE-LHC, or LHeC is uncertain to us. Moreover, a significant time gap between the HL-LHC and the start of the FCC-hh would pose serious challenges in sustaining community engagement and recruiting new early-career researchers. A gap of 10 years between the HL-LHC and the next facility is considered excessively long, potentially jeopardizing decades of investment and hindering the community's growth.

vi) **Sustainability.** Sustainability is paramount for Switzerland as a host state, making the minimization of the environmental impact during construction and operation essential. According to arXiv:2412.13130, the FCC-ee has significantly lower integrated energy costs than other options for a similar physics potential.

We consider the muon collider less mature than the other options mentioned above. We also assume that the above considerations applied to the ILC also hold for any linear e+e- collider at CERN, such as CLIC.

4) The remit given to the ESG also specifies that "The Strategy update should also indicate areas of priority for exploration complementary to colliders and for other experiments to be considered at CERN and at other laboratories in Europe, as well as for participation in projects outside Europe." It would thus be most useful if the national inputs explicitly included the preferred prioritization for non-collider projects. Specific questions to address:

a. What other areas of physics should be pursued, and with what relative priority?

While the primary focus of this ESPP update is to consider the future program at CERN, this question invites us also to put forth our other recommendations for priority areas of exploration beyond the CERN program.

Through its roadmap process, the CHIPP community has identified flagship projects and recommendations in each of the three pillars (defined in the introduction).

Pillar one encompasses high-energy and low-energy/high-intensity accelerator physics experiments that we conduct at CERN and PSI.

Regarding CERN, we also support the present and future exploitation of the CERN accelerator complex beyond the primary LHC experiments. This includes experiments that enable precision tests of the SM and searches for BSM physics using novel approaches. We recommend prioritizing the upgrades to NA64 and FASER and the construction of the SHiP experiment. Future facilities that further enhance the scientific reach of the HL-LHC should be given serious consideration. In particular, the Forward Physics Facility presents a compelling opportunity to host upgrades of existing experiments, such as FASER, but also to enable new physics searches and the unique study of neutrinos at TeV-scale energies.

Concerning PSI, we support the present and future exploitation of the High-Intensity Proton Accelerator (HIPA) complex at PSI, with its unique intensities of low-energy pions, muons, and ultracold neutrons. We recommend that a portfolio of dedicated experiments with a unique reach (as e.g. n2EDM, MEG II, Mu3e, MuEDM, PIONEER) at the low-energy/high-intensity frontier should be pursued and strong support of the High-

Intensity Muon Beam (HIMB) program at PSI be allocated. Mu3e is considered a flagship experiment of the CHIPP program.

Pillar two encompasses neutrino physics. We recommend with highest priority support of the long-baseline neutrino programs of DUNE and Hyper-K to maximize scientific reach. Complementary neutrino studies including the search for neutrino-less double beta decay with LEGEND-1000 and multi-messenger studies with IceCube are also recommended with lower priority.

Pillar three encompasses astro-particle physics. Our highest priorities are the CTA Observatory, which explores cosmic accelerators with multi-messenger astrophysics, and XLZD, the next-generation multi-tonne dark matter search facility. In addition, smaller-scale experiments with unique reach to low-mass dark matter, such as DAMIC-M and TESSERACT, are recommended with lower priority. The Einstein Telescope is expected to become a high-priority observatory within the next four years, due to its importance for multi-messenger science and its unique sensitivity to various dark matter candidates.

b. What are the most important elements in the response to 4a)? (The set of considerations in 3b should be used).

i) Physics potential

The quest for new physics at high energy is complemented by a diverse set of accelerator-based experimental activities at lower energies with precision obtained using high intensities. These activities are supported by dedicated accelerators, either at the national laboratory (PSI) or at CERN, and by using the SPS and the AD facilities, running in parallel with the LHC. These experimental efforts are avenues towards high-precision SM tests and exploring intriguing BSM scenarios with unique reach and, therefore, have a high physics potential for discoveries.

In neutrino physics, measurements of CP violation and the nature and ordering of neutrino masses drive our project recommendations.

In astroparticle physics, understanding the nature of dark matter and relating particle physics to cosmology are primary drivers.

ii) Long-term perspective

NA64 and FASER are running experiments, which should undergo an upgrade during LS3 to retain their leading role in searching for dark sectors. SHiP has been approved at CERN and will start construction during LS3, with operations in the 2030s.

PSI will continue to provide the highest intensities of pions, muons, and ultracold neutrons for experiments with stopped or trapped particles. Performance-improving consolidation and upgrading the infrastructure of and around HIPA are essential activities. HIMB will provide new opportunities for low-energy, high-intensity frontier experiments into the 2050s, and a physics program well into the 2030s is already foreseen.

Hyper-K, DUNE, and LEGEND-1000 will continue well into the 2030s in neutrino physics. The XLZD and CTA astroparticle physics experiments are expected to operate well into the 2030s. ET is expected to begin operations in the late 2030s and run for 50 years.

iii) Financial and human resources: requirements and effect on other projects

Project financing from the SNSF and Swiss institutions is largely commensurate with community size. The successful implementation of the recommended program would capitalize on previous and foreseen investments in these experiments.

About 70% of the CHIPP community is engaged in high-energy, low-energy, high-intensity energy experiments at CERN and PSI. 50% of CHIPP is involved in the collider projects (CMS, ATLAS, LHCb) at CERN.

Beyond colliders, 10% of CHIPP is involved in non-collider experiments at CERN, including SHIP, GBAR, NA64, and FASER; 10% is involved in PSI experiments, including n2EDM, PIONEER, and Mu3; 15% is involved in neutrino experiments, including Hyper-K, DUNE, and LEGEND; and 18% is involved in astroparticle experiments, including CTA, XLZD, and ET.

The PSI HIMB upgrade was approved by Swiss government as part of the IMPACT project (Isotope and Muon Production using Advanced Cyclotron and Target technologies) and is proceeding, attracting both national and international collaborators with its unique capabilities.

iv) Timing

Regarding CERN, LS3 provides the opportunity to perform upgrades of NA64 and FASER and prepare the SHiP beam dump facility.

At PSI, The HiMB facility upgrade is foreseen during a prolonged HIPA shutdown from the end of 2026 until the middle of 2028. At this point, Mu3e will be able to capitalize on the increased muon statistics.

In neutrino physics, Hyper-K is foreseen to finish construction and begin operations in 2027, with potential upgrades around 2029. DUNE phase-1 will finish construction around 2031, with proposed phase-2 upgrades completed by 2033.

In astroparticle physics, XLZD is expected to begin construction around 2027, while previous experiments of XENONnT and LZ continue operating. CTAO will finish construction around 2027. ET site selection is expected in the mid-late 2020s, and construction will follow shortly thereafter.

v) Careers and training

NA64, FASER, and experiments at PSI, such as Mu3e are smaller-scale experiments that provide a great opportunity to train a new generation for searches for a Dark Sector with SHiP and later with FCC-ee.

The neutrino and astrophysics program comprises both large experiments, which offer long-term career potential, and smaller experiments, in which young scientists tend to engage in more aspects of the work.

vi) Sustainability

By exploiting existing infrastructure with only minor upgrades, we minimize environmental impact, reduce resource consumption and operational costs, and maximize efficiency and long-term viability.

c. To what extent should CERN participate in nuclear physics, astroparticle physics, (and neutrino), or other areas of science, while keeping in mind and adhering to the CERN Convention? Please use the current level and form of activity as the baseline for comparisons.

CERN pursues a diverse nuclear and particle physics program, e.g., at ELENA and ISOLDE, and considerable synergies exist with the low-energy program at PSI.

CERN should continue to support a broad range of low-energy initiatives that incorporate aspects of nuclear physics.

Such a commitment not only enriches CERN's expertise but also strengthens its role as a provider to other scientific communities that can benefit from CERN's technical know-how and administrative infrastructure.

We advocate organizing existing efforts in astroparticle physics through a common interface. The current neutrino platform could serve as the seed for an astroparticle platform that brings together the disparate activities underway at CERN under a clear umbrella. It is noted that many astroparticle physics experiments are supported as CERN recognized experiments, but it is often not clear what support CERN provides, nor what possibilities exist for shared activities. A centralized effort along these lines could raise the recognition and awareness of CERN's existing activities in astroparticle physics at minimal additional cost, while also providing the astroparticle community with a clear framework to propose future collaborations with CERN.

Regarding the neutrino experiments currently in preparation, we encourage CERN to consider partnerships with Japan and remain open to opportunities for collaboration and support for Hyper-K. As with DUNE, Hyper-K holds strategic relevance for CERN's engagement with the international community and should be considered within its long-term plans.