UPPSALA
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ECFA
European Committee for Future Accelerators

European Strategy for Particle Physics: input from Uppsala University

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Current activities at UU

- High-energy physics program:
  - Experimental collider physics: ATLAS (data-analyses, detector operation and upgrade), FCC-ee physics prospects.
  - Astroparticle physics: IceCube, ARIANNA, RNO-G.
  - Theory: BSM Higgs physics, high-energy astroparticle and neutrino physics, quantum chromodynamics, electroweak phase transitions and gravitational waves, etc.
- Nuclear physics program:
  - Hadron structure: Belle II, PANDA, BES III.
  - Flavour physics: Belle II, LHCb.
  - Other activities: hadron theory (structure & flavour), NNBAR/HIBEAM.
- FREIA Laboratory:
  - Neutrino long-baseline infrastructure: ESSnuSB.
  - RF cavity and amplifier tests for ESS, CLIC, AWAKE, MYRRHA.
  - Superconducting magnets, cryostats and magnetic-field probes for HL-LHC.
- Other scientific areas of interest in Uppsala: SHIP, PTOLEMY, Hyper-Kamiokande, REDTOP, CEvNS, etc.

ESPP input from UU

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

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We support the baseline scenario, i.e. an e^+e^- Higgs factory (FCC-ee) followed by a high-energy hadron collider (FCC-hh), as proposed at the previous ESPP update.

In the absence of any new physics at the LHC, the priority of the high-energy collider physics community should be to perform high-precision measurements in the electroweak and Higgs sectors as soon as possible, while planning the future facility to explore the high-energy frontier on the longer term.

ESPP input from UU

Question 3b – Most important elements in the response to 3a?

(i) Physics potential
(ii) Long-term perspective
(iii) Financial and human resources: requirements and effect on other projects
(iv) Timing
(v) Careers and training
(vi) Sustainability

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While we have not ranked these six elements, we provide some reflections on each of them in preparation to the national input.

ESPP input from UU

Physics potential (1)

- Accurate electroweak measurements, close interplay with the Higgs sector.
- Much more precise measurements of the properties of the Higgs boson than those expected to be achieved from the legacy results of HL-LHC.
- Searches for new physics are also possible:
  - directly: e.g. low-mass states, feebly-interacting and long-lived particles, exotic Higgs boson decays, etc.
  - indirectly: EFTs with better accuracy than at HL-LHC thanks to a clean environment.
- Local expertise in the study of soft colour and colour interference effects in both initial and final state evolution and fragmentation + assessment of sub-leading colour effects in the hard scattering.
  - Ideal to have an e^+e^- machine first (where to isolate such effects in distributions and model these appropriately in a clean environment) followed by a hadron machine (where this colour dynamics would manifest to affect cross-sections too).

Physics potential (2)

We must be aware of some limitations in the FCC-ee physics program though:

- Running a lepton collider at fixed energies may limit the potential of direct BSM searches, however ISR emission provides a low-energy tail for some BSM searches.
- The FCC-ee running baseline cannot reach the ttH threshold. Running at that energy would allow to probe the top-Higgs interaction with precision. At FCC-ee, the top Yukawa coupling will then be measured indirectly.
- Similarly, FCC-ee will remain below the 500 GeV threshold for direct HH production. Precision measurements of the Higgs self-coupling at FCC-ee can only proceed indirectly through accurately determining electroweak corrections to single Higgs boson production.
  - Is the advertised predicted precision of 20-30% really competitive against HL-LHC? It is 50% per experiment right now, but will likely get better.
  - What are the theoretical uncertainties on electroweak corrections and will they be a limiting factor in measuring the Higgs self-coupling?

ESPP input from UU

Long-term perspective

- We need to keep the field alive and ensure continuity, we do not want to risk sidelining a whole community like what happened in the USA once Tevatron was closed.
- Hence we need a collider at CERN shortly after HL-LHC. Not building the FCC means that we will lose the expertise and we may even lose know-how for building linear colliders or muon colliders.
- Going for ILC or CLIC instead of FCC-ee at CERN likely means that there is no immediate perspective for upgrading to a hadron machine in the 100 TeV range.

ESPP input from UU

Financial and human resources: requirements and effect on other projects

- Going forward with FCC-ee should not jeopardise R&D for muon colliders. It is obvious that R&D for high-field magnets should be supported as part of the baseline towards FCC-hh.
- Including forward physics and similar satellite programs in the FCC baseline is important to keep that community alive (forward physics at colliders can be informative for neutrino physics).

Timing

- We need a collider at CERN shortly after HL-LHC, and this needs to be approved within at most 5 years from now.

ESPP input from UU

Careers and training

- We want young people in the community to have long-term career prospects, hence a flagship project at CERN in order to guarantee faculty positions for our current and future PhD students. This is needed to revitalise the field.

Sustainability

- Obviously important and the FCC project should be planned with that in mind, nevertheless with the risk of increasing its cost. Still, sustainability principles should be in the guidelines and technical choices.
- FCC-ee followed by FCC-hh in Europe is sustainable because only one tunnel is built, with respect to digging several tunnels in parallel. Also, sustainability is likely to be worse elsewhere.

ESPP input from UU

Question 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?
(ii) if China proceeds with the CEPC on the announced timescale?
(iii) if the US proceeds with a muon collider?
(iv) if there are major new (unexpected) results from the HL-LHC or other HEP experiments?

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We believe that this scenario is unlikely. Compared to a linear e^+e^- collider, FCC-ee would still win on the luminosity reach. If one wants to reach higher energies (500 GeV and above), then CLIC is the best way to go, not ILC. We fear however that CLIC is technologically "risky": the two-beam acceleration technique with high enough gradient would need to be demonstrated on large distances.

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Even if China decides to proceed, we should not consider that FCC-ee has lost the competition because CERN has the infrastructure to successfully complete the project.

History has shown that such large projects carried out from scratch by a single country come with a significant risk of failure (e.g. SSC).

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If e^+e^- physics eventually proceeds outside Europe, CERN should definitely retain its expertise in accelerator and technology development, by having a flagship project, which does not necessarily mean a large tunnel:

- High-energy LHC (high-field magnets, same tunnel): how much time without colliding physics this would imply, physics potential?
- Energy recovery linacs → ep collisions right after HL-LHC to bridge the gap towards another flagship project at CERN... Physics case and attractiveness?
- Regardless, R&D on high-field magnets must continue in any scenario.

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A muon collider would be a great scientific opportunity: such a machine can perform precision measurements, while reaching higher energies than any e^+e^- collider. There are significant technical challenges to overcome and the project is unlikely to be ready shortly after HL-LHC.

A muon collider should not affect the plan for FCC-ee followed by FCC-hh!

R&D for muon colliders should continue and intensify. But, if resources are too thin, CERN should prioritise high-field magnets, while supporting muon collider R&D (and other accelerator development) elsewhere.

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The short- to mid-term appearance of new physics results is very speculative. We would need to calibrate our physics program based on such BSM physics. Still, it seems that the (flexible) baseline of FCC remains viable, meanwhile one should invest more on muon colliders, if this helps understand and probe such new physics.

Depending on the new type of physics, smaller-scale projects may also be suitable. Guidance from the theory community to understand the origin of new physics is instrumental before embarking on constructing new facilities.

EESPP input from UU

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No consensus on the prioritisation of other projects was reached at UU → to be discussed at national level.

Regardless, any project that is not the FCC should come very soon after HL-LHC. A long gap before the next flagship project at CERN is likely to negatively affect our field. The total gain of any project must be considered: science, technology, timing, sustainability, etc.