

Dear authors, dear editor,

Thank you for your response to my earlier comments, and for all the edits to the manuscript. The authors have made substantial improvements in avoiding speculation outside of the discussion paragraph, providing essential details on the processing of data to ensure transparency and reproducibility, and have even added novel analyses and supplements. There is now a clearer explanation of the assumed mechanisms behind the observed patterns. Improvements have been made in phrasing more clearly which statements are interpretations, and which are actual findings of the study.

I see the scientific value in this large-scale and holistic evaluation of climate – vegetation growth relations that the authors provide for Kalaallit Nunaat / Greenland. I think Biogeosciences is an excellent platform for such a large-scale and interdisciplinary endeavour.

I do however still have several concerns and in my vision the manuscript is not publishable in its current form. I hope this next revision round will be helpful to get this work published and I would like to kindly ask that the authors are a bit more thorough this time. All line numbers refer to the text without tracked changes.

Please keep working on this! Wishing you lots of success!

Major to moderate concerns

1. I agreed with the second reviewer that the aims reported in the introduction, the methods, and the conclusions mentioned in the abstract and conclusion, were not yet fully aligned. While improvements have been made, several issues still remain. See minor comments. I particularly have a concern about the second aim: "We examine the combined effects of bio-climatic indicators ranging from sub-surface factors (such as soil water availability) to above-surface factors (such as the thermal growing season, heat stress, and frost) with summer spectral greenness." (L. 114-116). I would assume that you mean "combined effects [...] ON summer spectral greenness" rather than "[...] WITH spectral greenness"? More appropriately, I would write "association with" rather than speak of "effects on" in this correlative study. In my opinion, this would be more aligned with running a PCA that includes greenness. If the aim is to study combined effects of bio-climatic indicators on greenness itself, then a multivariate method that models greenness as a response variable (such as pls regression) would be more appropriate.
2. The other reviewer and I both indicated that improvements were necessary throughout the ms in terms of sentence structure, grammar and language errors. Many improvements have been made, but there are still too many examples of grammatical errors and typo's, particularly in the newly written text. I have made many suggestions in the previous round of editing, but ultimately I believe reviewers should focus on the scientific aspects of a ms, and not provide free language editing services. Well-structured and unambiguous sentences are necessary to convey complex scientific matter to the broad and interdisciplinary readership of a journal like biogeosciences. The authors will have to do thorough language checks, ideally by a native speaker, or potentially use AI based language improvement tools. This also goes for any newly edited or added text.
3. The authors have added extended definitions of the different terms they use for various manifestations of greenness. This does not mitigate the fact that there is an excessive amount of different terms in use throughout the ms, and that in some cases two or more different terms are used for the same thing. Please add specific and singular definitions under 3.1. I would assume that you would need only about 3 terms ("greenness" -> seasonally averaged NDVI, please add time range in months. "seasonal duration of greenness" -> amount of months within the season that NDVI was > 0.15. and perhaps another term along the lines of "green pixel" to indicate pixels with NDVI > 0.15...?). Perhaps use either "greenness" or "spectral greenness" but

not both. These are suggestions of course; the bottom line is that all terms need to be defined clearly, and that there needs to be a stricter limit to the amount of different terms in use.

Example: L. 213-2014 "Finally, we calculated a seasonally averaged NDVI, hereafter referred to as spectral greenness and interchangeably as green vegetation" -> it is unnecessarily confusing to use two very different terms for the same thing.

Another example: the terms "extent of green vegetation", "green vegetation extent" and "spectral vegetation extent" are all used interchangeably. Please pick one. All the different terms in use really mean and imply different things from ecological perspectives and remote sensing perspectives and correct terminology matters in this context (if necessary, consult review by isla myers-smith, 2020, already cited). Please critically evaluate the whole ms including figures labels, tables and captions for greening terminology, simplify and realign thoroughly.

4. The authors replied that they disagree with my request to add scale bars for "greenness" in figures, since it refers to a 32year average of the mean JJA NDVI. If you judge that a 32 year average is not informative, then I would wonder why you choose to depict it at all. Please either remove the figures completely, if you judge that the greenness patterns are not useful for interpretation, or add numbers. As a reader, I want to know how to place this information in the context of panarctic ndvi values and trends, data from different sensors, saturation values and the thresholds that you use for defining "greenness". It is also basic cartography and potentially a journal requirement that continuous scale bars include numerical labels.
5. The authors replied to my earlier request to implement thematic discussion points in the ms by explaining (only in the response) what the paragraphs are about. Please make sure that this rationale is actually visible within the ms itself. You could for example add sub-headers for several main lines of interpretation so that the reader knows what is going to be discussed, and what is relevant for them to read, instead of seeing one large page of text. Many readers will not read the discussion from beginning to end, but rather focus on specific aspects that are of interest to them. Please implement a more logical flow to the information presented in the discussion. (And a general request: make sure all comments are met by changes in the actual ms itself and cite the line nrs. Or explain and argue why no changes were made).
6. Paragraph 5.1, general: I now better understand the different dynamics of earlier season, slower melt of shallow snowpacks, and later season melt under warmer conditions in deep snow. Thank you for elaborating. This helped a lot. Throughout paragraph 5.1, I could not help but wonder to what extent the observed increases in soil water content in spring (MAM), and association thereof with greenness, are not simply a result of the soil's increasingly unfrozen state (due to shallow snow that melts early, as well as warming earlier in the season). Would this not also result in an increasingly unfrozen state of Greenlandic soils in spring, corresponding to higher water content and lower ice content in spring? Or can you provide additional argumentation, that clearly shows that increased spring soil water content is indeed the result of slower melt dynamics of shallower snow packs? This needs more argumentation.

Minor comments

1. L. 105: here "permafrost thaw" was replaced with "ground thaw", for reasons that aren't clear from the response. I find this new term unnecessarily vague, and I would recommend to change it to "permafrost thaw" or "ground ice degradation", depending on whether the authors are referring to seasonally frozen water resources in the active layer, or availability of new moisture resources from degrading, ice-rich permafrost.
2. L. 119 "greenness distribution"; seasonal or spatial distribution? Such nuances are important, especially given the many different terms and definitions of greenness that are used throughout the manuscript.

3. Line 133-136: Thank you for adding this critical information on how soil water and ice content are derived. Can you also add information on the accuracy of these subsurface components of the re-analysis products? Otherwise it remains a bit of a black box whether the reported associations between snow, soil water and greenness variables accurately reflect real-world processes, or whether they simply result from the way that the different sub-models are defined (with risk of circular reasoning).
4. L. 161: "likely signifying areas with no vegetation presence". I think this part of the sentence should be removed; vegetation can be present under clouds, cloud shadows or seasonal inundation. Or even under snow (for instance, bryophytes and evergreens under snow may not be photosynthetically active at that moment, but they are still there).
5. Table 1) one of the indicators in "SWE_{max}", but it just gives the snow water equivalent at any give moment as far as I can judge from the definitions. Don't you mean "SWE" here, as also referred to later in the ms? If you mean maximum SWE, add to the definition that this is a seasonal maximum?
6. In a more general sense, it is rather inconsistent among Table 1 and the text under 3.2 whether and how seasonal integration/averaging is mentioned. This is explicitly mentioned in the table for variables like T and greenness, but not VPD (for which it is only mentioned in the text). Please find a way to make this clearer. I recommend mentioning your definition of what "seasons" are before the table, then listing descriptions in the table and adding a column to the table that explicitly states whether it is an annual variable (starting when? Previous autumn or previous winter?) or a seasonal variable, and which seasons are considered or not (e.g. some are only calculated for spring/summer and others only for winter, which makes sense).
7. Line 285-286: "The combination of this region's complex topography with frequent cloud cover resulted in its exclusion from the analysis". Many outcomes for this ecoregion are still reported (Fig. 2, Fig. 4 and associated text, discussion). Please explain why such findings are still presented despite the limitations mentioned here.
8. Line 303-310: This is background information on the NAO and GBI, not a method. Please integrate into the introduction of the role of NAO/GBI in the introduction and use the methods section only to describe what you did yourself.
9. L. 313 – 315; again use of the phrase "influence with", which is grammatically incorrect and methodologically confusing. PCA cannot demonstrate influences, only associations. It also seems incorrect to me to speak of an influence when greenness is treated as one of the variables going into the PCA, rather than a response variable.
10. L. 331: "This will diminish noise"; please avoid different tenses in the methods. Everything else is in past tense, so please rewrite to past tense and check tenses throughout the manuscript for consistency within paragraph.
11. L. 354-355: ", due to the typically shallow snow cover". This is an example of an interpretation in the results that is not backed by a figure or a statistic. Please avoid such interpretation while reporting the outcomes of your analyses, or back them up with evidence. Please recheck the results section for such interpretations.
12. L. 359-360: "Correlations between green vegetation extent and summer GBI are investigated for three periods: AVHRR (1991-2013), VIIRS (2014-2023) and the full period (1991-2023), and are shown in Table S1.": example of methods, mentioned under results. Please move to methods.
13. L 360 -370: many grammatical issues (tenses, plural/singular). Please carefully check whole ms.
14. L. 375-395: some variables are removed from PCA analysis based on their degree of association with greenness. This is a methodological choice that needs to be described and backed up in the methods, based on *a priori* informed criteria. No threshold value (in correlation or p value) is mentioned at all, making the choices seem arbitrary (even though they are probably not). Please describe all choices and criteria in the methods (as I requested in the previous revision round), so that here you can stick to reporting the outcomes, and you only need to mention which variables make the benchmark for inclusion.
15. L. 396: please rewrite all instances of "influence with" and clarify in the aims whether this analysis is meant to show influences (= impossible with PCA) or associations.

16. L. 427 – 434: Here new analyses (methods) are introduced in the results. Please move to methods and strictly report outcomes in results.
17. L. 440: "strips" instead of "stripes"? Strips of what?
18. L. 471-473: another example of methodological information in the results. This is already described, in different wording, in Line 210-ish. Please integrate this information there and report only the outcomes here. Adding just a little sentence as a reminder of what you did as a guideline for the reader should be ok, but restating the whole processing approach is excessive.
19. L. 476: as I mentioned in the previous round of revisions, calling this "expansion of vegetation" is very misleading to all readers with an ecological background, since vegetation expansion is more or less exclusively used for spatial expansion. If this is about temporal expansion of photosynthetic activity, then please name it something else, like "extension of the growing season".
20. Table 2: the methods state that this is to be referred to as "changes in greenness distribution" later in the ms, but this does not seem to be implemented, since the results only mention greening "expansion" and "shrinkage", interchangeable with "vegetation" expansion/shrinkage. Or sometimes "reduction". This is an example of the major comment on greening terminology. Please use uniform terminology.
21. Discussion, general: There are many grammatical errors in the newly added text. The discussion really needs to be evaluated for language.
22. L. 499-505: This paragraph needs backing from literature (on the blocking events) and reference to figures and tables.
23. L. 518-529: Please tone down your statement that observed dynamics are a consequence of permafrost thaw (this is an interpretation, and cannot be described as a causal influence without further backing).
24. L. 531-533: Here you mention "spring (winter)" and "winter (spring)". Please elaborate on what you mean by this. Do you mean something like "in winter, and to a lesser extent in spring"?
25. L. 595: Why shrubs, specifically, and not other plant functional groups? Please explain in the ms.
26. L. 628: "longer roots". Please write "deeper roots". Roots can be long without extending deeply. Perhaps write "deeply rooted species" (not only graminoids) and cite papers on actual root development under warming rather than this model study (van der kolk) based on assumed vegetation growth parameters rather than actual observations. Suggestion: <https://besjournals.onlinelibrary.wiley.com/doi/abs/10.1111/1365-2745.12718>
27. L. 630: "This ecological shifts might will also affect".
28. L. 660-661: "Such periods have favoured exceptional vegetation growth across western ecoregions as shown in our results". In your previous response letter you stated that cloudiness prevented you from actually inferring whether warmer and more humid conditions during such events are indeed associated with greening. So I am a bit confused now. Please refer to the specific results that support this claim.
29. L. 682-685: These are implications, not recommendations or limitations.
30. L. 688: "effects with", please change to "effects on", or rather, "associations with".
31. L. 693-694: "This slow snowmelt rate allows the ground to retain more liquid water during the ablation period". I find this too much of an interpretation to belong in the conclusion. See also major comment nr 6. If you want to keep it here, please add something like "We interpret this as [...]" so that readers (who sometimes only read the conclusion!) do not assume that this is an actual finding resulting from your study design.
32. Fig. S7: This seems like a copy of a previous main text figure? Also here, no scale bar for greenness and the caption still mentions place names, which aren't in the figure. Please recheck.
33. Fig. S14-S22: please change "confident levels" to "confidence levels".