We would like to thank the editor and the reviewers for the constructive comments. In the following, we addressed the referees' comments point-by-point. We marked the comments given by the referee in red, provide our answers and comments in black, and indicate how we address the amendments in the manuscript in green.

Tiago Silva, on behalf of all co-authors.

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.

Thanks for your kind lines and we appreciate that you can see the value of our study. We are looking forward to working on the constructive comments to present a mature study.

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.

Thank you for your remark. We apologize for the caused misunderstanding. We agree that rewriting the aim as you suggest is an important clarification. Indeed, we examine the associations among the bio-climatic indicators with summer greenness. i.e. how these factors co-interact with greenness.

By adjusting LN116 in the following manner in the Introduction as: We examine the associations among bio-climatic indicators ranging from subsurface factors (such as soil water availability) to above-surface factors (such as the thermal growing season, heat stress, and frost) with summer spectral greenness. We also extend our study of bio-climatic changes beyond the summer by examining indicators from the preceding winter and spring and assessing their combined interactions with summer spectral greenness.

In the *Interconnectedness among bio-climatic indicators* in LN405: Note that these physical features are constant through time and were not considered when investigating the combined associations among bio-climatic indicators with greenness in the PCA.

We agree that this subtle change has clarified the focus of our work.

After revising the manuscript, we did not find any other instance where the relationship between bio-climatic indicators and greenness could lead to similar misunderstandings.

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.

Thank you for your remark. We made yet another and even more thorough grammar and sentence structure check in the revised version and we hope to have improved the readability of our study. We apologize that the initial submissions did not meet the expectations.

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.

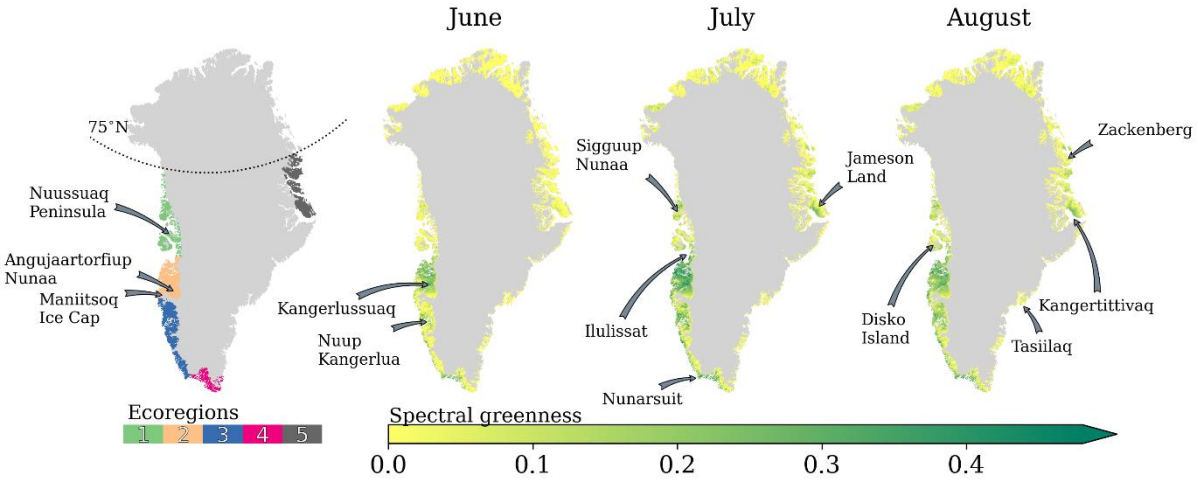
Thank you for the comment and suggestion that really improves comprehensibility. We revisited every term related to greenness in the manuscript and sticked to three terms, that are now defined in the subsection *Spectral greenness*. We will keep using greenness as defined by Isla Myers-Smith et al. (2020), greenness extent and greenness distribution (LN227).

Since we perform calculations for temporal and spatial changes, in the revised version of the manuscript we indicated that: Pixels exhibiting a monthly NDVI of 0.15 or greater are indicative of monthly greenness. The area derived from this monthly greenness is defined as the greenness extent. Additionally, we calculated the summer average greenness (see subsection 3.2 for the season definition), which we will refer to greenness hereafter. We also assessed spatio-temporal changes in the greenness extent between the periods of 2008--2023 and 1991--2007. We described these comparisons as changes in the greenness distribution, where an increase in greenness distribution is characterized as an expansion and a decrease as shrinkage. In addition, we analysed temporal changes in greenness (more details about trend analysis provided in subsection 3.4), wherein positive trends denote as greening, and negative trends denote a reduction in greenness.

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.

Thank you for the comment, which makes us realise that we were not clear enough in our earlier answer. We wrote in point 15 of the previous point-by-point answer that we "kept the delineation of the ecoregions and the greenness evolution during summer in the *Methods*, as they will support the readers to understand the geography of the ecoregions and to recognise the greenness dynamics across Greenland from June to August as well as what entails the summer averaged greenness."

In the revised version, we add the numbers to the scale bar correspondents to the 32-year monthly averaged spectral greenness.



Additionally, to avoid further misunderstandings and help with the interpretation of averaged spectral greenness in Figure 1, we now write in subsection 3.3 that: In Figure 1 we also show the 32-year monthly averaged greenness for summer months. As mentioned in subsection 3.1, the typical NDVI analysis that consist in averaging either the entire NDVI range or selecting the maximum NDVI are more prone to artifacts. Therefore, the 32-year monthly averaged greenness here shown is not necessarily based on 32 values in every pixel. This is reflected by the monthly averaged greenness over 32 years to be lower than 0.15 in many regions. While the 32-year monthly averaged greenness spatial variability can be assessed with Figure 1, direct quantification of greenness saturation should be taken with care given the interannual variability in greenness. Maps with the correlation coefficients between greenness and North Atlantic Oscillation (NAO) index and the Greenland Blocking Index (GBI) between 1991 and 2023 are shown in Figure S7.

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).

Thank you for the comment which supports the clarity of our work. We revisited the discussion in subsection *Key findings and interpretation in the context of the current literature* and add the sub-sub-section titles as indicated in the point 32 of the previous point-by-point document: 5.1.1. Changes in greenness extent; 5.1.2. PCA performance and basis for interpretation; 5.1.3. Northern ecoregions; 5.1.4. Southern ecoregions; 5.1.5. Changes across ecoregions, 5.1.6. Atmospheric drying in the interior of ecoregion 2; 5.1.7. GrowDays elevation dependence explained; 5.1.8. Spatio-temporal changes in greenness and in bio-climatic factors reported in literature.
Also, we added line numbers at most of our replies and indeed made sure that all information present in the response to referees also ended up in the revised manuscript.

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.

Thank you for your reflection and for pointing to a missing explanation. We acknowledge that the reduction of ice water content in spring is not elaborated in the manuscript and now added to the respective subsubsection *Changes in the Northern ecoregions* in LN555:

After investigating the relationships among changes in SWE_MAX, MeltRate, SoilWaterMAM, SoilIceMAM, and greenness, and examining the levels of SoilIceDJF, we find no significant trends in SoilIceDJF. This suggests that to a certain extent, the proportion of frozen ground has been restored during the cold season. Changes in SoilWaterMAM are moderately proportional to changes in SoilIceMAM, indicating that the increase in the liquid water content in the soil during spring primarily originates from snowmelt. Subsequently, the presence of liquid water in soil with higher thermal conductivity, coupled with shallow snow depths (and eventually snow-free conditions), allows for a more efficient exchange of energy between the surface and the atmosphere, consequently leading to ground thawing.

These processes can be better understood by a deeper analysis of the interplay among sub-surface variables at multiple depths on a daily scale.

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.

We apologize for the unnecessary change which came from an attempt to generalize but indeed, it is clearer to stick to permafrost thaw which we do in the newly revised version.

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.

Thank you for the remark. This point is answered in major point 3.

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).

We agree, even though there is not that much literature available showing the performance of CARRA for Greenland from validation studies. However, there is the study of van der Schot et al. (2024) validating snow depth/SWE of CARRA against independent observations in Greenland and showed a promising performance of CARRA simulating snow depth/SWE. This is quite an extensive validation, as snow depth and SWE are the result of several other atmospheric variables including air temperature and precipitation.

In addition to the snow depth/SWE validation from van der Schot et al. (2024), we also add a new paragraph in the subsection *Copernicus Arctic Reanalysis* to inform the reader about a set of model schemes and parameterizations implemented in SURFEX7.2. In LN139, we wrote: The snow and frozen soil parameterizations from the ISBA (Interactions between Soil, Biosphere, and Atmosphere) scheme, as described by Noilhan and Planton (1989) and implemented in the SURFEX7.2 (Masson et al., 2013), have been tested in model intercomparison campaigns across northern Europe (e.g., Luo et al. 2002; Slater et al. 2000), high latitudes (Decharme and Douville 2006), and the Alpine regions (e.g., Decharme et al. 2016).

The physical parameterizations within the ISBA have seen progressive developments over the past decades, particularly in its snowpack scheme, Crocus, which accounts for various snowpack features — such as thickness, temperature, density, liquid water content, and grain types — and incorporates physio-geographical attributes like the surface slope. Crocus has been consistently coupled with global reanalysis like ERA5 (e.g., Ramos Buarque et al., 2025) and other atmospheric models (e.g., Luijting et al. 2018). When integrated with the atmospheric model AROME, Crocus accurately reproduced the evolution of the snow surface temperature over Dome C (Antarctica) during an 11-day period (Brun et al., 2011), and it has effectively represented snowpack features in the French Alps (Vionnet et al., 2012) for more than a decade.

Regarding surface and subsurface parameterizations, ISBA scheme explicitly calculates the actual ice and water content in the soil to determine the heat capacity and thermal conductivity of the ground. The ground thermal conductivity depends on the surface and soil heat fluxes, which in turn are dependent on the soil scheme. For soil schemes with vegetation, ISBA allows roots and organic matter to favour the development of macropores which can lead to enhanced water movement near the soil surface. To our knowledge, accuracy data for SURFEX schemes when coupled with AROME-HARMONIE are not yet available.

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).

Thanks, we correct the sentence to: Negative NDVI values are typically associated with water, clouds, or snow, with no spectrally visible vegetation.

5. Table 1) one of the indicators in "SWEmax", 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?

Thank you for the remark. The definition of SWE_MAX in Table 1 is now written as annual maximum mass of liquid water from melting the snow per unit area. This is now in agreement with the text and with the minor point 6.

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).

Thank you for your comment. We added to the description of the Table 1 whether a certain bio-climatic indicator corresponds to annual or seasonal statistics (count, average and sum). We also moved the paragraph explained the season definition before Table 1.

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.

Our previous statement refers to greenness in Southeast Greenland specifically, where we refrain from interpretation throughout the manuscript. Southeast Greenland is not defined as an ecoregion (please see Fig. 1) and therefore not represented in the above-mentioned figures. Only the output from CARRA is available and shown in Figure 5 along the Southeast coast.

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.

Thanks, we deleted this background information on the NAO and GBI from the *Methods* since climate oscillations are already well described in the Introduction.

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.

Thank you for the remark. We replaced the noun "influence" by "interaction" not only in the mentioned sentence, but in the other instances where "influence" is used in the same context. The referred part is now changed to: Principal Component Analysis (PCA, Pearson 1901; Lorenz 1956), often used on remotely sensed and environmental data (e.g., Mills et al. 2013; Yan and Tinker 2006), was employed to investigate the combined interactions among bio-climatic indicators with summer spectral greenness.

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.

Thanks for the remark. We revisited the *Methods* and corrected the verb tense to past tense.

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.

Thanks for the comment. We already described the climatology of certain environmental variables upon the delineation of the ecoregions in the sub-section *Ecoregions*. We consider that this information was already backed up. Therefore, add: (see subsection 3.3)

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.

Thank you for your remark. We consider this piece of information more suitable for the current *Results* section, as the statistical output is used for interpretation of the results. However, we added to the *Statistical Methods* in LN355 that: We performed correlation and trend analysis in three periods: AVHRR (1991--2013), VIIRS (2014--2023) and the full period (1991--2023) between greenness and climate oscillations to assess their statistical strength and tendency as dependent on the sensor period.

13. L 360 -370: many grammatical issues (tenses, plural/singular). Please carefully check whole ms.

Thank you for the remark. We scanned the manuscript once more to correct grammar issues.

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.

We are sorry that the selection of indicators was not clear enough. The selection is covered in both *Statistical Methods* and *Interconnectedness among bio-climatic indicators*.

In the subsection *Statistical Methods* we write: The calculated correlations are displayed in a correlation matrix, and bio-climatic indicators with similar correlations are sorted with hierarchical clustering. This helped to visually discern bio-climatic indicators with comparable statistical relationships and supported on the empirical reduction of indicators accounting for the relevant physical and ecological processes on the tundra ecosystems, later used as part of the PCA. This aimed to diminish "noise", redundancy and ultimately boost the clarity of interactions across atmosphere-biosphere-cryosphere.

In subsection *Interconnectedness among bio-climatic indicators* we write:

SoilIce is largely negatively correlated with the volume of water in the soil (SoilWater). Therefore, we decided to arbitrarily use SoilIce in winter (SoilIceDJF) and summer (SoilIceJJA) and SoilWater in spring (SoilWaterMAM) and autumn (SoilWaterSON) in the further analysis. Additionally, SnowDays and DegreeDays are not used since both are highly explained by GrowDays. While DegreeDays accumulate T2m during GrowDays, SnowDays complement FrostDays and GrowDays -- together, they represent snow-free occurrences when daily T2m is negative and higher than 1°C, respectively. Strong correlations between Rain and RainRatio are found in spring and autumn, but not in summer. Consequently, we will retain both Rain and RainRatio variables exclusively for the summer. Finally, MeltRate is removed as it is physically explained by the snowpack depth.

We hoped that this point was already clear in previously revised version. In order to improve clarity, we added to the *Statistical Methods* in LN350*:* Certain bioclimatic indicators exhibited high correlations among them, primarily due to physical reasons. Other bioclimatic indicators corresponded to complementary quantities. Consequently, the selection of bioclimatic indicators for the PCA was made on an arbitrary basis further detailed in subsection 4.1.

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.

Thank you. This instance was already considered in minor point 9.

16. L. 427 – 434: Here new analyses (methods) are introduced in the results. Please move to methods and strictly report outcomes in results.

Thank you for the remark. We move this paragraph to the methods.

17. L. 440: “strips” instead of “stripes”? Strips of what?

Thank you for the remark. We corrected the sentence to: Along the narrow ice-free strips of land in the Southeast, there is a modest increase of GrowDays (approx. 5 days per decade), at several elevations around Tasiilaq.

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.

Thank you for the remark. The paragraph is now simplified and found in the revised version: In order to assess which regions became greener due to greenness expansion, we detected whether a pixel met the summer greenness criterion annually from 1991 to 2023. A detailed explanation on how the study period was split into two to investigate changes in greenness distribution is found in sub-section 3.1.

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".

Thanks for the remark. We already elaborated this concern in major point 3.

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.

Table 2 caption is now written: Percentage of expansion and shrinkage of greenness distribution, and ratio (fraction of expanded by shrank area) between 2008 and 2023 with respect to the period 1991–2007 in % of the total ecoregion area. This also considers the feedback from major point 3 and minor point 19.

21. Discussion, general: There are many grammatical errors in the newly added text. The discussion really needs to be evaluated for language.

Thank you for the remark. We corrected the grammatical errors.

22. L. 499-505: This paragraph needs backing from literature (on the blocking events) and reference to figures and tables.

Thank you for the comment. The same paragraph written in the revised version is:

Greenness extent has increased over time across Greenland, with an increase rate of 2\% per decade in ecoregion 1 to up almost 6% per decade in ecoregion 4 (Figure 2). When comparing the recent half of the time-series (2008--2023) to the earlier half (1991--2007), the distribution of greenness has also changed. In ecoregions 3 and 5, the distributions of greenness expanded to nearly double and eight times the size of the areas that shrank, respectively (Table 2). Within the time series, maximum greenness extent was observed in 2019, aligning with the end of a period of frequent, long-lasting and intense summer atmospheric blocking conditions in the

vicinity of Greenland, conditions which promoted advection of relatively warm and humid air from the North Atlantic along West Greenland (Silva et al., 2022).

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).

Thank you for recognising our over-interpretation. We rewrote the paragraph as follows:

Our study found that areas related to expansion in the northern ecoregions appear to be associated with a rise in SoilWaterMAM along with declines in both spring soil ice content trends (SoilIceMAM) and maximum snow depth (SWE_MAX). In Northwest Greenland, including ecoregion 1, regional exceptions of widespread increases in SWE_MAX with regional delays in the onset of the thermal growing season (Onset) are found along coastal areas and are not related to greening. Conversely, areas related to greening are statistically linked with rising SoilWaterMAM, accompanied by higher spring temperatures (T2mMAM) and earlier Onset. Despite regional trends on higher summer rainfall amounts (RainJJA, Niwano et al. 2021) in northern Greenland, we did not find a clear link between greening and changes in RainJJA.

Interestingly, trends in summer soil water content (SoilWaterJJA) and soil ice content (SoilIceJJA) are both negatively related to near-surface air temperatures in summer (T2mJJA). This could result as a consequence of surface thawing and subsequently increased evaporation caused by higher vapour pressure deficits in these northern areas (Fig. S22). The greening of the recently emerged vegetated areas in the northern ecoregions respond to different seasonal soil water contents. Greening in ecoregion 1 correlates best with SoilWaterMAM patterns, similar to the remaining southwestern ecoregions. In contrast, ecoregion 5 is more closely connected with SoilWaterJJA, likely due to a later onset of the GrowDays.

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”?

Our apologies for the confusing sentence. We write in the revised version: Despite an increase in fresh snow accumulation and a reduction in drought days during the spring, the observed declining trend in SWE_MAX for West Greenland is linked to a decrease in winter snowfall; conversely, for East Greenland, it is attributed to reduced spring snowfall.

25. L. 595: Why shrubs, specifically, and not other plant functional groups? Please explain in the ms.

Therefore, we may argue that the spectral greening is generally related to vascular green vegetation expansion throughout the past three decades, as early proposed by Sturm et al. (2001).

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

Thanks for the comment. We write “deeply” instead of "longer” rooted systems and use the recommended reference.

27. L. 630: “This ecological shifts might will also affect”.

Thank you for the remark. We have corrected the sentence to: These ecological shifts might 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.

Thank you for the remark. It seems that the answer from the previous letter was not implemented in this point. We correct the sentence to: Such periods have favoured exceptional vegetation growth across western ecoregions as shown in our results. However, surface reflectance retrievals may have been impacted by cloudiness, partly hindering the spatio-temporal changes in spectral greenness.

29. L. 682-685: These are implications, not recommendations or limitations.

Thank you for the remark. We moved this paragraph to *Significance and implications*.

30. L. 688: “effects with”, please change to “effects on”, or rather, “associations with”.

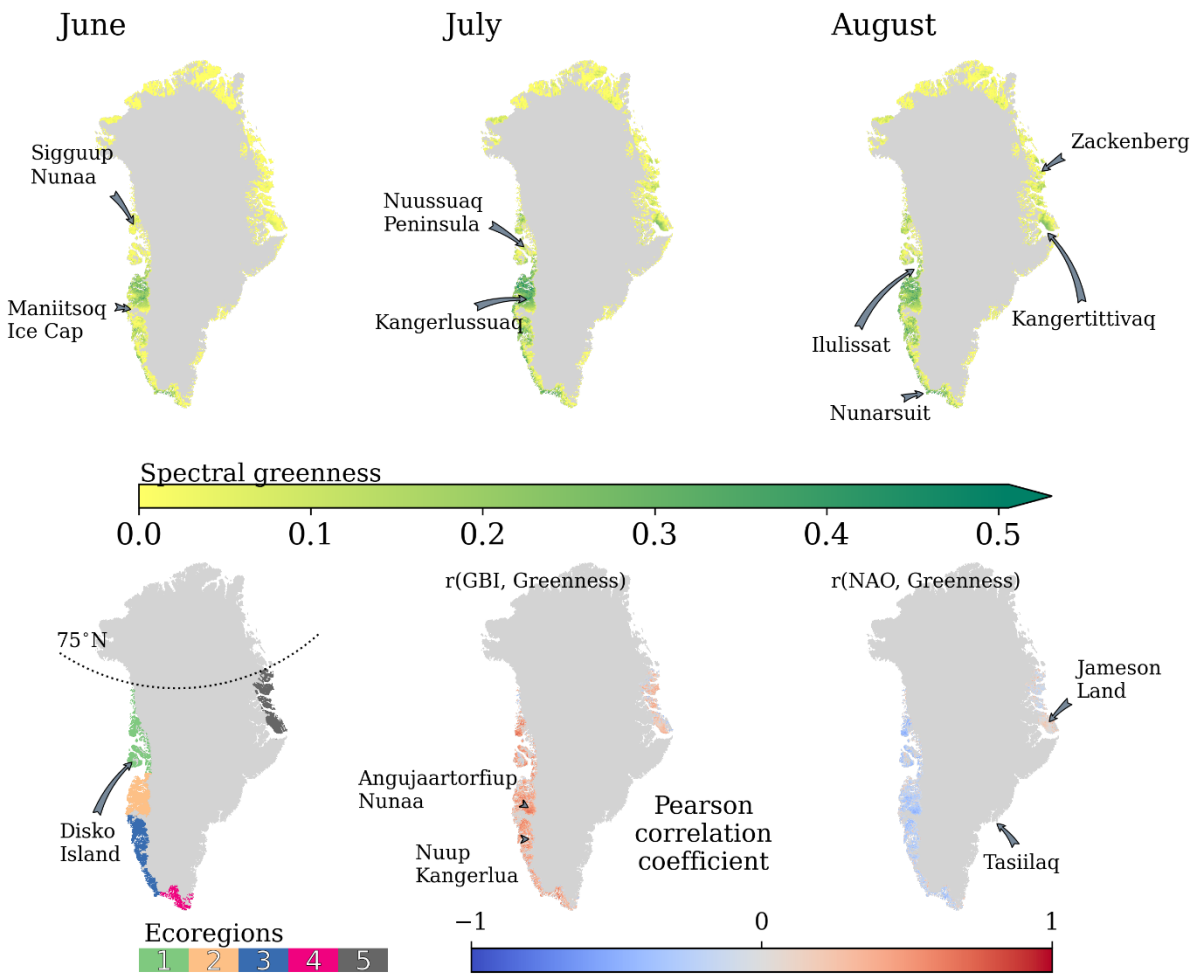
Thank you. This instance was already considered in minor point 9.

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.

Thank you for the comment. We start this statement as suggested by the referee.

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.

Yes, Figure 1 from the initial pre-print was moved to the supplementary material, where the correlations maps of summer greenness and climate oscillations are shown. We changed the colour bar as mentioned above in point 4 and kept the sentence in the caption with the placenames by showing them again.



33. Fig. S14-S22: please change “confident levels” to “confidence levels”.

Thank you for the remark. The word “confident” was now changed to “confidence” in the supplementary figures with trend maps.