

Vos et al.

## Summary

Firstly, I would like to comment that the authors' response is not particularly kind to reviewers in our attempt to determine if the requested changes have been made. Whilst areas of change have been highlighted, none of the responses are linked to the changes (e.g. line numbers in new document), and my major comments 2-4 with a single comment that is essentially '*Read the new Methods section, and we've made a selection of changes throughout*'. I request that, in future, the authors please empathize more with the reviewers.

I appreciate the addition of a Methods section; this is simplistic, but GC allows for a pragmatic and *ad hoc* data collection methodology. Attention has been paid to my detailed comment. I am afraid, however, that the authors have failed to make progress with respect to my criticism about 'novelty'. In short, it is not clear from the writing that the outputs of a similar workshop, with identical findings, was not published last year in the *Journal of XXXXXX*.

This could be fixable relatively simply through a number of actions - see detail below (i) a few sentences, (ii) a little stylistic tweaking in places including the abstract, and (iii) some detail to your recommendations. But, this remains a major point in terms of the presentation/framing of the work.

In light of how the response was presented, I started re-reading the abstract, including re-reading the initially submitted abstract. At the end, despite various changes e.g. a '*list of concrete recommendations*' I still have no idea about the novelty in this work. To be specific: How many of these recommendations are new, and how many are simply repeats of previous similar workshops? In reality, if none are new, and all are simply repeats of suggestions in previous work (i.e. our problems have not gone away since 2015), this is fine. But, I personally believe it is necessary to give due credit to past 'state-of-the-subject' workshops and similar if they exist, or be clear if they do not.

The following bullets are suggestions, which I intend to be constructive to fix the 'novelty' issue on the assumption it is presentational (i.e. there isn't a recent similar identical paper).

- Is it that there are no previous/recent/relevant attempts to summarize views on this subject? If so, please state this. If there are, please add a couple of sentences to outline what they are, giving references.
- Is the contribution of this paper that it has a small but convenient literature review that 'describes the progress of open weather and climate science in the context of open science developments in general'? (OPEN SCIENCE section). This could have value.
- Is the contribution a snapshot of expert opinion? (TOWARDS OPEN WEATHER AND CLIMATE SCIENCE section). This will have value if it isn't re-inventing the wheel, with a few sentences are no added to demonstrate/assert that this is not the case.
- You could partially fix my concerns about a disconnect/lack of awareness of previous views of challenges/issues by adding some kind of categorization to your list of recommendations (e.g. N = new, R = recent - perhaps last 2 years, O = ongoing/long-term). A simple round-robin e-mail to the co-authors asking them to assign these, then going with the majority, would work.

I apologise if the tone of this review is 'grumpy'. I think it is fair, but it highlights the potential value in making life easy for reviewers (i.e. some of them may not make the effort to get over their initial mood).

I look forward to re-reading this relatively soon,

John