Second review for: Biogeosciences Discussions

Title: The Southern Annular Mode (SAM) influences phytoplankton communities in the seasonal ice zone of the Southern Ocean
Authors: Greaves et al.
Reviewer: Damiano Righetti, 12 March 2020

Summary of changes implemented:

A substantial effort has been made towards improvement of the ms. Several remaining points may further improve the quality of the ms and can be fixed. One corroborating statistical robustness test is encouraged.

Major remaining point:

It needs to become clear to the reader in the paper's discussion section whether the data shown support a *likely* or potential impact of SAM on phytoplankton community composition or whether they actually *demonstrate* such an effect. I think the former is the case, not the latter. The data at hand (i.e., 52 samples, spanning a relatively narrow geographic area of the total sea ice zone of Antarctica) and the methods used (correlational inference) cannot demonstrate any direct effect of SAM on phytoplankton composition in the Southern Ocean *yet*. To achieve this, future studies need to be based on wider data coverage and experimental incubations. On the other hand, I believe that the study establishes a valid hypothesis, i.e. that a SAM-phytoplankton linkage exists in the ice zone of the southern ocean, and this hypothesis is statistically supported by the (still rather limited) evidence for 12 out of 22 taxa.

The level of support or demonstration with regards to the main conclusions in the paper should be carefully re-checked, e.g.:
(1) Line 475 ff: "The present study demonstrates, for the first time, that variation in the SAM influences the community composition of phytoplankton in the SIZ (…)".
(2) Line 480ff: "We found that the Southern Annular Mode influenced phytoplankton community composition in the SIZ (…)". And, finally, in the same paragraph
(3), 485ff: "These observations suggest that the phytoplankton of the SIZ are indeed susceptible to changes in the SAM".
While the first two statements are rather optimistic on the basis of the relatively sparse data at hand (and hence might be clarified to: "This study demonstrates a statistical association between a large fraction of taxa and SAM… or " This study found statistical support/evidence for the influence of … Or ."SAM explained phytoplankton composition better than…"), the last statement appears very valid. In sum, I suggest being a bit more careful with any statements about what the study demonstrates. Note that the SAM itself is a proxy for several drivers that, in turn, influence the composition of phytoplankton. Also, SAM explained 13.3% of variation in composition (as stated in the abstract), while 86.7% remain unexplained.

I recommend checking/adjusting the strength of the conclusion made in line 303ff to the strength of conclusion made with regards to community composition statements (examples above), as exactly the same number of samples (data power) was used there (but measuring chlorophyll).

Minor points of critique and suggestions:

- The authors mention several times that it is the first study to show an influence of SAM on phytoplankton composition in the SIZ of the SO. This does not need to be stated more than once. The value of the study is emerging from a careful presentation and interpretation of results, rather than the claim of novelty (and the study's novelty, ideally, already emerges from the literature review in the introduction).

- I think a remarkable piece of evidence is that the study finds a statistical association between community composition and SAM, but only for "SAM August" and "SAM Spring", unlike not for "SAM winter" (when there is sea ice cover). This point may deserve more space in the paper's discussion section. Second, if I understood correctly, a similar pattern emerges from the independent, remotely sensed Chl data, providing additional support to the ineffectiveness of "SAM winter".

- I have criticized during the first Review that there was a lack of clarity in several instances, and that the argumentation lines or paragraph structures were partially broken. While it is the task of the authors to make sure that these points are fixed, I provide a few more inputs herein that were apparent during the revisit of the ms:

- Clarity of SAM definition line 71 ff: i.e.: "The SAM is estimated either from station measurements as the difference in normalized zonal mean atmospheric sea-level pressure between 40◦ S and 65◦ S (Gong and Wang, 1999; Marshall, 2003), or from Principal Component analysis of gridded data of atmospheric pressure or temperature, at sea-level or at a geopotential height (Ho et al., 2012)."
For the reader, it is
(a) unclear if air pressure at 40° S is subtracted from the air pressure at 65°S - or vice versa (or if the absolute difference is taken) - and
(b) unclear, if the Principal Component method involves a comparison of the same latitudinal positions or not. Please clarify.

- Clarity of community composition definition: Phytoplankton community composition (as used currently in the paper) both denotes the number of taxa found and their relative cell counts/contribution to the total cell counts in a sample. Hence, species identities and their abundance are both included in the definition. I hence, advise against using "taxonomic composition" in line 233 or "community taxonomic composition" in line 241 as synonyms, as they mean something else (e.g., contribution of (larger) taxa to the community). Please be consistent in use of terms.

- I acknowledge the authors' effort to split the 22 taxa investigated under the microscopes into test groups. Obviously the information available was insufficient to perform such a test. I hence, do not insist to include lines 391-395 in the ms. (Yet, I agree that it is a noteworthy point that a significant fraction of the taxa in the study are endemic to the region and/or lack observation records in OBIS from other regions).

- I proposed to put primary productivity in the SIZ into the context of global primary productivity to give the reader a sense of the importance of the study region. This has not been implemented. The sentence now reads as follows, line 34 ff: "Total productivity within the SIZ of the SO has been estimated at 68–107 Tg C yr^{-1} from 1997 to 2005 (Arrigo et al., 2008), corresponding to roughly one third/fourth… of the

…. Tg C yr produced globally, and consequently SO phytoplankton play a role in mitigating the accumulation of anthropogenic greenhouse gases in the word's atmosphere (…).
Could the authors include a statement (see my red textual edits) to provide global context? Only this would enable the reader to grasp the global relevance of the SIZ.

- Caption of Figure 2, second line. Taxa codes listed in Table 3 not Table 2, I guess.

- CAP method explanation: It would be helpful – at an appropriate position in the paper –to briefly explain to the reader how community composition was related to environmental covariates in CAP. The reader does not intuitively comprehend, if (a) community composition is reduced to a single expression and then related to the environmental covariate, or if (b) the abundance values of each species are related to the covariate, simultaneously, in such CAP analysis. Please clarify.

- Comment: I wondered why the clustering of community samples was used as a strategy to test the hypothesis of SAM influence on composition. If I understand now correctly, it served to provide additional context to the data shown in Figure 6 and hence served as a complementary analysis.

- Results section: Line 223: This sentence interprets the results. In the results section, I suggest to present the (main) results first, without interpretation. Also slightly confusing: Cluster analysis is introduced here as a method.

- Statistics: I acknowledge that the authors present *p* values in many instances. In Figure 5, I suggest to present *r*-squared values inside each panel, rather than *r* values, as *r*-squared values reflect the variance explained, which is often referred to elsewhere in the paper. (Please also stick to "variance", or "variation", in the ms).

- Replicates per sample: Does figure 5 show sample means (of the three replicates per sample)? If so, could standard deviations be added to each dot to depict how much variability was involved between replicates per sample? The reader gets no sense at the moment about the within-sample variability/uncertainty between replicates.

- Line 41ff: Indeed, it has been estimated that productivity declines by 1%. Yet the paper has been disputed. I hence suggest using e.g. past tense, not present, and to refer to a criticizing paper (e.g. brief communication by David Mackas: DOI 10.1038/nature09951) as well.

- Line 21vs line 61. Contradictory.

- Line 153: Has a parallel analysis using absolute abundances instead of relative abundances, been attempted as a robustness test? (I.e., How many taxa do show positive associations with SAM in this case?). I strongly recommend running such a parallel test in order to demonstrate the statistical robustness/sensitivity of results.

- Line 221: Figure 3. Could the same x-axis scales be used in both panels? Slightly confusing why panel a) uses months and days while panel b) uses days only.

- Line 224: These (plural) vs. other analysis (singular). Mismatch.

- Line 265ff: The sentence is not clear to me, “most variation due to seasonal succession due to”. Perhaps: Most variation *in* the seasonal succession of.. ?

- Line 278ff: I suggest to replace SIMPROF by “similarity profile analysis” or similar, as the reader does not remember what SIMPROF stands for.

- Line 281ff: This is a methodological argument, could this be introduced earlier on? E.g. in the methods section.

- Line 296ff: I do not think that indicators were derived. I argue that relationships were examined or tested by the data. I agree, however, that this methodological statement may help the reader (else it should be omitted in the results section).

- Line 314: I suggest deleting “taxonomic”. It is not the abundance of taxa/species, but the abundance of cells per species or taxon-group that matters.

- Line 324ff: I do not fully comprehend this point. Why should a demonstration of separation of samples into clusters support the conclusion that SAM affects community composition (stated in line 322)?

- Line 356ff: Please specify: Was a statistical association between SAM indices and phytoplankton composition found (?).

- Line 372: Comment: Why not including this expectation (SAM winter has no effect vs. other SAM have effect) in the main hypothesis stated upfront in the paper? If this expectation is stated here, a chance to incept this fascinating idea earlier on to the reader is being missed. The conclusion in line 384 (potentially adjust, see main comment above) may then actually be expected (adjust if needed).

- Line 388ff: This sentence is rather long. I do not get the key point / essence of it.

- Line 404ff paragraph: What does this paragraph mean/suggest? Can a final sentence or statement summarize the message to the reader?

- Line 432: “that” is missing in this sentence.

- Line 435ff: The first phrase (i.e., “The maxima in the variance in total chlorophyll explained by the SAM …” is hard to understand. (Rephrasing possible?).

- 4.4. Implications. Line 446ff This first paragraph provides an excellent embedment of the study’s result in the literature. However, this seems to be a key aspect of the discussion, rather than an implication of the study. Also the paragraph in line 463ff seems to be rather a discussion point than an implication.

- Line 463: “It is not surprising that climate in both autumn and spring influence (…)” Climate or weather mode? The sentence treats the hypothesis that is established in the paper as granted. Please see my point above on the degree of the study’s conclusion.

- Line 468ff: “The surprise is that (…)”. Wasn’t this expected based on the hypothesis of the paper?