For the main meeting on 15th Nov (https://indico.cern.ch/event/847002/)

Speaker: Patrick Schwendimann
Chair: Abhijit Mathad
Minute takers: Julian Garcia Pardinas and Nils Hermansson-truedsson
Human and social factors liaison: Elisa Manoni

Minutes of the meeting that took place on 11th Nov Monday (b/w 13.00 to 15.00)

Location (Vidyo meeting): https://indico.cern.ch/event/861435/
Briefing book: http://cds.cern.ch/record/2691414/files/Briefing_Book_Final.pdf
Participants: Abhijit, Dominik, Elena, Elisa, Julian, Kilian, Maja, Nicolas, Nils, Olivier, Patrick, Rado, Tom.

1. **What is missing according to your group in the corresponding chapter in the Physics Briefing Book?**

   The general consensus of the attendees with regard to this question was that the briefing book very nicely describes the short, mid and long term goals of the topics covered by this WG. We empathise that in preparing such a concise document one is forced to omit the full scale of the physics explored by some of the experiments discussed. Nonetheless, the ECRs did bring to our attention some of the following topics:

   a. There is mention of tau EDM but the tau magnetic moment (g-2)_{\tau} isn't mentioned at all. NP scenarios can enhance this measurement severely when compared to (g-2)_{\mu}. There also have been several proposals to probe this using various techniques: LHC pp collisions: 1610.01601 and 1711.02405; LHC Heavy ion collisions: 1908.05180 and 10.1016/0370-2693(91)91309-J; Belle II: 1601.07987, 1803.00501; CLIC/ILC/Fcc-ee: 1804.02373 and 1810.09570; LHeC/Fcc-he 1809.01963, 1903.04135. Additionally on page 71 bent crystals are mentioned in the context of heavy baryon electric and magnetic moments but there are also proposals for using them for taus: 1810.06699 and 1901.04003
   b. Although the importance of charm physics is highlighted with the New Physics (NP) scales that one can probe with precision studies with charm. It was mentioned that there were no concrete estimates for future sensitivities to rare charm decays and mixing parameters. In light of B physics anomalies, exploring such decays could serve as a complementary subsystem to get a better understanding of their origin.
   c. There have been steps taken to explore the c and b baryon sectors. Future precision studies with flavour changing decays of baryons is also envisaged. However, the briefing book has no mention of flavour physics with baryon

systems. A lot of theoretical and experimental work is needed to better understand these sectors.

d. The section about LFV misses the point that also LHCb has a vital program of searches for LFV decays of heavy flavoured mesons.
e. In light of the dry spell for NP, model independent NP searches must be encouraged within all the experiments.

Could add a little detail on the nutau sector. Measurements of nutau appearance and/or disappearance required for PMNS unitarity and lepton universality tests. IceCube nutau appearance is mentioned in the "Cosmic Messengers" section (perhaps the wrong place), but direct prompt nutau production from e.g, the LHC could also be mentioned. Nutau cross section measurements are required from support experiments to aid these programs, e.g. DsTau/NA-65 experiment at CERN SPS, nutau detector at SHiP, etc. These measurements also help with e.g. nutau backgrounds in next-gen LBL, astrophysical nutau detection in neutrino telescopes (a key issue in using astrophysical flavor triangle measurements for fundamental flavor physics). Also good sensitivity to BSM oscillations in nutau sector.

f. No mention of solar neutrino experiments or coherent scattering experiments
g. Could mention some of the more exotic BSM that is accessible via neutrino experiments (Lorentz invariance violation, quantum decoherence + quantum gravity, large extra dimensions, etc). Longer baselines often support such searches. Or low energy neutrino BSM like magnetic moments, millicharge, etc.
h. "Synergies with HEP" section in "Cosmic Messengers" could include possible new accelerator measurements of hadronic interactions targeted for use in e.g. cosmic ray air shower modelling. For example a proposed solution is 7i6
i. O7

L87LHC run with oxygen. In general astroparticle/multimessenger physics likely to produce major results in coming decades and will need inputs from accelerators for e.g. hadronic interactions to fully exploit.

j. "Realtime" observations between connected observatories will be fundamental to cosmic messengers program (e.g. neutrino, GW, gamma ray etc telescopes alert each other to transient events so all can observe). Not currently covered.
k. Current flavour anomalies and DM searches includes the study of channels with missing energy in the final state. In the report there are no quantitative estimates on the performances in reconstructing such final states with regard to the strawman scenarios proposed.

2. **Influence of different possible [‘strawman’ scenarios] for future colliders will influence the topic of the subgroup? For some topics (EDMs, mu->e transtions, Kaon physics, neutrinos, etc), are actually not concerned with different scenarios.**

    a. General remarks on strawman scenarios:
        i. Some flavour topics can not be directly investigated with large scale experiments. The existence of dedicated small experiment should be

supported and accounted for when discussing budget for the large scale experiment.

   ii. Smaller scale experiments allow young researchers to understand the various/all aspects of an experiment, this may be quite hard in a large scale experiment where you are part of a much bigger chain. Considering this training programs for young researchers in large experiments is crucial
   iii. Large scale experiments will allow strong R&D on the detector and accelerator side (with technological repercussions) from which also smaller scale experiments can benefit (e.g. parassite kaon beams from hadron beams)

b. Neutrino landscape:
   i. Long baseline next generation experiments are foreseen in US and Japan, a third is not needed. European neutrino researchers are deeply involved in the US and Japan projects. New generation of neutrino experimentalists will benefit from these experiences and Europe may be ready to host the next-to-next big neutrino experiments in the future. As is this case with all the future experiments new technology and approaches are needed and therefore very strong involvement of Europe in these projects must be encouraged.

c. On the LFV side, the long term program is mainly through experiments hosted in Europe (PSI) and the US, which are currently limited from muon beam line rate. Depending on the results from the planned experiments, higher rate sources need to be investigated and Europe should encourage such experiments in the future alongside US.

d. Future table-top experiments should be highly encouraged and efforts need to stay strong in understanding the physics outputs from these future accelerators.

3. **From the briefing book, for flavour physics FCC-ee, FCC-hh are presented as a very attractive alternatives. How appealing in terms of innovation, research, etc. is each scenario for your group? Are there any drawbacks you want to mention?**

   a. While the FCC-ee is presented as a clean e-e environment for flavour physics, it is not as clean as the accelerators colliding around the Y(4S) threshold. You still produce two b-jets, and not an entangled B-Bbar pair. Flavour tagging performance is not as good as the B-factories, and also the centre-of-mass constraint is not as accurate for the B from Z -> bb production. I think it would be fair to show the expected flavour tagging performance for Bs mesons.
   b. The FCC-hh solution is only ideal to flavour physics if it is shown that flavour physics can be done with extremely high pile-ups, as the HEB solution for the heavy-flavour experiment will ideally still deal with a pile-up of about 500. In that sense, the requirement for additional innovation and research for this solution is certainly present, but it’s uncertain whether it’s feasible. Certainly the research for

the Upgrade-II of LHCb will be very useful, which is yet to be completely approved. On the other hand, the leap in instantaneous luminosity is required to justify the additional costs, as we already recorded 300/fb of p-p data by then.

c. I think it's important to emphasise that the FCC-ee will not be competitive to the results from the upgrade II of LHCb, except from a few channels in which the background rates are deemed too high. Only with the use of a dedicated HEB experiment can we resolve e.g. |q/p| for the B system.
d. In the absence of a clear path forward for many areas, the cost of a huge new general purpose accelerator should be compared to the option of many small, more targeted experiments.
e. FCCee, to start prior FCChh, is considered a good opportunity since technology for FCCee is almost mature while the one for FCChh is not, moreover having FCCee will allow R&D and real date in a shorter timescale.

4. **More collaboration b/w theorists and experimentalists?**
Good collaboration between theorists and experimentalists is crucial to discuss the interpretation of the available experimental results and to plan future measurements of interest. In small groups/experiments (specially MEGII @ PSI), theorists are next door, discussion are frequent.
In some experiments, theorists are part of the collaboration. They know about experimental results before they are published. (Even if they cannot use those prior they are made public, they have some advantages wrt other theoreticians).

5. **More platform for collaboration b/w different experiments to share resources (e.g. computing, statistical analysis techniques, etc)?**
This exchange happens in small scale experiments (the same people working in different experiments). Workshop on machine learning for HEP at CERN already happening and in IEEE conference perhaps (so technical presentations in IEEE like conferences should be encouraged).

6. **In some experiments, in order to be eligible as author, you have to perform service tasks what do you think about that?**
You may end-up doing things that you don't like but, on the other hand, you have the chance to learn new things you wouldn't have done, you serve your experiment in doing tasks that probably almost nobody wants to do but are necessary to the collaboration.

7. **How about opendata?**
In some neutrino (and cosmic ray) experiment, final level data are made public. May be a good idea after the other context, once the analysis has been finalized by the collaboration which has collected the data themselves. Against publishing raw-data, not correctly understandable from out-of-collaboration people.

8. **Small experiment vs big experiment to get a position.**

If the “figure-of-merit” to have a position is the number of paper, people from smaller experiment are disadvantaged. It depends anyway on the funding agencies and from country to country.

9. **What about projects outside Europe? Are ECRs willing to stay long-term away?**

   May be very attractive but it depends on the family situation. But for young scientist, staying long-term outside of Europe was not considered as a problem or a disadvantage. Ofcourse, people with families will/may have a different opinion to this.

10. **Satisfied participation of europe in global projects?**

    Generally yes.

11. **Training of young PHD students should be highly encouraged.**

    In some collaboration, starter kits are held and they’re very helpful for new-comers. The idea of this can be extended to other bigger or smaller collaborations.

12. **Human factors, such as:**
    a. **Support for people moving out of research.**
    b. **Reconciliation of family and a scientific career.**
    c. **Work-life-balance.**
    d. **Gender aspects.**
    e. **Recognition within the collaboration and in the field**

    There wasn’t enough time to discuss these issues in much depth (maybe on 15th Nov). However, the general opinion was that each collaboration (big or small), need to have a dedicated representative to deal or provide support with the issues faced by the members. Such representatives need to exist at experiment/collaboration level rather than institute level (maybe at institute level too). For smaller collaboration, some of these topics could be merged.

13. **Other topics of the six WG in existing ESG (such as Sustainability and environmental impact), travel considerations for non-European collider, etc.**
    The Sustainability WG has a stronger statement to make regarding this. Therefore was not discussed.

14. **Outreach improvement to various audience from layman tax-payers to experts.**
    Paper accompanied by video to explain the big picture are encouraged (for experts maybe a 15 min conference talk, indeed resources need to be available for this). Some experiment public pages are available at the moment, but the style and contents are not very attractive for the public audience. Masterclasses in place for many experiments (both HEP, neutrinos, cosmica ray experiments), this has produced understandable

material for broad audience (but above all for students). To get students to do physics and join a given experiment, outreach activities at institute level could be performed. Physicists who are interested in outreach activities should have the chance to receive some education on that, e.g. during their PHD. Professionists in communication, with some knowledge in physics, should support physicists in outreach activities. Use of technology (Virtual reality), to spread scientific knowledge to the wide audience.

15. **Is the recognition of talent, when working on software/technical projects fair within the collaboration?**
    In smaller collaborations, the feeling was that is was fair. However, we did not discuss this aspect in depth.

16. **Career trajectories of people working in different fields?**
    The proposed experiments within and outside of Europe, if approved will open up many opportunities in the field for young researchers and Europe should therefore strive to be the hub for high/low energy physics. Maybe this aspect was not discussed in much detail in the meeting.

Minutes from Julian

- Concern: no comments have been raised about the ILC or CLIC (although some studies for the ILC have been done).
- Comment on the potential problems due to high pile up for FCC-hh: maybe the running conditions can be well tuned for the specific physics goals, such that there is no issue to deal with in the end.
- Comment on the importance of cross section measurements in cosmic ray experiments.
- The importance of having a heavy-ion program for the future should be better outlined.
- The generality of the briefing-book statements does not help to decide among the strawman scenarios.
- Missing in the briefing book: real time networks for cosmic messengers, gamma rays …
- Comment that some of the “small” experiments are not that small: they require important funding. This is specifically beneficial for flavour.
- Question about the motivation of people for going for a big hadron collider (that takes a lot of time). Is the expected increase in precision good enough to make it worth?
- The importance of the flavour anomalies should be better outlined, both at the experimental and at the theoretical level.

Minutes from Nils

Ask ourselves: We need precision in EW sector, and hh machine for heavy ion people. Is this enough motivation for a 100 km tunnel?

Reach of cosmic messengers will increase a lot in the future. This can be done with heavy ion colliders at higher energies (see below for explanation).

Diversity and smaller exp. not necessarily small... For flavour physics this will profit most from diversity, i.e. needs smaller projects and money for R&D.

Forthcoming anomalies in B sector. Both theoretical and experimental efforts should go here now, important for theory and experiments. Emphasise this in the briefing book.

Explanation of point about heavy ion beams as input to high energy cosmic messengers (Christian Bierlich):
High energy atmospheric leptons sit on a very large background of leptons produced in a cascade induced by protons (with energies up to PeV) hitting the atmosphere consisting of Oxygen and Nitrogen. Simulations of such backgrounds are dominated by uncertainties in total cross sections and particle production models, which are constrained at much lower energies, and then extrapolated. A future high energy run including pO or pN would work to greatly decrease otherwise irreducible systematic uncertainties coming from such simulations.