ECFA

European Committee for Future Accelerators

ECFA Newsletter #3

Following the ECFA-EPS meeting, 13 July 2019

https://indico.cern.ch/event/577856/sessions/291392/#20190713

Summer 2019

Picture: Patou Van Decraen, EPS-HEP2019 LOC

The process of updating the European Strategy for Particle Physics is in full swing. Based on the 160 input documents received, the Open Symposium in Granada was organised by the Physics Preparatory Group (PPG) and wonderfully hosted by the Spanish particle physics community (https://cafpe.ugr.es/eppsu2019/). More than 600 participants spent four days debating the scientific aspects of eight themes covering technology and physics. Both the input received and the discussions at Granada will be summarised in the Physics Briefing Book, which is being prepared by the PPG.

Opportunities to explore both the smallest and the largest scales of nature emerge when we improve our ability to make the invisible visible, hence our invention and innovative use of novel technologies related to the hardware, software and computing used in our research. The success and future of our field depend on our ability to attract the most talented researchers, to recognise their individual achievements, especially within large collaborations, and to provide them with adequate career opportunities. Surveys carried out by, among others, the ECFA Detector Panel, have shown clearly that this remains a challenge that needs to be addressed, especially for those researchers with aspirations at the technology frontier.

The impact of innovative research is evident in the LHC research programme. With the knowledge we had back in 2013, the potential precision on Higgs boson couplings that could be reached with the ATLAS and CMS experiments at the HL-LHC was estimated. While at that time a window was given between a pessimistic reach and an optimistic reach, the 2019 update of this potential precision included all the subsequent analysis and detector innovations, and now significantly outperforms some the most optimistic scenarios of 2013. This illustrates the bright future of opportunities for innovation at the LHC and HL-LHC, in both the instrumentation and the algorithmic side of analysis and computing. These innovations will play a key role in cementing the legacy of the HL-LHC.

A key objective of the overall Strategy process is to establish the most effective particle physics research programme in Europe, taking into account the global context of our field. The community-driven Strategy process is key to laying the groundwork for the next major project following the HL-LHC. With the aim of breaking ground in the late 2020s, the strongest and most concrete project proposal in terms of administration, technology and organisation should be delivered for a final decision at the latest during the next Strategy update, so that the project can be launched in a timely fashion.

The previous update of the European Strategy for Particle Physics called for design studies for accelerator projects at the energy frontier. With the present update, the time has now come to see whether the community, taking into account the global context of the field, can indicate a preference for either a new linear collider or a new circular collider to be built at CERN. Meanwhile, the R&D programmes will, of course, continue to work towards developing more powerful accelerator structures, while keeping an eye on potential cost-reduction and energy-efficient options. Although the proposed colliders are based on the traditional accelerator technologies that we have invented over the last few decades, novel accelerator technologies are also emerging in our field. Overviews of the ongoing studies into plasma-based acceleration, muon acceleration and high-temperature superconducting magnets for future colliders will be presented at the next open and plenary session of ECFA on 14 November 2019 at CERN.

Based on quantum field theory, the last half century has seen the invention and experimental verification of the Standard Model of particle physics as a complete description of the fundamental interactions at the electroweak scale among the building blocks of matter. We should not, however, confuse a complete description with a profound understanding of the physics involved. The most intriguing open questions must be answered in order to elevate, and potentially extend, this theory to an overall theory explaining how these fundamental interactions and building blocks at the smallest scales accommodate the evolution and content of our universe at the largest scales. Paradigm-shifting ideas must be explored in order to come to an understanding of, among other things, the electroweak hierarchy, baryogenesis and the features of dark matter. With an open mind, we need to prepare the most effective experimental and theoretical settings to produce the broadest hunting grounds in the next half century. Although there is no lack of novel theoretical ideas, there are no clear indications as to where new physics is hiding. We therefore need a strong and diverse, yet coherent and concerted empirical exploration. Needless to say, a strong research field needs a strong story to be told, and together we have to shape that story.

The community is considering the most profound study ever undertaken of the scalar sector as a prime avenue in the search for new physics. Delivering a comparison of the potential precision achievable by all future colliders was the mandate of a working group established by ECFA; the results were reported in arXiv:1905.03764. In the immediate future, i.e. the upcoming 20 years, the HL-LHC experiments will be able to probe many Higgs boson couplings to the level of a few percent. In addition to the HL-LHC's sensitivity, all proposed first-generation e^+e^- colliders operational in the mid-term future, i.e. roughly in the second block of 20 years, will be able to achieve major and comparable improvements. In a third stage or long-term future, a higher-energy e^+e^- collider or hadron collider operational in roughly a third 20-year block will make it possible to reach the ultimate sensitivity, especially on the Higgs boson self-coupling. Novel accelerator technologies may very well mature, for example via dedicated demonstrator facilities in the mid-term future, and may turn out to be viable for high-energy and high-luminosity colliders in the long-term future.

There is new physics out there, and our main objective should be to discover it. The exploration of the scalar sector with colliders is only one possible avenue in the search for new physics. During the Open Symposium in Granada, discussions revolving around five other physics themes were held. Following the HL-LHC era, in the mid-term future, a Z, W, Higgs and top-quark era might emerge with colliders, and the long-term future could deliver a high-energy era. Each era provides a very broad spectrum of opportunities in the search for new physics, through indirect exploration via precision physics, through dedicated experiments to break the Standard Model, or through direct searches in both the visible and the hidden sectors. The approach is up for debate, but with a granularity of 20-year eras and in the absence of clear indications for new physics, it would probably be wise to follow the general principle that, in each era, important steps forward should be taken in the widest possible variety of directions in which new physics might be found. The physics potential provided by non-collider experiments is strong and key to covering all the scientific bases. Strategic choices must be made in order to achieve the most competitive and complementary non-collider programme in Europe.

The connection between the studies of the largest and smallest scales of nature is being explored by astroparticle and particle physicists, especially on the technology front, where synergies emerge. At the first JENAS event (https://jenas-2019.lal.in2p3.fr/), jointly organised by ApPEC, ECFA and NuPECC, we will further discuss the synergies needed to unravel both the smallest and the largest scales of nature.

With the input from the Physics Briefing Book due by the end of September, the next step in the European Strategy process is to define some overall long-term scenarios, keeping an eye on evolutions in the global landscape, and to discuss within the European Strategy Group (ESG) their scientific coverage, feasibility and community support. Given the long-term impact of these strategic choices, ECFA is organising a full-day event in the Main Auditorium at CERN on 15 November 2019, to enable early-career scientists to debate the Strategy. Participation in the meeting will be limited to early-career researchers and, in order to cover the full spectrum of the community, will be by invitation only. ECFA members are asked to consult within their national community and to nominate, via their Restricted ECFA members, up to 10 early-career researchers from within the institutions in their country. Nominations are to be delivered to the ECFA Chair by 9 September. Both PhD students and postdocs active in particle physics and/or adjacent fields are eligible, and eligible nominees will receive an invitation from the ECFA Chair. After the event, the early-career researchers will be mandated to deliver a brief document overviewing their thoughts on the Strategy. There will be no need to reach a consensus on all aspects, but the document must cover all the topics that were discussed during the meeting. The ECFA Chair will bring this document to the attention of the ESG.

There is, of course, some conflict between the aspirations of scientists and the constraints of funding bodies, and it will be challenging to unite both in a bottom-up strategy process. The ESG will gather for one week in January 2020 to draft the Strategy update, which will be submitted to the CERN Council for consideration during its March session and then tabled for approval in May 2020, during a dedicated session of the Council.

This ECFA Newsletter reports on topical talks presented during the joint ECFA-EPS Open Session organised on 13 July during the 2019 EPS-HEPP conference at Ghent (Belgium). All talks can be found at https://indico.cern.ch/event/577856/sessions/291392/#20190713 .

Jorgen D'Hondt, ECFA Chair
Carlos Lacasta, ECFA Scientific Secretary

Technology for future colliders

by Caterina Biscari (ALBA Synchrotron, Spain)

Several applications of particle accelerators drive the progress in their technology, future colliders being one of the most demanding in terms of new developments. The main objective in the field of high-energy physics colliders is to obtain the highest reachable energies and the highest possible luminosity at those defined energies. The community is considering collider projects based both on technologies that are already available and on the potential products of challenging R&D programmes. Energy efficiency and management are being addressed in all options. Investing in the development of technologies to improve the energy efficiency pays off in terms of savings and will serve society in general.

Future electron–positron colliders are designed to provide the highest possible luminosities in order to serve as Higgs boson factories. The technology for linear colliders has essentially already been developed and demonstrated. A staged approach to reach higher energies is under consideration for ILC, reaching centre-of-mass energies of 250 to 1000 GeV, and, for CLIC, of 380 to 3000 GeV. The proposed superconducting RF system for the ILC relies on those deployed at facilities that are already in operation, such as the European XFEL, or that are under construction, such as LCLS-II. The high-gradient-high-frequency cavities for CLIC are being tested in order to reach the design parameters, and the dual-beam acceleration concept has been demonstrated. Progress in the demanding beam emittance and nanobeam stability requirements is ongoing, with tests under way at the ATF and progress being made in the photon science community with the new technology of diffraction-limited storage rings and XFELs. Even though only one linear collider has been operated in the past, the community is confident that a future one would perform well.

Circular lepton colliders, such as the proposed FCC-ee and CEPC machines, are based on well-established technologies, and their design is founded on several examples of existing colliders at lower energies, from the LEP collider to the B and Phi factories. The biggest challenge is the handling of the synchrotron radiation power, which limits the luminosity that can be attained. The long circular tunnel, with its demanding civil engineering requirements, could be reused to host a future hadron–hadron collider. The FCC-ee design foresees collision stages at the Z, WW, ZH and top quark pair working points, corresponding to centre-of-mass energies of 91, 160, 240 and 350 GeV, respectively.

The quest for the highest collision energies is the basis of the hadron–hadron collider designs. The biggest challenge for the FCC-hh (or the SPPC) is the production of high-field magnets. The history of superconducting magnet technology has seen uninterrupted progress over the last five decades, but also shows the long time needed for advances in the attainable magnet field and for moving from a successful magnet prototype to mass production. As of today, a few 11 T magnets are being installed in the LHC for tests before its high-luminosity phase, and a 14 T prototype has been successfully tested at Fermilab. Two to three more decades would be needed to reach the full magnet strength in the ultimate FCC-hh design, i.e. 16 T in order to achieve 100 TeV collisions in proton–proton mode and be adequate for high-energy ion–ion collisions. High-temperature superconducting (HTS) technology, which also has applications in several other fields, may be an alternative possibility to reach these parameters while being more sustainable in terms of operating power.

The community is not only focusing on the above projects, all of which have already been documented in CDRs, but is also exploring alternative possibilities. Energy-recovery linacs (ERL) are being considered as an alternative for delivering high-energy electron and positron beams. Muon colliders are being studied as a way of reaching the ultimate high energies with high efficiency. While the technology is still far from being mature, even for a CDR, new concepts for the production of muons using positron beams are being developed. Plasma acceleration is demonstrating the capacity to reach accelerating gradients several orders of magnitude higher than those achieved with conventional RF acceleration. Preliminary concepts for possible colliders based on this technology are being developed in collaboration with a wide community interested in this field for its numerous applications. It is expected that decades of R&D will be needed to prove the feasibility of such collider technology.

Community challenges and opportunities for detector R&D

by Ariella Cattai (CERN)

Enormous challenges face all detector technologies for the next accelerators! Future detectors need to be fast, precise in time and space, ideally linked to energy determination and able to exploit in-situ artificial intelligence. We are heading towards a future of multi-functional complex detectors in which high-precision elements are integrated into large-scale apparatuses with a strong interdependence between the detection technologies, electronics and advance mechanical structures. This calls for: 10x10 μm^2 pixel cells for the vertex detectors, which also aim for extraordinary light mechanics: ~0.05% X_0/layer; 4D tracking with precisions as low as ~10 ps per hit; a new reconstruction philosophy, fully complementary to the vertex reconstruction, which introduces timing layers, as precise as 30 ps, to disentangle tracks on the basis of their arrival time; TOF in the range of 20ps; 5D imaging calorimetry based on a PFA algorithm, where the spatial information of the energy deposited in a millimetric cell is accompanied by its arrival time; and very high tolerance to radiation up to $10^{17\text{-}18}$ $1MeVn_{eq}/cm^2$ at the hottest point in the FCC-hh. It is remarkable that many of these challenges are concentrated in a very ambitious apparatus, ALICE^{++}, which relies entirely on CMOS-MAPS technology, from the vertex to the electromagnetic Shower Pixel Detector.

Extreme resolutions, very large numbers of channels, radiation hardness and the requirement of very high readout speed have a direct impact on the specifications of the front-end and on-detector electronics, which have become the polarising issues for the future. Very deep-submicron technologies, advanced interconnections and 3D vertical assembly could meet the future experimental requirements. However, their development is driven by high-volume commercial applications and access to these new technologies requires substantial financial and human resources. To successfully meet the upcoming challenges, our community needs to select and pursue vigorous R&D on a very limited number of technological node(s) (e.g. one 28 nm and one 17 nm or lower) that are expected to be available for a long time. In addition, common technical support must be put in place and all development efforts must be coordinated. As opposed to the presented decentralised R&D programme for detectors, the R&D on micro-electronics technologies requires a minimum critical mass team providing technical support to the community and strong coordination of the developments in order to share the prototyping and production costs.

Many examples show that it can take 10-15 years to mature a technology from the demonstrator stage to an established technique suited for large-scale production. Today, experiments last a lifetime with thousands of people. Hence, this scheme poses a host of ineluctable challenges, which were discussed in Granada, organised around three major themes.

a) *The coordination of R&D:* should it be distributed everywhere in an independent way, or centralised in laboratories and consortia? Based on the obvious evidence that expertise is distributed across many institutions, the general opinion converged on the conclusion that R&D should not be centralised exclusively in large-scale facilities. There is evidence that the current R&D collaborations (RD#, AIDA2020, CALICE, etc.) are effective models of collaboration: hence we should reflect on establishing new ones and on optimising their reviewing process, implementing regular reassessments in addition to the internal collaboration procedures. Additionally, collaboration must be enhanced among physics communities in order to search for synergies and exchange information between physics fields, technology specialisations and industry.

b) *The human factor:* how can we give recognition to young people? How can we foster recruitment? How can we provide effective long-term training? First, it is felt that instrumentation activities must be recognised as fundamental research that guarantees a valuable PhD thesis and grants equal career opportunities. Next, priority must be given to the training of young researchers, offering adequate instrumentation courses at university and providing maximal support to the organisation of specialised training platforms in large laboratories and institutes.

c) *Should R&D be guided by existing experiments, or should "blue sky R&D" be better supported?* It is historically proven that major detector technologies (TPC, RICH, MPGD, etc.) have not flourished due to R&D performed within large collaborations where, most of the time, studies are unavoidably polarised by internal challenges. It is therefore important to maintain an active generic R&D community that fosters new ideas for future detectors, eventually suited for applications outside fundamental physics. For this last point, experience shows that this step can be significantly sped up if adequate attention is given to early technology transfer programmes in close collaboration with industry. However, a recent survey on the status of R&D in Europe showed that only 30% of projects exploit technology transfer strategies and, when this is done, almost 70% of the groups feel that they do not get enough support in solving financial, workforce, technical and legal issues. To conclude: we all agreed that it is necessary to bring fundamental research closer to the needs of the whole of society; therefore, we need to review and strengthen our technology transfer model.

What will future colliders reveal about the Higgs boson?

by Christophe Grojean (DESY and Humboldt Universität zu Berlin, Germany)

The Higgs boson discovery was a major milestone for high-energy physics. Many of us are still excited about it. And others, especially in other fields of science, should be very excited too. Because the discovery of the Higgs boson was first and foremost a discovery of new forces among the elementary building blocks of nature, forces of a different nature than the fundamental interactions known so far: the Higgs forces are not associated with an underlying local symmetry, but instead are deeply connected to the structure of the space–time vacuum, resulting from the intimate laws of quantum mechanics and special relativity. But also because the Higgs boson is the simplest Q-bit: as far as we know, it has no structure, no spin, no charge, unlike other SM particles. This vacancy may actually be what makes it so important. According to the principles of Quantum Field Theory, the Higgs boson can easily couple to a hidden sector that may make up the dark matter of the universe.

Knowledge of the values of the Higgs couplings is essential to our understanding of the deep structure of matter, from the stability of the nuclei as a result of the up- and down-quark Yukawas, to the size of the atoms set by the electron Yukawa and the lifetime of the electroweak vacuum decided by the top-quark Yukawa. The Higgs self-coupling controlled the thermodynamics of the electroweak phase transition that probably occurred 10^{-10} s after the Big Bang and that might be responsible for the matter imbalance within the universe. At the same time, the Higgs measurements have not yet taught us much about the physical laws beyond those of the Standard Model, since the typical Higgs coupling deviations inherited from new physics would scale as $\frac{\delta g_h}{g_h} \sim \frac{g_*^2 v^2}{\Lambda_{BSM}^2}$ compared to the SM predictions and a measurement at 10-20% accuracy does not do better than direct searches for new physics, which are already probing scales above one TeV ($v \approx$ 246 GeV is the weak scale, Λ_{BSM} is the typical mass scale of new degrees of freedom and g_* is the size of their interactions with the SM particles). Five hundred years after Magellan and Elcano, the explorers of the unknown can use the Higgs boson as a new compass, but the quest should start with a precision measurement programme.

Within the SM, all the Higgs couplings are uniquely fixed in terms of other quantities that have already been measured. Measuring the Higgs couplings thus requires a parametrisation of the deviations from the SM induced by new physics. The so-called kappa-framework is the simplest parametrisation directly related to experimental measurements of the production and decay modes of the Higgs boson. For this reason, it has been widely used by the community. It only compares the experimental measurements to their best SM predictions and does not require any new BSM computations *per se*. From a more theoretical perspective, its relevance arises from the fact that it actually fully captures the leading effects in single Higgs processes of well-motivated scenarios. The constraints derived from such analysis can be readily exploited in order to derive constraints on the new physics parameters. This kappa-framework has, however, its own limitations when Higgs measurements need to be put in perspective and compared to processes with different particle multiplicities, or combined in a global way with other measurements carried out in different sectors or at different energies. An effective field theory (EFT) approach naturally extends the kappa-framework. First, it makes it possible to exploit polarisation- and angular-dependent observables to which a κ-analysis will remain blind. Also, such analysis is a useful tool to probe the Higgs boson in the extreme kinematical regions relevant for colliders operating far above the weak scale, exploring the tails of distributions.

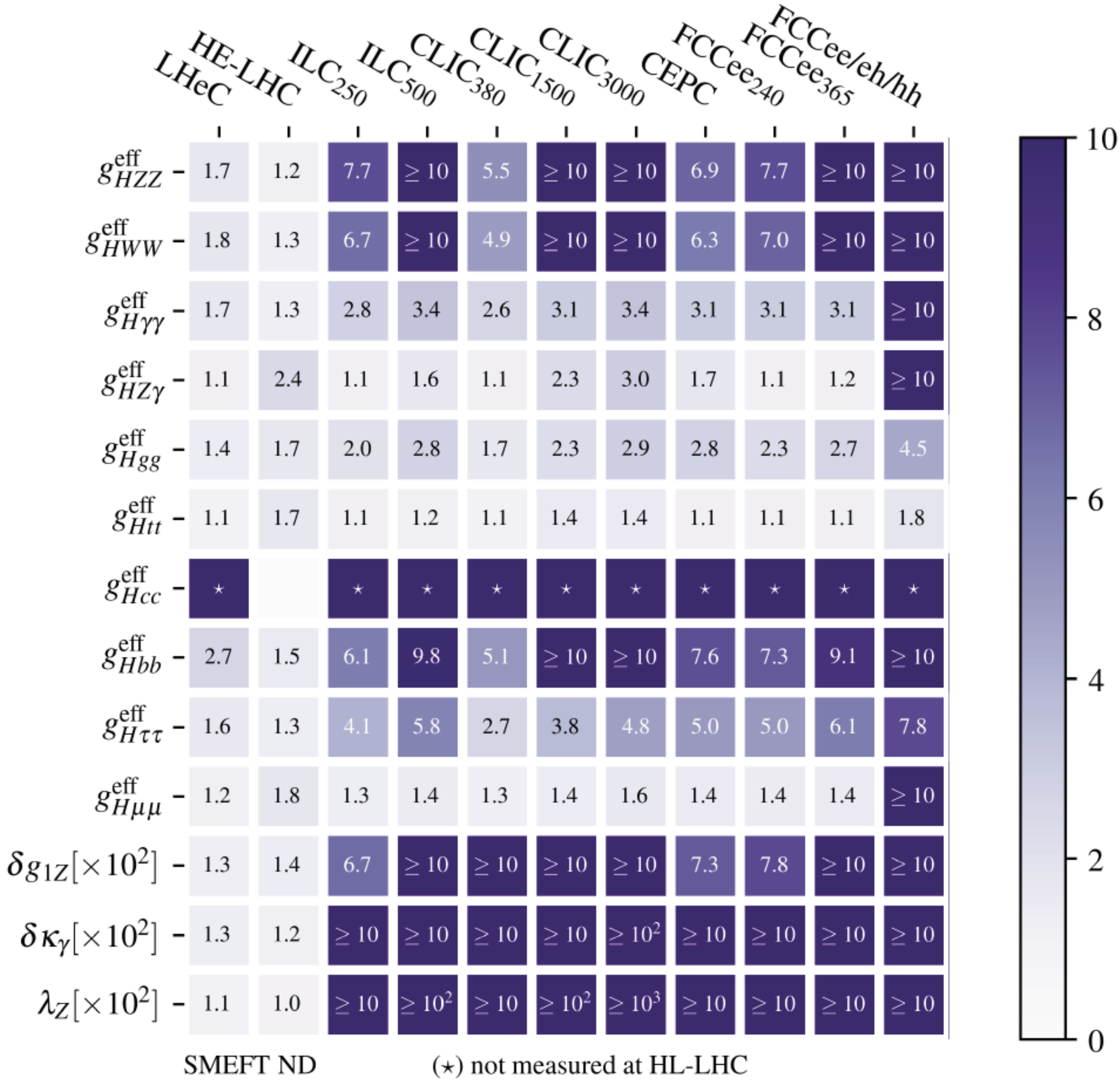

Figure: Factor of improvement in different channels viz. HL-LHC. From Higgs@Future Colliders ECFA working group report.

The Higgs@Future Colliders working group mandated by ECFA studied in detail the physics opportunities of the various future collider projects that could succeed the LHC. Either circular or linear, with leptons or with hadrons, each of them comes with their pros and cons, but all offer a rich Higgs programme, especially when combined with LHC measurements. The full exploitation of the experimental programme will also require earnest theoretical efforts in order to reduce the uncertainties of the Standard Model predictions through dedicated calculations at the highest order, often requiring the invention of new mathematics.

The figure gives a factor of merit for the various proposed future colliders with respect to what will be known at the end of the high-luminosity phase of the LHC. As a way to put these numbers into perspective, it should be remembered that an improvement factor of 5 means that if no deviation is seen at the HL-LHC at 68% CL, a 5σ discovery is still possible at a future collider. With the exception of the statistically limited channels ($\gamma\gamma$, γZ and $\mu\mu$), which will have to await a machine producing a billion Higgs bosons, and the channels involving the top quark, which have already been copiously probed at the LHC, all the proposed future colliders have the potential to outperform the HL-LHC in Higgs physics and leave us with legacy measurements that will populate the next generation of physics textbooks.

Main reference: "Higgs boson studies at future particle colliders", J. de Blas et al. (Higgs@FC WG), arXiv:1905.03764.

Physics Beyond Colliders

by Claude Vallée (CPPM, France)

The growing theoretical and experimental interest in searches for new physics beyond the high-energy frontier motivated the CERN Management to launch the Physics Beyond Colliders study, as input to the update of the European Strategy for Particle Physics. The goal was to identify fundamental physics domains that could uniquely benefit from the CERN accelerator complex and infrastructure and were complementary to other worldwide facilities and high-energy frontier colliders in operation or under discussion.

Following calls for ideas within the community, working groups involving more than a hundred contributors have investigated the physics potential and implementation issues of the proposed projects (http://pbc.web.cern.ch/). A set of reports have been submitted as input to the Strategy update, including a summary report providing an overview of the study [arXiv:1902.00260].

The PBC proposals range from non-accelerator experiments to projects hosted mainly on the SPS and the LHC in a non-collider mode. The low-energy facilities of the LHC injector complex (ISOLDE, n_TOF, Antimatter Factory) have not been considered thoroughly, since they recently benefited from upgrades that secure their future for the next decade.

The PBC study has identified several domains where the CERN complex offers unique opportunities. Fixed target collisions at the SPS and LHC correspond to specific kinematic domains for both structure function measurements and investigations of the QCD phase transition, where there is little competition worldwide. In particular, full exploitation of the LHC fixed target potential with internal gas targets, as already pioneered by LHCb, would allow significant improvements of the proton structure function determination at high x, a region of high interest for the HL-LHC discovery potential. As regards searches, precision and rare decay experiments can provide indirect access to higher masses than direct searches at the LHC, and beam dumps at the SPS energies investigate a unique hidden sector domain in the MeV-GeV mass range, between the LHC domain and low-mass ranges covered by non-accelerator projects (cf. figure).

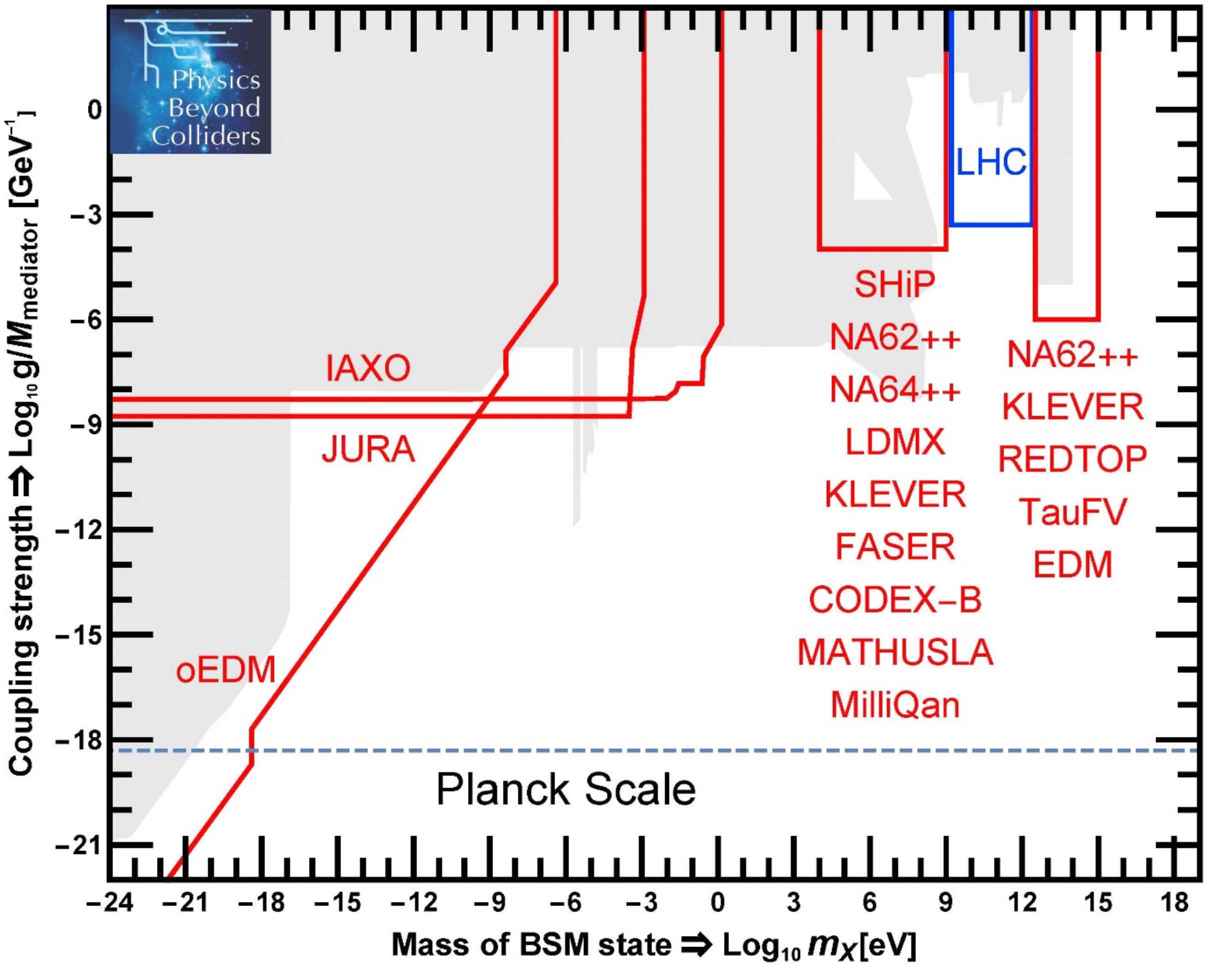

Figure: schematic overview of the BSM landscape, based on a selection of specific models, with a rough outline of the areas targeted by the experiments considered in the PBC sensitivity studies.

The short-term PBC projects for Run 3 raise no strategic issues and have been handed over to the SPSC and LHCC committees. The larger projects with a longer timescale compete with each other in terms of resources and implementation constraints, and will therefore require guidelines from the Strategy in order to proceed further. Among them, the SPS Beam Dump Facility with the SHiP and (possibly) the TauFV experiment has been identified as having unique potential in the worldwide landscape for dark photon and heavy neutral lepton searches, as well as for third flavour physics (ν_τ interactions and τ rare decays). It is now mature and ready for an implementation decision pending the Strategy guidelines. The two new electron beamlines that have been proposed also have interesting potential but are exposed to stronger competition: the LDMX detector for invisible dark photon searches on an eSPS beam has a more straightforward implementation opportunity on the LCLS-II beamline at SLAC, and the domain targeted by an AWAKE beam for visible dark photon searches will likely be covered by the competition in the coming decade.

Some of the projects discussed in PBC have found opportunities to develop outside CERN. The IAXO axion helioscope (the successor of CAST) is currently being positively reviewed at DESY. The proton EDM ring will require a prototype ring to validate the control of systematics, for which COSY in Jülich is a candidate site. The CERN contribution to that kind of project is decisive, and support from the Strategy of the national laboratories' initiatives will be welcome in order to maximise the resources available to the projects.

Synergies between particle physics, astrophysics and nuclear physics

by Caterina Doglioni (Lund University, Sweden)

One overarching objective of science is to further our understanding of the universe, from its early stages to its current state and future evolution. This depends on gaining insight into the universe's most macroscopic components, such as galaxies and stars, as well as describing its smallest components, namely elementary particles and nuclei and their interactions. It is clear that this endeavour requires combined expertise from the fields of astroparticle physics, particle physics and nuclear physics. A number of the contributions and discussions at the recent Granada meeting for the update of the European Strategy for Particle Physics (http://cafpe.ugr.es/eppsu2019/), as well as the contribution at the ECFA-EPS Open Session summarised in this newsletter, highlighted a growing wish for closer collaboration between ECFA and the astrophysics (ApPEC: https://www.appec.org) and nuclear physics (NuPECC: http://www.nupecc.org) communities.

Many physics problems where synergies between particle physics, astrophysics and nuclear physics are required are discussed in the ApPEC and NuPECC strategy documents. Among these, this contribution focused on the challenge of elucidating the nature of 27% of the matter–energy content of the universe, commonly called "dark matter" [1].

Pursuing these scientific drivers also requires mastering challenges related to instrumentation (e.g. beams and detectors), data acquisition, selection and analysis, and making data and results available to the broader science communities. Joint work and recognition of these "foundation" topics, also covered in detail in the contributions by C. Biscari, A. Cattai and G. Stewart, will help all communities grow towards their individual and common scientific goals. This contribution also presented one of the many common challenges faced by particle physics and astrophysics: the necessity of dealing with large, sometimes heterogeneous datasets and deriving insight from them in short periods of time.

A number of platforms and fora exist at CERN and in Europe to facilitate cross-talk among different communities. To name just a few: the LHC Dark Matter Working Group (https://lpcc.web.cern.ch/content/lhc-dm-wg-wg-dark-matter-searches-lhc), which is also of interest for the astroparticle community [2]; the recently inaugurated European Center for Astroparticle Physics, currently hosted by CERN (https://home.cern/news/news/physics/new-centre-astroparticle-physics-theory); the European Science Cluster for Astronomy & Particle Physics ESFRI Research Infrastructures project (https://projectescape.eu); the HEP Software

Foundation (https://hepsoftwarefoundation.org), which aims to facilitate cooperation and common effort in software and computing; and the highly successful CERN Neutrino Platform (http://cenf.web.cern.ch).

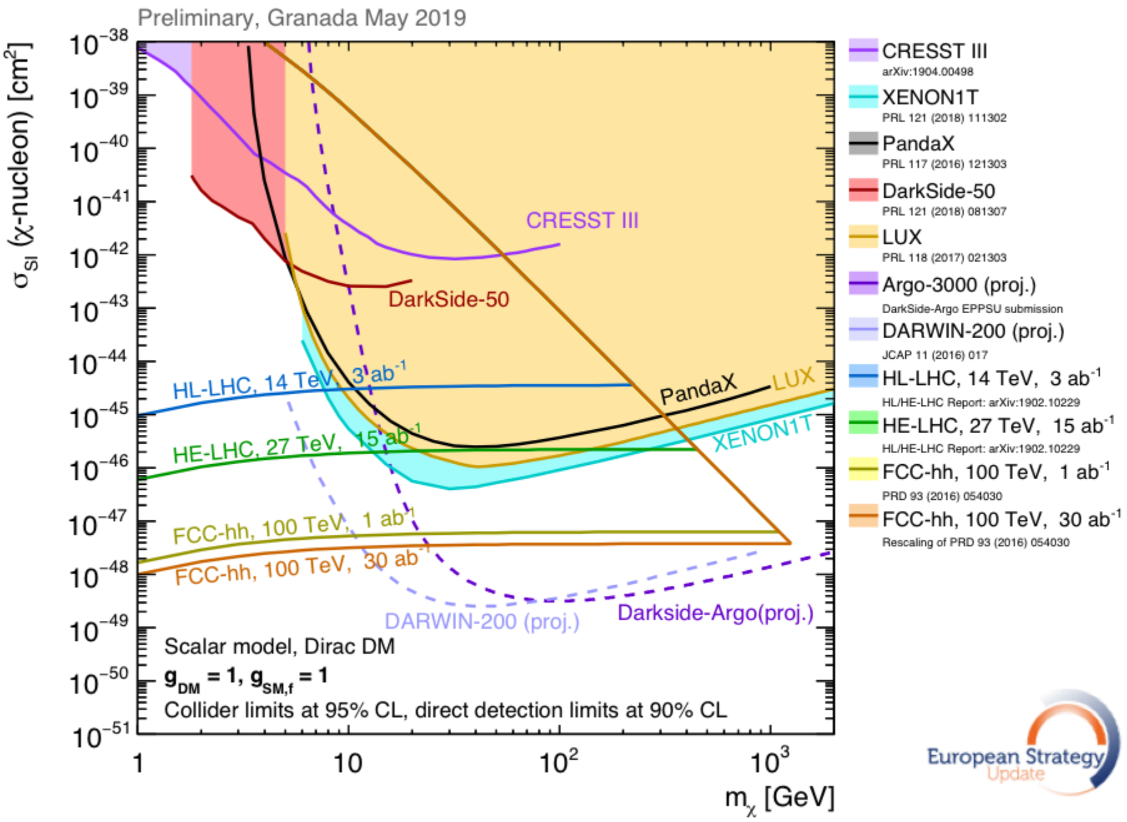

Figure: Comparison of sensitivities of future collider and direct detection experiments within a simplified model scenario. If DM is composed of particles within this model with a mass between 10 GeV and 1 TeV, future colliders and direct detection experiments will be able to confirm each other's discoveries in the next decades. Taken from [3], to appear in the EPPSU Briefing Book.

The examples cited in this EPS-HEP contribution are only a very limited subset of how the particle, astroparticle and nuclear physics communities could work together to answer challenging scientific questions. Other topics where synergies exist mentioned during the session were axion-like particles [3], the theory and experimental efforts bridging the gap between nuclear and high-energy physics [4], and the opportunities offered by astrophysics experiments (e.g. Auger) spanning a much higher and complementary energy regime with respect to nuclear and particle physics experiments.

Since detector technologies are often common to different communities, the CERN expertise stemming from the current world-leading collider programme could be an asset for experiment building in Europe and worldwide. Moreover, data collection and analysis benefit from becoming faster, more efficient and more open: using versatile computing strategies and tools to solve diverse problems fosters common expertise that lasts beyond a single experiment.

In conclusion, there is a common wish for the European Strategy process to facilitate closer collaboration between the particle, astroparticle and nuclear physics communities, in a context where the design of detectors, data acquisition systems and computing is an integral part of our quest to understand the universe.

[1] https://atlas.cern/updates/atlas-feature/dark-matter
[2] https://www.appec.org/news/eppsu-and-the-synergy-of-astroparticle-and-particle-physics
[3] C. Doglioni's talk at EPPSU: https://indico.cern.ch/event/808335/contributions/3373983/
[4] I. Irastorza's talk at EPS-HEP: https://indico.cern.ch/event/577856/contributions/3396833/
[5] J. Fiete's talk at EPS-HEP: https://indico.cern.ch/event/577856/contributions/3396853/

Software and Computing Challenges

by Graeme A. Stewart (CERN EP-SFT)

Software and computing form a critical part of the HEP scientific programme. HEP experiments, particularly at the LHC, require event generation, simulation, reconstruction and analysis at a massive scale, running on an infrastructure valued at hundreds of millions of euros.

Our code base, of tens of millions of lines in C++ and Python, originated in the era of serial processing on CPUs. We are challenged by the last decade, during which single-core performance has largely stalled and processor improvements come by way of *concurrency*, where increased throughput can be achieved only by performing more tasks in parallel. We have also seen the rise of compute accelerators, such as GPUs, which abandon the traditional architecture of CPUs and optimise for total throughput instead of single thread performance. Modern devices are often optimised specifically for machine learning problems, which we are only just learning to fully exploit. These varied devices usher in an era of heterogeneity, where different architectures need to be exploited coherently.

There is a considerable amount of ongoing R&D already in the community that addresses these evolving challenges. In particular areas we are well advanced, but we remain a long way away from having significant general HEP applications that are truly well adapted for running on all modern hardware.

In the computing domain, active R&D in data organisation, management and access (DOMA) aims to optimise our use of resources, particularly by reducing storage costs and exploiting high-speed networks.

Our precision physics programme requires more precise event generation, bringing ballooning CPU costs from NLO and NNLO calculations and serious issues of negative event weights. In detector simulation, speed increases are necessary to increase the simulated events to match higher trigger rates. We require code modernisation, as well as the integration of new techniques that can replace particle transport, at least in some areas where the physics impacts are acceptable. Machine learning to replace expensive calculations is an area of active R&D.

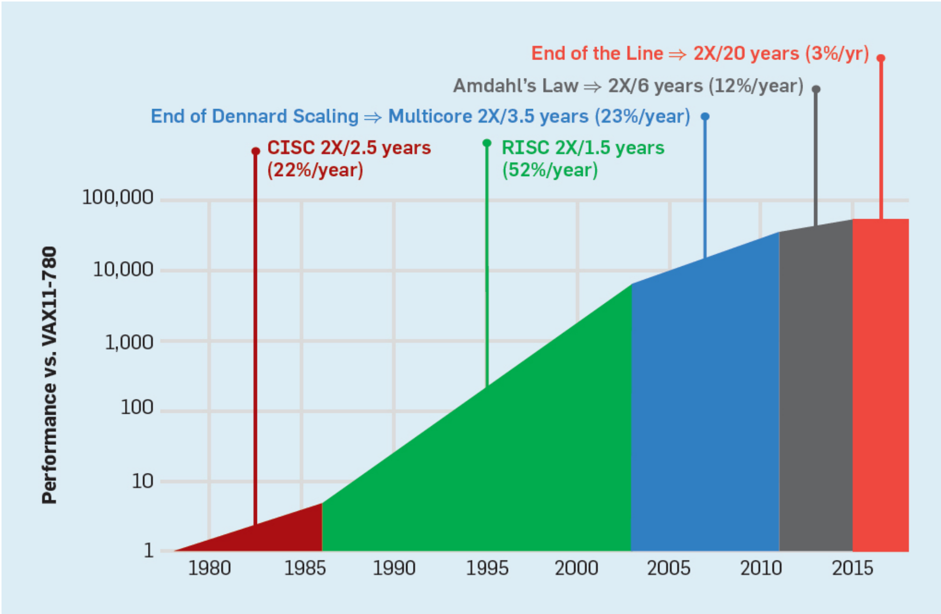

Figure: Slowing gains on traditional CPUs (from Hennessy and Patterson, Communications of the ACM, February 2019, Vol. 62 No. 2, 10.1145/3282307)

Physics considerations often now mandate triggers in software to get the best possible selection of events, aiming for offline quality reconstruction to be performed close to the detector. For some fraction of events, the raw data can be discarded, so that more events can be kept, increasing the amount of physics for a given budget. This can be extended to analysis outputs, which can then be generated in "real time", producing huge resource savings.

For scaling physics analysis, effective use of analysis data is improved by analysis trains, so that the cost of staging and reading data is amortised. A complementary approach is to reduce the required data for analysis to a minimum, allowing for greater physics content in a given storage footprint and an optimised EDM that helps keep up a very high event processing rate. General data science and machine learning techniques are very applicable to physics analysis, and effective meshing of our tools and external ones is an active area.

Despite many specialisms and peculiarities, there is much potential synergy between different HEP communities, other big data sciences, and academic specialists in software engineering and computer science. There is potential for new collaborations and virtual institutes combining problem solving and training to add more value to the investments that are needed. The final message is that HEP faces a vast challenge to effectively use modern hardware and techniques and that more investment in software is urgently required.

Announcements

This newsletter is available for you to communicate widely within your communities. To facilitate the distribution of future ECFA Newsletters, an e-group has been created, where anybody with a CERN account (or at least a CERN lightweight account) can register. You can do so under “Members” via the following link:
https://e-groups.cern.ch/e-groups/Egroup.do?egroupId=10319139&AI

A full-day open session of Plenary ECFA will be held on 14 November 2019 in the Council Chamber at CERN. The session is open for all to participate and will revolve around novel accelerator technologies: towards colliders using plasma wakefield acceleration, towards colliders with muons and towards high-temperature superconducting (HTS) magnets.

A full-day ECFA session for early-career researchers will be held on 15 November 2019 in the Main Auditorium at CERN. Participation will be by invitation only and limited to early-career researchers. With a deadline of 9 September, up to 10 early-career researchers can be nominated by each RECFA member, i.e. 10 for each country (plus CERN). Both PhD students and postdocs active in particle physics and/or adjacent fields are eligible, and eligible nominees will receive an invitation from the ECFA Chair.

On 15 November 2019, in parallel to the event mentioned above, a meeting of Restricted ECFA will take place, followed by a meeting of Plenary ECFA.

The details of these events will be announced in due time on the ECFA website, https://ecfa.web.cern.ch .