

Portuguese contribution to the update of the European Strategy for Particle Physics

Based on:

SUPPORTING NOTE FOR BRIEFING BOOK 2020

CERN/ESG/05

29 September 2019

1. In the absence of clear indications for new physics, is a broad exploration an adequate approach for our global field? Do we want to move forward in the largest variety of directions?

Portuguese input:

Physics programme:

Our current understanding leaves many questions unanswered, both in terms of theoretical consistency, explanation of well-established phenomena in astrophysics (dark matter and energy) and cosmology (baryogenesis), and a series of several intriguing measurements in many fields, from collider to neutrino physics, as well as observations of cosmic rays. Given the uncertainty of where clues for the next breakthrough may be found, a broad exploitation is certainly a good strategy, including non-collider neutrino, low energy precision measurements, fixed target and astroparticle experiments, but pushing the energy frontier at colliders remains a central way to push the field forward.

This can be done in two ways: (1) precision measurements of the Standard Model parameters, in particular those related to the Higgs boson, which are also a powerful tool to test low energy effects of a possible new theory at a higher energy scale; and (2) direct searches for new physics phenomena exploring as much phase space as possible, in particular in the energy frontier. The two approaches are complementary and should be pursued.

Financial:

Limited resources imply the need to define priorities for particle physics in the next decades. A broad exploration should still be possible, given the fact that non-collider experiments are significantly less expensive.

Community support:

Frontier machines capable of inspiring new generations and keeping the ambition to explore the highest-energy domain must be the priority. At the same time, in a field where the experimental life cycle is necessarily counted in decades, it takes a long time

to build up a body of knowledge that can be the basis for future breakthroughs. And a short time to lose it, if the conditions to pursue a line of research are not there. A diverse community is the repository of this knowledge, and must have the tools to progress in its various domains.

2. Would it be appropriate/sufficient to move the scientific diversity program to among the highest priorities for Europe? Should the strategy engage in ranking proposals according to priority? Which are the key proposals?

Portuguese input:

As mentioned above, we believe that a machine capable of exploring the energy frontier is essential to the future of this field; but that its physics programme should be complemented by a diverse research landscape.

An ordered list of proposals should be arrived at, in the short to medium timescale. Proposals should always be ranked according to physics potential, and that should be the leading element for determining priority.

We believe that the FCC programme presents the most promising, and flexible way to achieve the right conditions for the exploration of the energy and luminosity frontiers. It has significant advantages with respect to linear collider options, as outlined below. Within the two scenarios where the FCC would be in operation in the 2040-2060 period, we believe that the “FCC-all”, starting with the FCC-ee option, should get the highest priority; with the “LE-to-HE-FCC-h/e/A” scenario to be considered only as an alternative if, in a few years, it should be found to be viable and beneficial, which is not the case now, in our opinion.

Our reasoning is the following:

Physics programme:

- (1) While at the HL-LHC (~2027-2037) the Higgs couplings will be measured with a precision typically 5-10 times better than today, this may not suffice to probe the SM at the level required to reveal new phenomena. A new electron-positron collider at CERN would lead to a significant improvement, probing most of the Higgs couplings below the percent level, achieving a precise and assumption-free measurement of the Higgs total width;
- (2) The FCC-ee can be operated as a very high luminosity vector boson (Z, W pairs) factory and optimized as a 240 to 365 GeV Higgs factory, with a luminosity about ten times higher than that of a linear collider (CLIC or ILC). The higher luminosity

is a substantial advantage when precision is the main goal. It also covers top physics up to 365 GeV centre-of-mass energy. The ability to perform a comprehensive program of electroweak measurements at the highest luminosity in an extended energy range is a major advantage of the FCC ee option;

- (3) The FCC-ee circular collider does not require the same level of extreme performance in terms of beam size and stability of beam elements as a linear collider, or the extreme high dipole fields needed by hadron colliders. There is also worldwide much larger experience in operating circular colliders than linear colliders. This makes the FCC-ee a technological safe option which could deliver the desired luminosity shortly after the start of operation;
- (4) The several interaction regions of FCC-ee offer stimulating competition in the physics exploitation, and provides the essential possibility to independently confirm observations. This may prove to be a decisive factor in the case of observing deviations relative to the SM predictions. On the contrary, it would be a major challenge to have more than one experiment operating in a linear collider;
- (5) With the available information, the FCC-all scenario with the FCC-ee as a first stage seems a better option than the evolution from LE-to-HE FCC-hh. The latter scenario is not yet fully studied, but the initial understanding is that the cost would imply a larger financial effort than other proposals, while the gain of a factor 2-3 with respect to the LHC, and similar luminosity, can only marginally increase the precision of electroweak measurements. On the other hand the prospect of finding new particles is uncertain.
- (6) The FCC-ee is also an investment in the future for a later FCC-hh stage as a 100 TeV hadron collider. Just as it was for LEP/LHC, a circular collider offers a unique advantage of conversion into a hadron collider as the next logical option. At the energy frontier of 100 TeV, more than seven times LHC and far above the electro-weak scale, discoveries can be expected. Moreover, the huge production rate of interesting objects, such as Higgs bosons and Higgs boson pairs, will open the way to key measurements, e.g. a conclusive determination of the Higgs boson self-coupling.

Another key proposal would be the neutrino platform (NP) programme at CERN. Neutrino physics continues to have the potential for several discoveries and should also be a high priority in the next decades. The NP at CERN strengthens the capability of the European community to play an important role in the field worldwide, following the 2013 recommendations.

The above should be complemented by fixed-target experiments capable of exploring difficult to explore QCD regimes or hidden sectors of new physics.

The CERN Recognized Experiment mechanism should be pursued and strengthened whenever possible. It is and will be an important support factor to the Astroparticle experiments and community.

Financial and feasibility:

With its existing infrastructures and established expertise, we believe that CERN offers the best conditions for the success of the next large collider project.

Also, even the largest neutrino and fixed-target experiments are still one order of magnitude less costly than a frontier collider. And so it should be possible to maintain both sides of a diverse program, while still avoiding the risk of too much dilution of efforts.

Community support:

We believe that, while this may yet need further study, an ordered list of proposals should be arrived at, in a short to medium timescale; if possible, already for the current strategy update. This would avoid dispersion of means and energies, and would give clear goals to the community.

CERN and ECFA, with common physics and development programmes informed by the European strategy document, are the anchors in Europe for the particle physics community. While particle physics is a global endeavour, this anchor is still essential. This is especially in many smaller countries, which rely on the political clout of CERN to maintain a vibrant and international research programme, that would otherwise struggle to gain attention and funds from stretched research budgets.

3. Should we consider statements to strengthen the LHC and HL-LHC program? Should we stimulate the creation of coordinated programs at CERN and/or in Europe, e.g. AI@LHC for both data analysis and for control of instruments, etc?

Portuguese input:

Yes, we should consider statements to strengthen the LHC and HL-LHC program. A major step in consolidating and justifying post-LHC projects is the success and full exploration of the LHC. Coordinated thematic projects not only augment the physics potential of LHC data, but also serve as bridges to close-by communities.

4. Should we also support the fixed-target projects at (HL-)LHC?

Portuguese input:

Yes, as mentioned above. But with financial commitments that do not compromise the (HL-)LHC project itself or the development of future projects.

Physics:

Collider experiments do not cover for example the vast parameter space that hidden sectors can occupy. Beam-dump experiments have potential to detect new sub-GeV particles, a region still largely unexplored but favoured by many recent models to explain neutrino masses and oscillations, dark matter, baryonic asymmetry of the Universe and inflation. Fixed target experiments can also contribute with unique precision measurements of standard model parameters.

Financial and feasibility:

The most costly fixed target experiments would cost a fraction of the total FCC cost, and are expected to produce results on a 10-year timescale.

5. Because of the competition for the Interaction Region at Point-2@LHC, should we consider for the period beyond LS4 a choice between the next generation heavy-ion experiments at the HL-LHC and the LHeC?

Portuguese input:

A strongly motivated heavy-ion programme relying on detectors on other collision points (ATLAS, CMS, LHCb) focusing on collisions of nuclei lighter than Pb can take place. Physics cases for a next-generation detector at Point 2 and an LHeC should be compared in their relative added physics value, weighed by cost considerations

6. Do we remain open towards strong participation in future collider programs outside Europe? Should such a statement remain among the highest priorities? Should we extend the scope to include a variety of options like ILC@Japan, EIC@US, CEPC@China, ... ?

Portuguese input:

Europe should remain open towards strong participation in other future collider programs, but keeping the worldwide leadership in Particle Physics. This implies a clear engagement towards the construction of a future accelerator at CERN. We believe that the participation in collider programs outside Europe should be kept at a lower priority.

Feasibility and community support:

CERN provides the best conditions for the success of the next large collider project.

CERN also provides a truly international environment that has been welcoming physicists from all over the world for many decades. We believe that it is extremely important to maintain this commonwealth.

7. Anno 2013: “CERN should develop a neutrino programme to pave the way for a substantial European role in future long-baseline experiments. Europe should explore the possibility of major participation in leading long-baseline neutrino projects in the US and Japan.” Is the continuation of the CERN Neutrino Platform appropriate? Should we propose to extend the scope of the Neutrino Platform beyond long- baseline neutrino projects?

Portuguese input:

Yes. Neutrino physics is one of the hot topics today and should also be an high priority in the next decades. Detector R&D for the long-baseline neutrino projects has not finished. The current ProtoDUNE plan will continue after 2021, and its continued activity beyond that would be valuable towards the development and validation of the technologies for the DUNE far detector 4th module (called "module of opportunity"). R&D for the near detectors of DUNE, T2K and HK at the Neutrino Platform represents further opportunities for strengthening the European participation in these flagship experiments, and these capabilities could be extended to non-LBL projects.

8. Anno 2013: “Europe should support a diverse, vibrant theoretical physics programme, ranging from abstract to applied topics, in close collaboration with experiments and extending to neighbouring fields such as astroparticle physics and cosmology. Such support should extend also to high-performance computing and software development.” Should we strengthen this statement? Should we provide guidance how to achieve this?

Portuguese input:

The statement should be strengthened particularly in view of the fact that any future experimental precision measurements may have their impact constrained by theoretical uncertainties. These uncertainties can be brought under control through a dedicated theoretical programme that can only happen within a dynamic and diverse theoretical community.

Guidance, in the form of the creation of an European High energy Physics fellowship programme with also the possibility of stimulating the creation of High Energy Physics positions (in the form of co-funded bridge positions) at European universities, should be made explicit in the strategy.

This should be made to serve also the experimental community, offsetting a perceived difficulty in attracting independent European project positions by physicists who must devote a large fraction of their time to common tasks of the large experimental collaborations.

9. Anno 2013: “Detector R&D programmes should be supported strongly at CERN, national institutes, laboratories and universities. Infrastructure and engineering capabilities for the R&D programme and construction of large detectors, as well as infrastructures for data analysis, data preservation and distributed data-intensive computing should be maintained and further developed.” Should we strengthen this statement? Should we provide guidance how to achieve this? For example, related to new R&D cluster programs at CERN and in Europe, and related to the balance between blue sky R&D versus focused R&D.

Portuguese input:

The development of this field is based upon the development of detectors. And future experiments will need the best possible detector capabilities to make the most out of the unique conditions provided by accelerators. A coherent strategy for detector R&D is very desirable, as long as a wide participation is also ensured. CERN’s role as an aggregator of efforts and a stimulator of collaborations between teams and institutes should be pursued. Good examples of this are projects such as AIDA (<http://aida2020.web.cern.ch>), which brings together a broad network of institutes, universities and technological centers and CERN.

10. Should we make concrete the technology collaboration with the gravitational wave community?

Portuguese input:

There might be some room for collaboration with the gravitational wave community (and other communities) is sensors, monitoring and data analysis techniques.

11. Should the HE-LHC feature in our strategy update?

Portuguese input:

Feasibility:

The construction of the HE-LHC demands high-field magnets which don't currently exist. Their performance seems close to the one for FCC-hh magnets, so it seems that there is not enough gain in constructing the HE-LHC first. On the other hand, the complete replacement of the accelerator in the tunnel implies a long period of inactivity, such as has happened in the LEP-LHC transition, which would be bad for the community.

The LHeC would imply to build an electron Linac (the ERL). The LHC would be a little affected although the ERL could be built during LHC operation. It may be an interesting option if justified by the physics case.

Physics:

The HE-LHC energy represents only a factor two increase with respect to the LHC, which means that the discovery potential is limited.

12. In the context of the LE-to-HE-FCC-h/e/A scenario, would an adiabatic evolution from 6T to 16T/HTS magnets for FCC-h/e/A be an avenue to explore?

Portuguese input:

The LE-to-HE FCC-hh seems less well understood than other options at the moment, and so should be studied in more detail. The initial understanding is that the cost would imply a larger financial effort, with a 20% increase of the CERN annual budget, as compared to a 10% increase for other proposals. The expected gain of a factor 2-3 with respect to the LHC for initial running, and similar luminosity, can only marginally increase the precision of electroweak measurements. On the other hand the prospect of finding new particles is uncertain.