Birmingham Particle Physics Group Input to the Update of the European Strategy for Particle Physics

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This document is sent on behalf of the academic faculty members of the Birmingham Particle Physics Group. We begin by summarising our view of the most significant physics questions driving our field and go on to argue how we think they can best be addressed through new facilities, before ending with a few comments on maintaining our community and its wider impact.

1) What are the major physics questions driving our field for the next half century?

- It is hard to overstate the importance of the discovery of a Higgs boson in 2012. It is not just the 'completion of the Standard Model' (SM). It opens up a whole new scalar sector of fundamental particles, distinct from vectors and spin-half fermions for the first time. As well as being key to advancing our understanding of the mass generation mechanism for fundamental particles, it could potentially act as a portal to otherwise inaccessible sectors. There are endless possibilities for how scalar sector physics could play out, requiring the maximum possible exploration within available resources. This should include at least 1) tests of the compatibility of the 125GeV state with the SM in as many decay modes as possible, 2) searches for rare and exotic decay modes of the 125 GeV state, as well as unexpected production modes that may indicate new physics coupling through mass, and 3) searches for further scalar particles over as wide a range of masses and couplings as possible. In the longer term, it should also include detailed Higgs-self coupling studies.

- Electroweak (EW) scale SUSY looks to be in trouble, but the questions of naturalness, hierarchy, etc have not gone away. Many alternative BSM scenarios also require new particles or effects close to the EW scale. Others do not. Exploration of particle physics significantly beyond the EW scale is still in its infancy. The central business of our field has always been to search for previously unobserved effects at the energy frontier. Since we currently have no clear indication of the energies at which new physics will appear, it is important to maximise the mass reach (and intensity) at which we are able to probe.

- Dark Matter and Dark Energy are enormous questions for fundamental science which clearly point to the existence of BSM physics. However, as the WIMP paradigm becomes disfavoured, there is no single compelling theoretical framework for Dark Matter, opening up a significantly wider potential parameter space to explore. In addition, there seems to be no clear particle physics programme that fully addresses the Dark Energy question. The search for WIMP candidates should certainly be continued to the point where neutrino backgrounds become a major limitation, but even if directional information allows searches beyond the neutrino floor, there will come a point of diminishing returns for the required investment. Other paradigms should be

pursued in parallel, balancing costs of experiments against their impact on the range of allowed parameters.

- The dominance of matter over antimatter in the universe remains a deep mystery, especially since CP violation in the quark sector explains only a small fraction of it. Whilst neutrino physics also has something to say here, it seems probable that a fundamental breakthrough will be required for a full understanding. There are currently no clear pathways towards this, but we should be ready to invest experimental effort and resource as and when promising directions emerge.

- High-precision tests of the SM in the heavy flavour sector, through ultra-rare decays such as such as $K^+ \to \pi^+ \nu \bar{\nu}$, and through measurements of fundamental electroweak parameters are important both in their own right and through their potential to indicate BSM physics. Precision measurements can offer sensitivity to new effects at very large energy scales, well beyond those that can be directly explored.

- Whilst there has been (and remains) a diverse world programme of precision tests of the EW sector, there are many areas of the strong interaction where there is still scope for discovery. Despite significant recent experimental and theoretical progress, soft interactions remain poorly understood. Yet they contain emergent phenomena of a fundamental nature such as why the proton is absolutely stable and spin ½ and why quarks are confined, as well as hadronic mass generation. The taming of the growth of the gluon density towards low x in hadronic structure requires previously unknown dynamics. There may even be connections to gravity through AdS/CFT. It is important that we do not neglect this basic exploration of the phenomenology of a force we already know about.

- Neutrinos have become more and more accessible to experiment in recent years, through long-baseline oscillation experiments and in the use of reactors for both short baseline and neutrinoless double beta decay experiments. They may shed light on CP violation and it is important to understand the mass hierarchy and constrain the PMNS matrix. Involvement in neutrino physics therefore has to be a part of any complete European programme. Any large neutrino experiment should have wider capabilities than just its "core measurements", in order to maximise science returns and generate a satisfactory volume of publications. We note the importance of cross-investment when establishing engagement by CERN with neutrino programmes outside member states.

2) How can the major physics questions best be addressed in terms of a realistic set of facilities, focusing on what the strategy should be for Europe?

- The LHC and HL-LHC are the only energy frontier facilities that actually exist and/or are approved. They address many of the scientific areas above, including the top priority ones. It is of paramount importance to get maximum value from them. That includes making a total success of the HL-LHC upgrade and maintaining momentum towards full exploitation, with upgraded ATLAS, CMS and LHCb experiments. The utility of the wider CERN accelerator complex extends well beyond its vital role as injectors to the LHC. It should continue to be exploited through additional experiments that strengthen and broaden the physics programme.

- A cost-effective way of enhancing the LHC programme (~CHF 1Bn core cost, ie achievable within a decade using foreseeable resource) would be to add an electron accelerator to the LHC complex (LHeC). This offers PDF constraints at high x that tackle the increasingly dominant limitation in many LHC searches, as well as a standalone Higgs programme that complements the LHC (eg giving high precision on the $b\bar{b}$ and $c\bar{c}$ couplings) and some BSM sensitivity. It is also an excellent way to better our understanding of strong interactions / QCD. Its timely realisation (towards the end of the HL-LHC programme) requires immediate investment in high power energy recovery linac development.

- For several decay channels the best Higgs precision among short and medium term facilities would be through high energy e^+e^- scattering. It seems possible that such a facility may emerge in Japan or China, which would be very welcome. In view of the Higgs capabilities of the HL-LHC, the benefit to Europe of major investment in an e^+e^- project is questionable. Game-changing accelerator technology developments (eg plasma wake-field) that might yield order 10TeV fundamental fermion collision energies at adequate luminosity would change this conclusion.

- For both scientific and strategic reasons, it is imperative that CERN has a longer-term vision beyond the HL-LHC. Realisation of such a project is a long way in the future and we would not want firmly to commit ourselves prematurely. However, lead times are counted in multi-decades, so we have to be working already now. If there is a discovery at the HL-LHC or elsewhere, that will point the way. If not, the maximum possible mass reach for new particle discovery and sensitivity to the Higgs self-coupling are the highest priority considerations. FCC-hh makes the most sense from the current perspective, magnet development being the place where R&D is most urgently needed. The geopolitics needed to realise a clear financial model are a concern, but with ESA estimating the costs of the ISS at €100B[1] over 30 years, the precedent exists for international scientific collaboration on a scale beyond that required for the FCC.

- A strength of the European programme (through CERN) is the ability to maintain a portfolio of small-to-medium scale, modest cost, focussed, challenging but high gain, experiments that complement the energy frontier programme. Many such experiments indirectly explore higher energy scales than colliders, albeit in a more model dependent way. NA62 is a shining current example. This programme should continue.

- The CERN neutrino platform has been a success and has helped to cement strong cooperation in our field with the US. This is a good model and could very reasonably be continued if there are strong scientific motivations. It would, however, be strange to have an involvement at the same level with Hyper-K as with DUNE.

- The SHiP physics programme appears relatively weak (no guaranteed physics except the tau antineutrino discovery and a BSM search programme much of which appears increasingly to be covered elsewhere). As reported in the Conventional Beam Summary of the Physics Beyond Collider initiative, the existence of SHiP would significantly jeapordise or even require termination of other projects using the CERN North Area

[1] https://www.esa.int/Our_Activities/Human_Spaceflight/International_Space_Station/How_much_does_it_cost

beam facility. Given the cost (financial and loss of other activities), it should not be a priority.

- Tonne and multi-tonne scale direct dark matter (DM) experiments make good scientific sense, though one might question how many third generation experiments the world needs. With no discovery yet and the neutrino floor approaching, it is also important to take alternative approaches to dark matter searches, in particular low mass DM particles and axions. This seems to be an area which is progressing externally to CERN, though there may be a role for CERN R&D in collaboration with the European underground laboratories.

- Relativistic heavy ion physics has been an important component of the LHC programme, both in the dedicated ALICE experiment and also in ATLAS, CMS and LHCb. It has led to closer cooperation between formerly distinct communities, which should be encouraged to continue. Given that this is not a precision field (theory is typically 10%+ precision), there must come a point where it is no longer cost-effective to run the LHC with heavy ions when weighed against the necessary reductions in high luminosity *pp* running. This is a delicate balance that should be evaluated systematically before making a decision on when to stop heavy ion running.

3) What else is needed to maintain a strong European community in major strategic areas such as detector / accelerator R&D?

- CERN is the world's leading physics lab and gives a significant strategic advantage to European particle physics over the rest of the world. It is essential that we protect its continued existence, which means major European projects and expenditure should be sited there wherever possible. Its accelerator expertise and infrastructure is particularly pre-eminent internationally and a unique resource that must be carefully nurtured.

- "Doing the same thing again only bigger" must eventually approach the end of its plausible lifetime. As a community we need to invest in alternative acceleration methods such as plasma wake-field acceleration, as well as continuing to pursue cost reducing technologies. CERN provides an excellent platform for coordinated accelerator R&D and this should continue to be strongly supported as a collaborative venture across international laboratories and institutes.

- As a European community, we need to improve the way we support detector R&D. This could include changing the structure of the CERN RD collaborations, such that they receive direct support at CERN, are recognised as collaborations in their own right and are able to bid for external funding. The DRDC process that established the first RD collaborations in the 1990s is overdue for a refresh, with increasing focus on the generic R&D challenges for the facilities that lie beyond the HL-LHC. For semiconductor technologies, the prototyping costs for cutting edge developments have far outstripped the typical resources available due to the nature of the wider industrial context (very high non-recurring engineering costs for deep-submicron processing), making more efficient resource sharing especially urgent.

- The 25 year typical span from first prototypes to large system deployment for both accelerator and detector R&D requires a different funding mind-set and better

coordination across Europe and beyond, to avoid duplication of effort and sub-critical levels of investment. In the detector area, CERN could play a coordination role, though we expect that the resources will continue to be awarded by national funding agencies and the EU. While the accelerator community is addressing some of the possible challenges of the 2040's, some equally pressing issues for detector development (especially in areas that generate no immediate commercial interest) are not receiving anything like the same attention.

- The very significant potential of technologies developed for particle physics to continue to bring benefit to wider society, and to benefit from technologies developed for other applications, is generally well understood. Nevertheless, there could be a more coordinated approach across Europe to supporting technology transfer developments, recognising the need for sustained investment. In a fiercely competitive funding environment, national-level projects often win sufficient resource to achieve promising initial results but then cannot secure the necessary funding to take this forward.

- Past successes in demonstrating significant societal benefits of technologies developed first to tackle the immense challenges of cutting edge fundamental research complement the public's enduring interest in the science. Particle physics needs an ambitious vision for the coming decades with a coherent programme of accelerator, detector and computing R&D that promises the excitement to attract highly talented innovators whose developments will continue to drive technologies of benefit to all.