

Short Summary of Comments on Recognition Summary Report

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I. General comments:

The report contains very valuable and carefully analysed information related to the recognition of individuals in large collaborations. In particular, it contains a list of issues and best practices.

For two communities (astroparticle and nuclear physics) this kind of survey and analysis were realised for the first time.

Indeed, the ECFA community was generally more aware of the issue. A (follow-up) survey to the full JENAS community can be considered.

However, we feel that for the presentation of the final recommendations to the collaborations and communities a more concise document should be prepared. This should as well be approved by APPEC, ECFA and NuPECC.

The main document is intended to be 12 pages, which includes:

- "Stating the issues (1 page)",
- "Best practices"
- "Controversial ideas"
- "Conclusions"

The remainder of the document (Pages 13-43) is an appendix reflecting the feedback of the collaborations. This does not necessarily have to be included in the document but was added to reflect the specific feedbacks we received from the collaborations. How long a document did you have in mind?

Throughout the document, APPEC should be written with capital letters, only.

→ OK

II. More specific comments:

Page 4:

"The answers to the questions include a spectrum of feedback from the collaborations but also show the opinion of a majority. It is noteworthy to stress that the responses exhibited very similar patterns between each community, and the major driver for differences seemed to relate not to the field, but rather to the size and the stage of completion of the collaborations."

This is an important conclusion but the differences related to the size of the collaborations (where the ratio of small to large collaborations are different for the three communities) should be emphasized more explicitly already at this stage.

Indeed it is our intention to stress the *size* of the collaboration, more than the research field. Do you suggest we should try to make it more numerical?

One could already comment here on differences in the publication culture of large and small collaborations; we also appreciate that it is mentioned that there is awareness on recognition issues already in the large collaborations.

The fact that there is awareness inside large collab's is certainly true. In fact they are very constructive, support the efforts and look forward to feedback.

Page 5:

"This makes the evaluation of individuals difficult for referees. This is an issue for referees inside our field,"

We are surprised about the statement that there is an evaluation issue also for referees inside our field. Is this true? Can this be backup up? We think that in a very large fraction of cases referees inside our field are fully aware about the criteria, contributions of individuals and how to rank them.

Concerning backing up: this is hard as we report and reflect on feedback of collabs received. Although criteria may be clear to persons inside our fields, it is still difficult to judge individuals as their contributions to work can be hard to judge. It is often base on statements of the applicant.

For referees outside our field, the issue is clearly more problematic.

Related to the third bullet: It is not clear if "technical" work includes software, computing and data curation activities.

Yes, it certainly does. We well clarify better.

Page 6, Publications:

"The use of full alphabetical author lists is a widespread practice. It comes from a long-established tradition to recognize all stages of work within the publication, and to make sure that people involved in the technical work or early stages of an analysis chain get proper recognition."

This statement is not true for most of the nuclear physics collaborations outside of LHC as well as for astronomy-dominated collaborations in astroparticle physics. The PhD students or post-docs performing experiments and analyzing data are as a rule the first authors (including CERN collaborations like ISOLDE or n-tof). This fact is well documented in Appendix C but it should be repeated here as it concerns a large number of the collaborations participating in the survey.

OK, we can clarify.

"The reference could be: "XXX thesis in preparation, Univ. of YYY"

We doubt that unpublished theses ("in preparation") can be referenced in journal publications. At least it should be made clear here whether this is suggested for journal publications or for other types of publications.

We realize that perhaps the "in preparation" can be difficult. For published theses it can be encouraged.

Page 6: Talks&Conferences

Since you list best practices, we would like to suggest to mention as well the attribution of talk using a ranking method based on individual contributions to the work of the collaboration.

OK: Perhaps we were not clear enough and we could add a sentence. Although systems do not always have to be very formal.

Page 8:

Again, it should be more specified what you mean with technical work, in particular if it includes software, computing, data curation.

Absolutely intended. Will clarify.

Page 9:

There are also numerous prizes for young scientists) awarded by other bodies than collaborations (committees including, EPS, IUPAP and NuPECC, labs, conferences etc.). They might be mentioned here as additional criteria in the evaluation of individual achievements. The collaborations should be encouraged to send applications for these prizes rewarding young collaborators.

We can add the additional prizes.

The same applies to the ERC, MSC and other grants which are not mentioned in the report. A question related to how these prizes and grants are recognized by collaborations can be added in the survey.

Good point. We also give the feedback (food for thought) that prizes are sometimes felt to induce unnecessary competition. Also there may be "political" arguments ("not again institute/country X").

Page 10 and 11:

Also in this chapter a short discussion on specific differences between large and smaller collaborations should be given.

Could be done, but with which message/context specifically?

On the publication of backup documentation we see two serious issues:

- (i) A wealth of information would require huge efforts to be reviewed inside the collaborations and put it in such a form that it can be published; this would bind large resources and would make the publication process even longer and more heavy! As also noted in the report, it is already felt that the publication process in large collaborations is already long and heavy today.

Indeed this feedback was discussed and it is a possible downside. Publication issues should be shortened, not lengthened. Requires some more inventive mechanism if it is done.

- (ii) It would to a large extent provide a duplication of results. Would one find journals to accept them? If on the other hand “online publication” (as a public link to backup information) is planned, this should be clearly said in the document;

It was not our suggestions that this would have to go via journals. Perhaps indeed online, or by submission to arXiv, or something else. We have no ready-made solutions here but hope that people in the field may be triggered and come up with good ideas.

- (iii) The argument of “tension with publicly funded research infrastructure” may not be valid, as the major results are published. In addition, e.g. in HEPDATA details on results are provided in electronic form. As usual, the collaborations must decide what the relevant results to be published are. Not every single plot that is produced deserves to end up in a journal and one needs to focus on the main results.

This touches a part of the discussion that can be delicate (and also somewhat formal/legal, which we want to avoid here). We note that what we write is the input from the collaborations themselves: not our committee’s opinion. It is felt by various people in large collaborations that they may want to publish more individually.

- (iv) We would also like to point out that some of the large collaborations explicitly allow that internal (backup documents) can be shared with selection committees etc. for job applications. We think that this could as well be listed under “best practice” as it mitigates the problem of publication of internal / backup documents.

This seems like a good practice. I do not remember of it came up in the discussion. Can be added.

Page 11:

Technical papers on technology used in subsystems of large experiments are often published by sub-collaborations. More recently, this is the case for computing and software algorithms used in the experiments. The fact that this is not considered possible for publications of physics seems contradictory ...

The discussion on physics publications with short author lists has been extensively carried out in the large collaborations and the conclusions were to stick to the alphabetical ordering. This is also confirmed by what is written on page 29 (first paragraph). So, we do not think that it is useful to open this discussion again.

We do agree that for physics this has been discussed and the long alphabetical author list is most supported. At the same time we are now seeing a new development that collaborations are producing *very large number* of papers (eg ~1000 papers of a collaboration). One could perhaps wonder that with large proliferation of papers and results it has to be true for *all* papers. In CV’s individual people are becoming hesitant to claim authorship of such large number of papers and they themselves make selections.

Some of us argue that an author should be able to defend the since of any paper he/she publishes. This is far from realism in the current situation.

Furthermore, it is written on p29: " ... Other collaborations consider and implement publication of technical papers with limited author lists, perhaps making use of partial data sets or Monte Carlo data."

So, also this is not decided yet, as it is written "considered". So, based on this, we do not think that this should appear so prominently on page 11, unless you phrase it more carefully.

In fact, our text was not fully correct: collaborations already do it. We can make sure to reflect the correct state of affairs.

Page 11:

The title "Conclusions and recommendations" is somewhat misleading as the most important recommendations are indicated in Chapter 4b not in the Chapter 5. This chapter contains only some general conclusions.

Indeed, it does and we can reconsider this. 4b lists specific recommendations. We can just say 'Conclusions' or equivalent.

Page 35:

In total 32 collaborations from NuPECC were initially contacted, out of which 22 joined the working group. The questionnaire was answered by 9 collaborations, these answers form the basis of the following analysis

This is definitely an issue for NuPECC requiring improvement in the next survey.

"...as well as a small collaboration of NUSTAR@FAIR returned a questionnaire".

Well, NUSTAR is probably the largest nuclear structure collaboration in the world (>1000 members). The sentence should be modified.

OK

Dear Marcel, all:

Since I cannot participate because of lecturing, I am sending my comments to their comments here by email.

I Their general comment:

It would be good to know how concise they would like the document to be, e.g. 2-3 pages?

II Page 4:

I am not sure I understand their statement „where the ratio of small to large collaborations are different for the three communities“

NuPECC ranged from 50-814

ECFA from 60 -3000

APPEC from 40-1500

So its more the upper number that differs (by a factor of 4). All of the large collaborations have a similar governance structure, but I understand that the

factor of 4 matters for the visibility on an individual, irrespective of the community.

Of course, we could mention the differences in the publication culture of large and small collaborations, already here.

II Page 5:

* I do think, it is also difficult for referees in our field. Of course, we understand the alphabetical ordering but we often need to rely on statements made by applicants about what their personal contribution to specific papers has been. An if we don't want to, we need to get access (request) additional independent information.

* of course, we can define better what we understand as „technical work“

II Page 6 Authorlists:

* Our statement surely is true for PANDA, CBM, HADES which are the larger collaborations in NuPECC.

Astronomy dominated papers in fact use first author + rest alphabetical (as we have stated later)

Of course, we can clarify this already at this point.

* I agree, a thesis in preparation is not good and often not accepted by publishers. Once a thesis has been finished, it should be considered a good reference, IF publicly available.

II Page 6 Talks:

agreed

II Page 8:

Agreed

II Page 9:

Agreed

Page 11 and 11:

I don't think, we recommended publication of internal notes. We rather pointed out the negative aspects of it. Is there perhaps a misunderstanding?

Page 11:

I let you decide how to take that criticism and I don't have strong emotions here. On the one side, I definitely see the important advantage of FAL-papers in alphabetical order which are a proven technique helping to maintaining piece in a collaboration. On the other hand, my personal scientific ethics tells me (and I was educated this way) that I should be able to scientifically and/or technically defend any paper in public that I have decided to sign, at least at its fundamental level. With up to 700 papers/year it is definitely NOT possible. So what is the point of signing papers that I don't understand, to which I have not directly contributed, and to which no-one would give me any credits for?

Cheers and have a nice discussion,