In the following, we will address the referees' comments point by point. We mark the comments given by the referee in red, provide our answers and comments in black, and indicate how we addressed the amendments in the manuscript in green.

Tiago Silva, on behalf of all co-authors.

RC1: 'Comment on egusphere-2024-2571', Rúna Magnússon, 20 Oct 2024

Dear authors, dear editor
Thank you for inviting me to review "Bio-climatic factors drive spectral vegetation changes in Greenland", by T. Silva et al. for Biogeosciences. The manuscript explores the role of a wide range of bio-climatic factors in explaining satellite-derived vegetation dynamics in Greenland. The authors aimed to identify which sub-surface and above-surface climate factors were associated with greening in Greenland, and how such associations differed among ecoregions and latitudinal and altitudinal gradients. They report that increases in the duration of the thermal growing season show the strongest association with greening, with additional influences of snowpack dynamics, and differential strength of association across regions and altitudinal and latitudinal gradients.
This study is relevant in the context of rapid ongoing climate change in the Arctic, observed dynamics of "Arctic greening" and their implications for the future functioning of tundra ecosystems. The authors analyze a substantial amount of data for a large region, using various sources and environmental disciplines in a holistic and, generally, appropriate way. The manuscript is well within the scope of Biogeosciences and presents a relevant and timely case. I do, however, have several major concerns about some of the methodological choices and the structuring and argumentation of the work. I advise a round of thorough revision and rewriting of substantial parts of the manuscript before it can be considered for publication. This has resulted in a rather lengthy review report, but I would also like to stress that many of the points I raise are interrelated or specific examples of the major points, so I hope it is not discouraging. I am sure the ms will find a good home in a respected journal.
I have performed this review together with 7 MSc students for an open review course assignment at Wageningen University. Their help has been valuable, and they appreciated the opportunity to learn from this ambitious and relevant paper, and to contribute to the scientific publishing process. We have all enjoyed this activity and we wish you all the best as the manuscript comes to full maturity!

Rúna Magnússon,
with input from Annika Robben, Djordy Potappel, Aron den Exter, Muriël de Vries, Rikuto Shinagawa, Yente Reniers and Yorick Kwakkel.

Thank you very much for your comments. We address each of the points raised in more detail below.

Major comments

1. I hope the authors can make clarify how the potential mismatches between AVHRR and VIIRS NDVI products (e.g. masking differences) have been accounted for during statistical analysis and trend detection. Explanations on how this was done are sparse and not sufficiently clear to understand the implications.
Beside adding the shaded min-max range in Fig. 2 (that I also don't fully understand the procedure behind, can this be clarified?),
how did you prevent the use of two different records and sensors from affecting your temporal trends? And especially, how do you prevent this from unduly influencing the comparison between 2008-2023 and 1991-2007, that you describe in L. 380-392? This appears to be based on counts of NDVI > 0.15, where differences in bandwidth and snow/water/cloud detection easily become problematic. Miura et al. (2012) may be an appropriate source to evaluate the validity of trend detection across two satellite platforms, and you may want to statistically test for absence of trend breaks coinciding with the switch from one platform to another.
Thank you for raising these concerns.
This is a very important point, namely the homogeneity of the two NDVI time series to each other and what this means for a calculated trend.
The NOAA Climate Data Record (CDR) of AVHRR NDVI - Version 5 and the NOAA CDR of VIIRS NDVI - Version 1 are developed by Eric Vermote and colleagues (Vermote et al. 2018 and 2022) for NOAA's CDR Program. Both records have been processed considering the same atmospheric characteristics as in Miura et al. (2012) and both processed records are posterior to Miura et al. (2012) proposed correction. However, the correction proposed by Miura et al. (2012) is not assessed in polar regions, which may contribute to additional uncertainties in our study.
Unfortunately, no overlap periods are available for the parallel measurements of the two satellite sensors. Therefore, no systematic differences can be determined. However, as we state in *NOAA Climate Data Record for Normalized Difference Vegetation Index* there is work that has calibrated the utilized AVHRR product with MODIS (e.g. Franch et al., 2017) and thus improved the internal homogeneity of AVHRR, as well as work that has established the homogeneity of VIIRS with MODIS (Skakun et al., 2018) to improve the consistency of the NDVI datasets. This does not yet achieve perfect homogeneity, which we explain in the description of both products in *NOAA Climate Data Record for Normalized Difference Vegetation Index as follows*:
According to AVHRR and VIIRS technical reports, the NIR channel is centred at different wavelengths (830 nm vs. 865 nm). As there is no overlapping period available in the NOAA CDR, potential mismatches between AVHRR and VIIRS NDVI cannot be discarded.
We have also revised the same subsection in general to better understand the processing of two NDVI data sets and the problems of data homogeneity. We also reformulate in *Spectral greenness* how spectral greenness is derived from the AVHRR and VIIRS NDVI to better explain how the shaded area later shown in Figure 2 is calculated:
As estimates integrated through time are less likely to be influenced by temporal sampling artefacts at high latitudes than metrics based on maximum NDVI (e.g., Myers-Smith et al. 2020), we started by calculating monthly integrated NDVI. Also, since our focus is on green vegetation, only daily NDVI pixel values with higher or

equal to 0.15 are considered. Then, we divide the monthly integrated NDVI by the total number of monthly observations (n, see Figure S1 for the interannual variability of n) to obtain the monthly NDVI. However, before 2014 and as described in Subsection 2.2, the AVHRR algorithm was less strict in its data quality control compared to VIIRS from 2014 onward, resulting in higher n before 2014 that lowers monthly NDVI. To address temporal heterogeneities, we adjusted n from the AVHRR period with the number of monthly observations acquired during the VIIRS period. From 2014 to 2023, we identified the minimum, maximum and average number of observations for each month. Hence, using these three quantities, we generated a consistent variability range from 1991 to 2013 to recalculate monthly NDVI, considering a similar number of observations as from 2014 to 2023. This procedure assumes that the environmental conditions (i.e. snow-cover, clouds and shadow) between 1991 to 2013 are similar to those between 2014 and 2023. The maps for the average number of monthly observations and the associated standard deviation for AVHRR and VIIRS period before and after the adjustment regarding n are shown in Figures S2-S5, respectively.

We investigate at the start of *Results* how summer spectral greenness statistically relates with climate oscillations (e.g., Greenland Blocking Index) for AVHRR, VIIRS and the entire study period. We use these climate oscillation time-series, that are homogenous and independent of spectral greenness, as a reference to evaluate systematic inconsistencies that may arise due to sensor change.

It should be noted that prevailing weather patterns during summer months, like the North Atlantic Oscillation (NAO) and the Greenland Blocking Index (GBI), are highly correlated with spectral vegetation (Fig. S7). Therefore, summer weather patterns can accelerate or delay the maximum green vegetation extent given their link with temperature and precipitation. 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. Positive and significant correlation coefficients ranging between 0.5 and 0.8 are found between ecoregion 1 and 4, generally with higher correlations for VIIRS than for AVHRR period. Green vegetation extent in ecoregion 5 is poorly correlated with the prevailing weather patterns during summer.

While the AVHRR 22-year trend evidence general expansion of green vegetation, the VIIRS 9-year trend evidence decreases, particularly in West Greenland (Table S2). However, due to high variability and small sample size, most trends in both periods are not significant.

We address in *Study limitation and future research*, our concerns about the reliability of long-term time integrated NDVI analysis

The NDVI datasets employed in this study are sourced from two satellite products processed by NOAA, each utilizing a different type of sensor. Due to the absence of a temporal dataset overlap, the assessment of uncertainties was limited and potential for mismatches between the datasets cannot be discarded. This lack of a common calibration period raises concerns about the reliability of long-term time integrated NDVI analysis.

In the end, we follow similar approaches of recent literature (e.g., Madson et al. 2023, Pourmohamad et al. 2024) that make use of the full AVHRR NDVI and VIIRS NDVI without additional corrections.

2. Your methodology is ambitious and extensive, which is laudable. It does however lead to many choices during the processing of the data, and not all of these have been properly backed or described yet. Examples include (1) the use of 0.15 as an NDVI threshold without a reference, (2) the described use of CryoClim data that only go to 2015 without any visible inclusion of these data throughout later analyses, (3) why has only altitude, and not for example slope aspect, been included into the study? These, and further examples, are given in the minor comments. I suggest that the authors critically go through every step in the methodology and check whether all choices are described in sufficient detail for an independent reader to reproduce the study, and that choices are back-up either by literature, data or statistics. If needed, details on processing can be described in a supplementary methods section to prevent disruption of the flow of the main text.

Thanks for your kind words endorsing our ambitions! We are sorry that relevant information is missing here. This is essential to guarantee reproducibility in future interdisciplinary studies.

The citation for the choice of the NDVI threshold of 0.15 to derived spectral greenness is now expanded in *Spectral Greenness*: Arctic regions are characterized by sparse vegetation, that typically exhibit markedly low NDVI values, often as low as 0.15 (e.g., Gandhi et al. 2015; Liu et al. 2024), with dense shrubs above 0.5 (e.g., Walker et al. 2005), and signal saturation at around 0.7 (e.g., Myers-Smith et al. 2020).

Regarding the CryoClim, we expanded *Copernicus Arctic regional reanalysis* noting that: The data providers assure that the data for the period post-2015 have been produced and arranged in collaboration with the CryoClim developers at the Norwegian Meteorological Institute.

We have taken up the idea of including slope and aspect in the analysis. The results can be found in *Coastal, latitudinal and altitudinal dependence on trends* along with supplementary figures.

The relationship between GrowDays and topographical features such as slope and aspect were further explored. As the surface slope is highly correlated with surface elevation, trends in GrowDays tend to significantly decrease with steepness. The dependence between GrowDays and surface aspect is rather complex, without a predominant slope orientation promoting GrowDays, in general. However, latitudes immediately south of Maniitsoq Ice Cap show increases of GrowDays in slopes with southwest orientation. On the East coast, a western slope orientation is particularly pronounced along Jameson Land, whereas northeast exposure appears favourable north of ecoregion 5.

The dependence of the slope orientation for greenness changes is partly in alignment with the dependence of the slope orientation for GrowDays. Greenness trends increased in two latitudinal bands facing southeast in ecoregion 1 and 2. In Jameson Land a similar tendency for more greening is found towards southwest, while east facing slopes are preferred towards the northern part of ecoregion 5.

3. From L. 227 onwards, it reads as if the distinction between methods, results and interpretation of results (discussion) is lost. For example: results and maps are presented in the methods in L. 227-247. New information on choices of processing, variable selection and statistical tests (Pearson correlations) are introduced in the results in L. 275-305, L. 311-314 L. 380-384 and many other places. Throughout the entire (lengthy) results section, interpretations are added that go beyond the statistical results of your own methods. Lines 320 -350 for instance are very speculative for a results section, and other paragraphs and show similar interpretation or speculation. These would be better suited for the discussion and require backing by references. Please rewrite the methods-results-discussion in such a way that: (1) all methodological choices and tests are explained in the methods (2) only numerical and statistical outcomes are presented in results (with a minimum interpretation to make the results understandable, e.g. writing out abbreviations and description of patterns) and (3) interpretation and relation to unmeasured mechanisms such as permafrost, latent heat processes or photosynthesis are only kept for the discussion.

Thank you for pointing this out. We rewrote the Methods, Results and Discussion as suggested by the referees. To make the differentiation between these sections more evident, we:

1. Rewrote several parts in the subsection *Statistical Methods* and removed the above-mentioned methodological choices from the *Results*.
2. Rewrote several parts in *Key findings and interpretation in the context of the current* literature, removing the above-mentioned interpretations from the *Results* and back up our statistical outcomes with experimental studies.
3. Rewrote the *Results*, properly backing our statistical outcomes with supplementary figures. We apologize if this last point may be seen as too generic, but we show below along the Minor points the amendments made regarding rewriting of the methods-results-discussion, as suggested by the referees.

Several of these revisions are mentioned and expanded in the minor points.

4. In the results section and abstract, observed greenness dynamics are attributed to processes such as nutrient dynamics and permafrost. This gives the reader the impression that such variables were included or that you can at least confidently attribute greening dynamics to such processes. Given the set of bioclimatic factors that were included, however, I doubt whether you can make such claims. These processes can be touched upon in the discussion, with support from literature, but should not be presented in a way that readers might think that these are actual conclusions from this study. I also think that to properly discuss their role in the discussion, you will need to evaluate several lines of reasoning more critically: are the subsurface products (soil water and soil ice) that you include, given the limited representation of subsurface dynamics in the used reanalysis products, actually representative of permafrost conditions or hydrology? How can you better argue the role of snowmelt rates in relation to microbial activity and nutrient dynamics, especially to an audience that may not be familiar with works such as musselman et al.? Because at first it is very counterintuitive that shallower snowpacks melt more

slowly and with the current explanation provided, this line of argumentation is very hard to follow. I suggest you evaluate to what extent your bioclimatic variables are representative of processes such as permafrost dynamics and melt rates and nutrient dynamics, discuss their potential roles in the discussion section, and refrain from making any hard statements about their role in the abstract/results/conclusion sections.

Thank you for valuable comment! To better distinguish our conclusions from interpretations and to follow the advice of the referees:

1. We rewrote the *Abstract*, given more emphasis to our results than certain interpretations.
2. We rewrote parts of *Introduction* to better explain how soil water physically relates to the snowpack characteristics and, in particular, to show the sequence of processes by which a shallow snow cover can influence microbial activity:
   A relevant characteristic of the snowpack is that deep snow requires more energy to equalise the cold content and the liquid water holding capacity to subsequently initiate and sustain melt than shallow snowpacks (Colbeck 1976; Musselman et al. 2017). (...) Concurrently, meltwater from relatively shallow snow percolates the soil more efficiently during the ablation period, in contrast with fast snowmelt that quickly saturates the soil surface and runs off (Stephenson and Freeze, 1974). See also our detailed answer under minor comment.
3. And also, the importance of climate oscillations: Grimes et al. (2024) has recently shown that the doubling of vegetation across ice-free Greenland is linked with warming. The warming observed in Greenland over recent decades has been associated with more frequent and intense weather patterns that promote widespread clear-sky conditions and the advection of relatively warm air masses from southern latitudes along Western Greenland (Barrett et al., 2020). Weather patterns can be related to indices by analysing specific atmospheric variables over time and space. For instance, the North Atlantic Oscillation is driven by surface pressure configurations in the North Atlantic (Hurrell et al., 2003), and the Greenland Blocking Index by the geopotential height in the mid-troposphere over Greenland (Hanna et al., 2016). Both indices are commonly utilized in climate studies to deduce influences on various components of the climate system in Greenland and vicinity (e.g., Bjørk et al. 2018; Olafsson and Rousta 2021).We added more details on CARRA (*Copernicus Arctic regional reanalysis)* regarding the multi-layer surface model (SURFEX) and its schemes as dependent on the surface type: SURFEX is a multi-layer surface model that computes specific schemes dependent on the surface type (e.g., vegetation, soil, snow), allowing soil water phase changes and enabling runoff over frozen and unfrozen soil. This helps to better represent areas with permafrost and ice surfaces in Greenland as they are not well described in the present version of HARMONIE-AROME.
4. We added to *Study limitations and future research directions*, the limitations of CARRA on the representation of permafrost and nutrient dynamics, thus better contextualizing the explanatory value of these variables for the vegetation

changes in Greenland: (...) better representation of the permafrost extent and active layer thickness along with the inclusion of dynamic tundra vegetation models within CARRA could be beneficial in improving our knowledge on interactions among atmosphere, vegetation, carbon and nitrogen cycling, water and permafrost dynamics.

Minor comments

1. The writing could be improved by splitting up some very long compound sentences into shorter ones. I provide some examples in the "technicalities", but I recommend a thorough re-reading for writing style and grammar.

Thank you for this very relevant point. In addition to the examples mentioned in "technicalities", we revised our sentence construction to improve readability throughout the manuscript.

2. The abstract ends with the conclusion that you "identify a set of bioclimatic variables" and that you provide a "basis to validate bioclimatic indicators from climate models". Your conclusions section states more or less the same. I suggest that you reflect more specifically on how exactly your findings help to achieve this (more to the point). This will hopefully also better explain how you advance the field, since the role of growing season onset and snowmelt timing are already well established in Arctic ecological studies.

Thank you for pointing out that we had to further develop our abstract and conclusions to show how our work advances the field. We hope that the revised *Abstract:*

The terrestrial Greenland ecosystem (ice-free area) has undergone significant changes over the past decades, affecting biodiversity. Changes in near-surface air temperature and precipitation have modified the duration and conditions of snowpack during the cold season, altering ecosystem interactions and functioning.

In this study, we statistically aggregated the Copernicus Arctic regional reanalysis (CARRA) and remotely sensed spectral data on green vegetation, spanning from 1991 to 2023. We use principal component analysis (PCA) to examine key sub-surface and above-surface bio-climatic factors influencing ecological and phenological processes preceding and during the thermal growing season in tundra ecosystems. Subsequently, we interpreted spatio-temporal interactions among bio-climatic factors on vegetation and investigated bio-climatic changes dependent on latitude and topographical features in Greenland. Ultimately, we described regions of ongoing changes in green vegetation distribution.

Our results show that green vegetation has responded highly to the prevailing weather patterns of the past decades, particularly along West Greenland. The PCA effectively clustered bio-climatic indicators that co-vary with summer spectral vegetation, demonstrating the potential of CARRA for biogeographic studies.

The duration of the thermal growing season (GrowDays) emerged as the pivotal factor across all ecoregions (with increases up to 10 days per decade), interacting with other bio-climatic indicators to promote summer vegetation growth.

The lengthening of GrowDays is explained by reduced winter precipitation associated with warming (up to 1.5 °C per decade). Significant decreases in snow height occur along with earlier snowmelt (up to 20 days per decade), leading to an earlier onset of

GrowDays. We find that regions with shallower snowpacks, experiencing slower snowmelt rates during the ablation period, are linked with a higher soil water content in spring; this relation not only coincides with the greenest regions in West and Southtwest Greenland, but also with regions where green vegetation has recently emerged. Such processes occur prior to GrowDays and were combined with summer weather conditions that favoured warmer and clear-skies that resulted in significant summer greening.

The relatively warmer and drier summer conditions experienced in the northern and interior of the studied regions evidenced surface thawing and drying. Despite these summer bio-climatic interlinks green vegetation expanded northward and upward. Green vegetation has expanded in Northeast Greenland by 22.5% with respect to 1991--2007 period, leading to new vegetated areas.

We report little to no change in the length and onset of the GrowDays along the coast in Northeast Greenland, in contrast with more pronounced changes inland and at higher elevations, hence showing an elevation-dependent response (increases up to 5 days per decade per km elevation).

Our statistical outcomes and interpretations derived from reanalysis and remote sensing data that include uncertainties, are corroborated by in situ studies conducted in the tundra region. The bio-climatic indicators and the associated insights serve not only as a foundation for validating bio-climatic indicators from climate models to assess future changes in vegetation, but they also advocate for the inclusion of permafrost dynamics schemes. This integration will enhance the quantification of atmosphere-vegetation-permafrost-carbon feedback loops across terrestrial Greenland amid the evolving climate.

and *Conclusions* are now more concise. Our study aimed to better understand the long-term, large-scale interactions among various bio-climatic indicators and their collective effects with summer spectral greenness in ice-free Greenland. This study utilized remote sensing Normalized Difference Vegetation Index and bio-climatic indicators from the Copernicus Arctic regional reanalysis between 1991 and 2023. Bio-climatic changes are influenced by a complex set of factors, not only centered in summer, but also dependent on winter and spring atmospheric temperatures, precipitation, solar radiation, soil properties, and soil water availability.

We conclude that regions under green vegetation expansion in ice-free Greenland are associated with reductions in winter precipitation. The resulting shallower snowpacks melt earlier in the season but slower. This slow snowmelt rate allows the ground to retain more liquid water during the ablation period. Such conditions occur prior to the start of the thermal growing season are mentioned in experimental studies to facilitate vegetation growth. Longer thermal growing seasons accompanied by prevailing summer weather patterns, with its peak in 2019, that promoted warmer and clear-sky conditions over the past decades also contributed to vegetation growth.

The spatio-temporal changes in summer greenness distribution depend on ecoregion, elevation and latitude. Overall, the bio-climatic changes during the study period led to more vegetation expansion, particularly towards the interior and northward. Ultimately, this study encourages the incorporation of dynamic tundra vegetation schemes to improve our knowledge on deeper interactions among atmosphere, vegetation, carbon and nitrogen cycling, water and permafrost dynamics, particularly for future projections.

3. 35 – 43. Several references seem out of place in this paragraph. I suspect you mean Bjorkman et al. (2018) instead of Metcalfe et al. (2018), since Metcalfe et al. (2018) does not deal with the type of findings you describe at all, and Anne Bjorkman's paper does. Sturm et al. (2001) is a rather old and case-specific (albeit popular) reference for shrubification of the Arctic. ITEX papers (e.g. Elmendorf et al., 2012) or syntheses (e.g. Mekonnen et al., 2021; Martin et al., 2017; Myers-Smith et al., 2011) would be more appropriate.
Thank you for identifying misplacement of references. We followed your advice and adapted this accordingly.

4. 66 – 68, this proposed increase in nutrient availability under deeper snow is at odds with your statements in the abstract, results and discussion that shallower snowpacks should melt more slowly. It should be clear from the introduction onwards which snowpack properties can be expected to facilitate faster or slower melt, and how would relate to nutrient cycling. If the literature on the influence of snow dynamics on microbial turnover and nutrient availability is ambiguous in itself, then I would refrain from making any statements about nutrients as a mediating effect between snow dynamics and greenness.
Thank you for raising this point. We improved a few paragraphs in the *Introduction* better explaining the relationship between snowpack characteristics and snowmelt, rate and how they relate to nutrient cycling. The most relevant revised paragraphs in this regard are:
Introduction
Increased snow depth during the cold season usually causes increased plant growth in the following summer, as more snow provides insulation, less frost damage and, depending on the snowpack characteristics, increase in water availability (e.g., Lamichhane 2021; Migała et al. 2014; Wang et al. 2024).
A relevant characteristic of the snowpack is that deep snow requires more energy to equalise the cold content and the liquid water holding capacity to subsequently initiate and sustain melt than shallow snowpacks (Colbeck 1976; Musselman et al. 2017). As a result, deep snow often subsists for longer periods, potentially delaying the start of the growing season, which can hinder plant growth (Schmidt et al. 2019). On the other hand, the insulation provided by deep snow has also been demonstrated to promote increased microbial decomposition, enhancing the nutrient supply for the following growing season (e.g., Cooper 2014; Pedron et al. 2023; Xu et al. 2021). The higher amount of energy input needed to melt deep snow means that it melts later but also faster, which can cause nutrient loss through increased runoff.
Concurrently, meltwater from relatively shallow snow percolates the soil more efficiently during the ablation period, in contrast with fast snowmelt that quickly saturates the soil surface and runs off (Stephenson and Freeze, 1974). These slow snowmelt rates allow water to remain in the soil for extended periods, which is critical

for activating soil microbe communities. These microbes then produce nutrients that are vital for vegetation growth (Glanville et al. 2012).

Results:

These shallow snowpacks seem to be linked to more water content in the soil in spring (SoilWaterMAM loading vector opposite to SWEMAX loading vector). Additionally, the earlier snow depletion and thus earlier onset of the thermal growing season relates to enhanced spectral greenness (Onset loading vector opposite to Greenness loading vector).

In the *Discussion*:

For most ecoregions in ice-free Greenland, we find that snowpacks are becoming shallower, and consequently melt slowly, but earlier in the season. This feature was mentioned by Musselman et al. (2017) and is attributed to global warming. Musselman et al. (2017) explains that in Western North America regions with shallower snow are experiencing snow season contractions. Shallower snow is susceptible to snow season contraction because shallow snow requires less energy to initiate melt than deeper snow. This earlier start of the ablation period occurs at a slower rate due to a combination of near-surface warming with relatively low solar altitude angles. In contrast, for deep snowpacks that require more energy to initiate runoff, it is also more likely for the snowmelt water to refreeze within the snowpack (Dingman, 2015). Therefore, early season slow snowmelt rates in shallow snowpacks allow for efficient soil water percolation and subsequent water storage (Stephenson and Freeze, 1974). The successful percolation of liquid water into soil plays a key role in tundra regions during the snow ablation period and start of the growing season, as during this time soils are generally dry due to high drainage (Migała et al., 2014).

Increased water availability in the soil could stimulate dormant microbial communities and thus increase the decomposition of soil organic matter, releasing soil nutrients (e.g., Glanville et al. 2012; Salmon et al. 2016; Xu et al. 2021). This in turn could prime the soil for earlier and more efficient vegetation growth and colonization. The increased spring soil water content (SoilWaterMAM), spring near-surface air temperature (T2mMAM), and lengthening of the thermal growing season (GrowDays) indicated in our results could therefore improve conditions for plant growth and colonization, especially in the southern ecoregions.

5. 111, here you mention the use of CryoClim data, that was chose to represent daily snow cover rather than the CARRA dataset. I do not see how this could be done since the data only goes to 2015, and this data product is not mentioned anywhere anymore in the remainder of the ms. Did you actually use it and if so, how? Perhaps it is a nice addition to incorporate data sources directly into Table 1 to resolve unclarities like this.

Thank you for the request to clarify this point. CryoClim is one of the data sources/products assimilated by CARRA that does not extend along the entire reanalysis period. Nevertheless, CARRA data providers assure that the data for the period post-2015 have been produced and arranged in collaboration with the CryoClim developers at the Norwegian Meteorological Institute.

We decided to highlight CryoClim in the data description to indicate that a remotely sensed product is used to a certain extent to represent snow-covered regions.

Although CryoClim has not been available since 2015 and thus inhomogeneity in CARRA could be assumed, a similar product has been provided by the Norwegian Meteorological Institute for the rest of the period. Additionally, van der Schot et al. (2024) reports how CARRA performs against in situ measurements until 2023 across Greenland with no obvious change after 2015. Accuracy metrics were provided as suggested in point 10.

6. 128-153: Can you give an indication of the match between AVHRRR and VIIRS? Calibration against MODIS does not seem to be the most relevant thing to mention here, since you do not use MODIS. See Miura et al. (2012), there seem to be some structural NIR differences and non-linear NDVI relationships between VIIRS and AVHRR?

Major comment 1 also highlighted revisions required in this area, please also refer to our response there. The following text is now included in the paper:

1. *NOAA Climate Data Record for Normalized Difference Vegetation Index*: According to AVHRR and VIIRS technical reports, the NIR channel is centred at different wavelengths (830 nm vs. 865 nm). As there is no overlapping period available in the NOAA CDR, potential mismatches between AVHRR and VIIRS NDVI cannot be discarded. However, AVHRR NDVI uses the MODIS Land-Sea mask and its cloud mask is spectrally adjusted using 10 years of MODIS data, with 90% match accuracy over land (Franch et al. 2017). As VIIRS will eventually replace MODIS for land science, MODIS is also used to calibrate VIIRS NDVI estimates (Skakun et al. 2018).
2. *And furthermore, in the Results*: It should be noted that prevailing weather patterns during summer months, like the North Atlantic Oscillation (NAO) and the Greenland Blocking Index (GBI), are highly correlated with spectral vegetation (Fig. S7). Therefore, summer weather patterns can accelerate or delay the maximum green vegetation extent given their link with temperature and precipitation. 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. Positive and significant correlation coefficients ranging between 0.5 and 0.8 are found between ecoregion 1 and 4, generally with higher correlations for VIIRS than for AVHRR period. Green vegetation extent in ecoregion 5 is poorly correlated with the prevailing weather patterns during summer.
   While the AVHRR 22-year trend evidence general expansion of green vegetation, the VIIRS 9-year trend evidence decreases, particularly in West Greenland (Table S2). However, due to high variability and small sample size, most trends in both periods are not significant.
3. *Study limitations and future research directions* that The NDVI datasets employed in this study are sourced from two satellite products processed by NOAA, each utilizing a different type of sensor. Due to the absence of a temporal overlap from the data providers, the assessment of uncertainties was limited and potential for mismatches between the datasets cannot be discarded. This lack of a common calibration period raises concerns about the reliability of long-term time integrated NDVI analysis.

7. 143, why did you use an NDVI threshold of specifically 0.15?
In the revised manuscript we clarify this point in *Spectral greenness* and add the following: Arctic regions are characterized by sparse vegetation, that typically exhibit markedly low NDVI values, often as low as 0.15 (e.g., Gandhi et al. 2015; Liu et al. 2024), with dense shrubs above 0.5 (e.g., Walker et al. 2005), and signal saturation at around 0.7 (e.g., Myers-Smith et al. 2020).

8. 146, can you provide a sharper definition of "interannual extent of vegetation"? To ecologists, this may be confusing since extent almost always refers to spatial extent.
Thanks, we agree, this was formulated in a confusing manner. In the revised manuscript, we sharpen in *Spectral greenness* that Pixels with monthly NDVI equal to or greater than 0.15, representative of the area covered by green vegetation, are used to estimate the green vegetation extent.

9. 147-155. It is very difficult for readers who are not intimately familiar with the AVHRR and VIIRS datasets to follow this paragraph, even though it is quite important for the quality of the results. Terms like "flag" and "n" may be unclear. Please provide more explicit description of exactly how the monthly max/mean/min nr. of valid pixels was used and how this translates to the CI's in Fig. 2. From reading this several times I still did not understand if any correction was applied before further analysis (and looking at Fig. S1 I would expect for that to be necessary).
In the revised manuscript, we clarify in *Spectral greenness*: As estimates integrated through time are less likely to be influenced by temporal sampling artefacts at high latitudes than metrics based on maximum NDVI (e.g., Myers-Smith et al. 2020), we started by calculating monthly integrated NDVI. Also, since our focus is on green vegetation, only daily NDVI pixel values with higher or equal to 0.15 are considered. Then, we divide the monthly integrated NDVI by the total number of monthly observations (n, see Figure S1 for the interannual variability of n) to obtain the monthly NDVI. However, before 2014, the AVHRR algorithm was less strict in its data quality control compared to VIIRS from 2014 onward, resulting in higher n before 2014 that lowers monthly NDVI. To address temporal heterogeneities, we adjusted n from the AVHRR period with the number of monthly points acquired during the VIIRS period. From 2014 to 2023, we identified the minimum, maximum and average number of valid points for each month. Hence, using these three quantities, we generated a consistent variability range from 1991 to 2013 to recalculate monthly NDVI, considering a similar reduction of points as from 2014 to 2023. This procedure assumes that the environmental conditions (i.e. snow-cover, clouds and shadow) influencing the number of data points between 1991 to 2013 are similar to those between 2014 and 2023. The maps for the average number of monthly observations and the associated standard deviation for AVHRR and VIIRS period before and after the adjustment regarding n are shown in Figures S2-S5, respectively.

10. 163, it would be useful to report an accuracy metric here.
Thank you for pointing this out. In in the revised version *Bio-climatic factors*, we write that van der Schot et al. (2024) demonstrate in a recent study that the agreement is

strong between the snow water equivalent modelled by CARRA and a snow model utilizing in situ observations in both the West and East coastal regions of Greenland. They report that CARRA is capable of successfully representing snow-related indicators, with correlation coefficients exceed 0.8 and mean absolute percentage errors less than 30%.

11. 169, why only from January onwards and not in autumn-winter previous year?
Thank you for reflecting on the definition of rain-on-snow days. In the revised version of *Bio-climatic factors*, we state that SnowDays, in combination with RainRatio higher than 50%, are used to derive days with rain-on-snow (RainOnSnow) between January and July to investigate potential snowpack warming before the thermal growing season onset.

12. 169-171, I have a slight doubt about the way that the melt rate is calculated here. If this basically represents the time that passes between the peak SWE and moment of complete snowmelt, and peak SWE occurs early in the winter-spring season, how representative is this timeframe really for the spring melt season and water release? Especially if heavy snowfall occurs later in spring and is followed by warming, this automatically leads to a situation where deep snow appear to melt more rapidly. As a reader, it is hard to fully grasp how such nuances in the choice of processing influence the results.
Thank you for reflecting on the definition of snowmelt rate, which made us adapt our methodology in order to improve. We changed Table 1 in *Bio-climatic factors* and expand for clarity: mean melt rate for ablation days between SWE_MAX DOY and Onset of GrowDays and in the text below During the snow melt period, we calculated daily changes of SWE from which we derived days with negative SWE changes (SWEmeltDays) and the mean of the negative SWE changes (MeltRate).
This improved approach significantly increased the correlation of MeltRate with SWE_MAX to 0.7, reinforcing the already physically discussed relationship between snow depth and snowmelt rates. Heavy snowfall occurring later in spring does not seem to impact the co-variability between SWE_MAX and MeltRate across the entire ecoregion and the 32 years in study.
To avoid redundancy in the PCA, MeltRate is no longer a feature.

13. 176, you mention rain, but rainfall does not seem to be included as a bioclimatic variable as far as I can see (Table 1, Fig. 3), while snowfall was, and rain fraction too. You refer to Fig S10 for statements on the role of rain, but this figure refers to “solid precipitation” which suggests that this is about snow. Since you discuss the role of rain regularly, why not include rain (total summer season liquid precipitation) as a bio-climatic variable explicitly? This would make your conclusions and discussion points on the role of rain more explicit and justifiable.
Our apologies for the incomplete point in the previous version. Thank you, again, to reflect on our approaches. Rain is added to Table 1 and Figure 3. Also, RainJJA, along with RainRatioJJA, is added as a feature in the PCA, as we acknowledge that changes in RainRatio and Rain amount are different quantities with different impacts on the surface.

14. You could statistically back up your choice for PCA and its assumption of linear relations. You could do this by reporting axis lengths, for instance.

Thanks for the remark! As we standardized all variables prior to analyses, we opted for unimodal and linear species response, as PCA is better suited for low variance, small gradients and more intuitive for the interpretation of the biplots.

15. Fig. 1, here results are presented, and completely new information comes in (NAO / GBI), so perhaps the figure should be presented later, in the results. I also miss a scale bar for greenness and it is unclear what "greenness" represents here (is this one the extent variables you calculated, or a mean, and are pixels < 0.15 included or not?).

We apologize for the misunderstanding and for the inconsistencies of the earlier submitted figure. We now:

1. Added to the Introduction that Grimes et al. (2024) has recently shown that the doubling of vegetation across ice-free Greenland is linked with warming. The warming observed in Greenland over recent decades has been associated with more frequent and intense weather patterns that promote widespread clear-sky conditions and the advection of relatively warm air masses from southern latitudes along Western Greenland (Barrett et al., 2020). Weather patterns can be related to indices by analysing specific atmospheric variables over time and space. For instance, the North Atlantic Oscillation is driven by surface pressure configurations in the North Atlantic (Hurrell et al., 2003), and the Greenland Blocking Index by the geopotential height in the mid-troposphere over Greenland (Hanna et al., 2016). Both indices are commonly utilized in climate studies to deduce influences on various components of the climate system in Greenland and vicinity (e.g., Bjørk et al. 2018; Olafsson and Rousta 2021).
2. Opted to keep the absolute values away from the greenness scale in Figure 1 as they correspond to a 32-year averaged greenness, and it would not be useful for any further interpretation.
3. Simplified Figure 1 in the main manuscript, with the correlation maps of summer NAO and GBI moved to supplementary material.

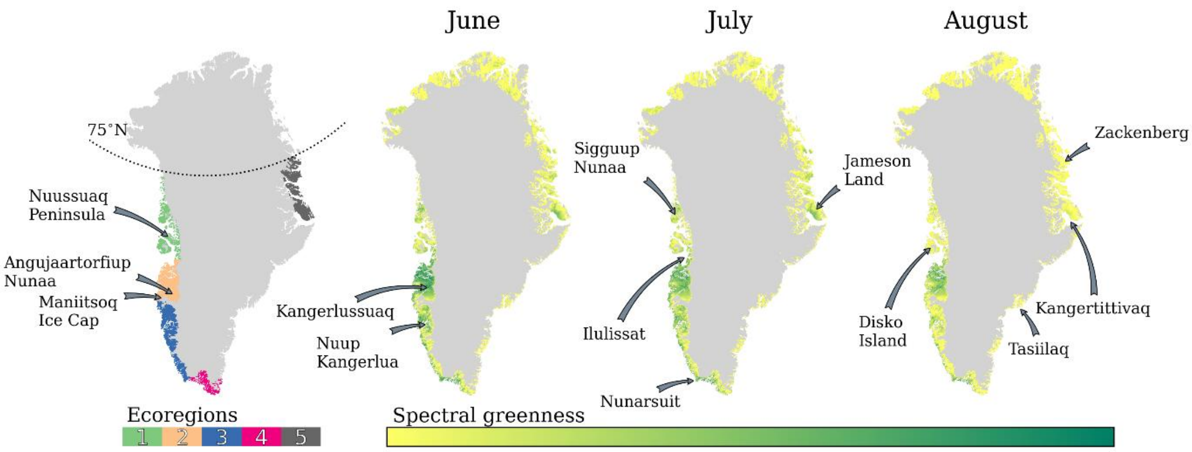

Figure 1 Ecoregions in ice-free Greenland, June, July and August averaged spectral greenness for the period 1991-2023. No scale shown in the colour bar because the aim is to illustrate spectral greenness patterns, not absolute values. Place names referenced in the study are indicated.

4. 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.

16. 227-247 seem to be combined methods and results. The source for the climate oscillation data, and the rationale for including them, have not been properly covered earlier in the methods. It is also unclear how the use of oscillations relates to your study aim and research questions.
Thanks for the remark. We added to the *Introduction* how climate oscillations play a role on vegetation as indicated in minor point 15. We added to Data a new subsection *Climatic oscillation index*, where we describe the data used.
A variety of analytic approaches, such as principal component analysis (PCA) or k-means clustering, are often utilized to characterize the North Atlantic Oscillation (NAO), with input data sourced either from reanalysis or station records. Here, the NAO derived from sea-level pressure applying PCA is used. In this study, the NAO index calculated applying the leading principal component derived from sea-level pressure anomalies within the Atlantic domain (20∘N–80∘N, 90∘W–40∘E) is provided by NCAR/UCAR (Hurrell et al., 2003). This product is posited to yield a more comprehensive representation of NAO spatial patterns compared to indices based on specific terrestrial stations. Notwithstanding, it is noteworthy to acknowledge the dynamic nature of PCA-based NAO indices, being subject to ongoing refinement with the integration of new data.
The Greenland Blocking Index (GBI) is derived from 500 hPa geopotential height over the region (60∘N–80∘N, 80∘W–20∘W), retrieved from PSL/ESRL (Hanna et al., 2016). Both the NAO and GBI indices originate from the NCEP/NCAR reanalysis dataset (Kalnay et al. 1996). Consequently, these climatic oscillation indices have undergone seasonal standardization against the baseline period of 1950–2000.
In the *Results* we also show how these climate oscillations statistically related with green vegetation extent and help to explain the opposite greenness trend signal for the AVHRR and VIIRS periods as shown in minor point 6.

17. 250, Pedregosa et al does not seem like the most appropriate reference for the use of PCA. I advise to find papers that specifically deal with the considerations and strengths of using PCA in a pixel-based remote sensing context.
Thank you for the comment. Now in the *Statistical Methods*, we expand 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 influence among bio-climatic indicators with summer greenness.

18. 259, the use of Mann-Kendall tests is state of the art, but it appears that later on you only show results for growdays and greenness, not all bioclimatic factors as suggested here? Perhaps mention only growdays and greenness then?

We performed a regression and the Mann-Kendall trend test to all bioclimatic indicators in our study. However, attempting conciseness, we only displayed GrowDays and Greenness in the main manuscript. Although other bioclimatic trends are not shown explicitly, their results are referred to throughout the manuscript (e.g., Fig S2 and S10). Therefore, we added more supplementary figures to back our results as also requested in point 23.

19. 262, please explain the use of a 90% confidence interval rather than 95%. With the vast amount of pixels at your disposal, and the relatively long timespan of the study, I would expect that the generally accepted 95% CI would be fine and I would be curious to know why you deviated from this standard.
Thanks, we followed your advice and decreased the test level to 5%, as commonly used in ecology.

20. 271-273, the statements made here need backing; how did you test whether significant long-term trends in vegetation extent were evident? Mann-Kendall test? Could sensor discrepancies play a role here?
Thank you for your comment. We expand on the sensor heterogeneity in our reply to major comment 1. We add in the *Results*: While the AVHRR 22-year trend evidence general expansion of green vegetation, the VIIRS 9-year trend evidence decreases, particularly evident in West Greenland (Table S2). However, due to high variability and small sample size, most trends in both periods are not significant. The significant long-term trends range from 2 % per decade in ecoregion 1 to approximately 6 % per decade in ecoregion 4.

21. 275-279, reads like methods and introduces a whole new aspect of the methodology. I would also provide some more explanation of why the use of detrended Pearson correlations is an appropriate method to evaluate linearity assumptions for a PCA.
Thank you for your comment. We removed the information mentioned in the comment from the *Interconnectedness among bio-climatic indicators* and added to *Statistical Methods* that: As the classic PCA requires the variables to be linearly related, we calculated Pearson correlation coefficients to investigate bio-climatic indicators by ecoregion. However, Pearson correlation assumes that the data are stationary; that is, their statistical properties do not change over time. In order to avoid serial autocorrelation, we transform the data into non-stationary time series by linearly detrending the data before performing the correlation.

22. 290 & 296, you describe how specific variables were removed from analysis a priori. This is essential information that should go into methods, and it seems at odds with your earlier statement that variables were excluded from PCA based on contribution to cumulative explained variance. I would recommend to present a single, unambiguous criterium for the inclusion of variables into PCA and figures, in the methods. Especially since the identification of useful bio-climatic indicators was an explicit aim of the study.

Thank you for your comment. We moved and rephrased the information mentioned in the comment from the *Results* and added in *Statistical Methods*: 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 the ecological processes on the tundra ecosystems, later used as part of the PCA. This will diminish "noise", redundancy and ultimately boost the clarity of interactions across atmosphere-biosphere-cryosphere.

23. 291-292 & L. 294-295, examples of interpretation of results, and no backing (figure, reference) provided to support these interpretations.
Thanks for pointing this out. We acknowledge that some of our statements were indeed too bold and misplaced. Certain statements were moved to and rephrased in the *Key findings and interpretation in the context of the current literature*, while others were rephrased, remained in the *Results* and backed up by a series of supplementary figures.

24. 311-314, I had to read this section a few times to understand the rationale and approach. So if I read correctly, you applied the PCA for all years and ecoregions separately, and then tested whether the variances explained by PC1 and PC2 were similar across the two time periods. I am not fully sure how this would demonstrate that the two NDVI records are comparable and valid in this context. The variances may be similar, but the greenness dynamics, and the associations between different variables and PC axes may not be (do I understand this correctly)? Sidenote: a lot of this information again reads like methods and not results.
Thank you for the remark. We provided information in major point 1 and minor point 6 on how we handled the NDVI records. Additionally, it was important to assess whether sensor discrepancies could have severely impacted the interactions among bio-climatic indicators, influencing loading vectors and the explained variance. Therefore, the inter-annual PCA was performed and assessed for statistically significant differences. The information referred in the comment was removed from the *Results* and added in *Statistical Methods*: Due to a change of satellite sensor from 2014 onwards, we also investigated how PCA performs interannually and whether there was a statistically significant change of the explained variance for years before and after 2014. The result is shown in Figure S8 for a set of 16 bioclimatic indicators, displaying that the two independent samples of explained variance have identical averages in all ecoregions, with a 95 % confidence level, as determined by a two-sample t-test.

25. This is a nice figure! Also here, a scale bar for greenness would help the reader understand what kind of magnitudes we are talking about, across regions.
Thank you for appreciating our charts! We added a scale bar in each subplot to display the range of greenness for the years between 1991 and 2023.

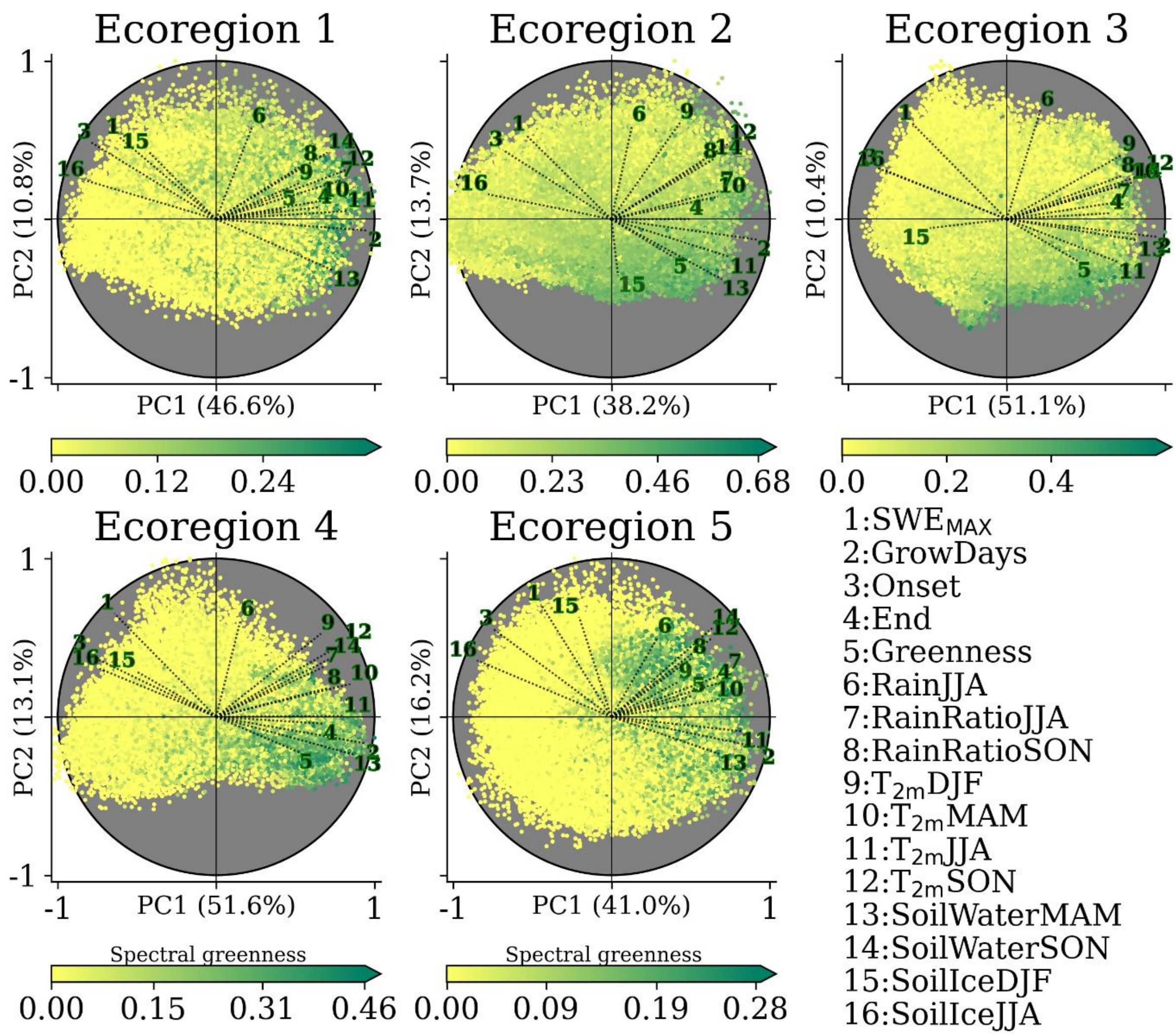

Figure 2 Biplot for scores between 1991 and 2023 for each ecoregion. The loading vectors are labelled and scaled by the maximum of each principal component. The scores are colour-coded based on the summer spectral greenness, with different scales to enhance greenness. The explained variance of the first (PC1) and second (PC2) component is labelled in the corresponding axis of the subplot. The 16 bio-climatic indicators are 1: maximum snow water equivalent (SWEMAX); 2: total number of thermal growing days (GrowDays); 3 and 4: start (Onset) and termination (End) of GrowDays; 5: summer spectral greenness (Greenness); 6: rain in summer (RainJJA); 7 and 8: averaged rain ratio in summer (RainRatioJJA) and autumn (RainRatioSON); 9, 10, 11, 12: averaged 2-m air-temperature in winter (T2mDJF), spring (T2mMAM), summer (T2mJJA) and autumn (T2mSON) 13 and 14: volumetric soil water in spring and (SoilWaterMAM) autumn (SoilWaterSON); 15 and 16: volumetric soil ice in winter and (SoilIceDJF) summer (SoilIceJJA). The abbreviations of the bio-climatic indicators are described in Section 3.2 and in Table 1. The spatial pattern of the averaged 1991–2023 scores for both components in every ecoregion, including their corresponding loadings, are shown in Fig. S10-S14.

26. 318-319, "PC2 is heavily shaped by continentality, permafrost extent and precipitation patterns, meaning that snow-related indicators, like SWEMAX and MeltRate have the highest explanatory power". I struggle to see how your variables and methods could allow you to conclude anything about continentality or permafrost. This needs to be either backed up better, or (ideally) kept for the discussion. I also do not see how this means that snow related indicators are most important (snow is something different than permafrost and continentality?).

Thank you for the remark. We indeed did not process enough data to reliably conclude about continentality or permafrost. Therefore, we rephrased our statement to According to the spatial maps of the first (PC1) and second component (PC2, Fig. S10-S14), PC1 is found to be highly controlled by the topography of the ecoregion, and is consequently related to temperature (and through that on elevation), making GrowDays the bio-climatic indicator with the highest loading in all ecoregions, and therefore, the most significant contributor to the pattern represented by PC1. Through the analysis of the trend map for summer rainfall (Fig. S15) and the spatial maps of PC2, we found that PC2 relates to precipitation and snow patterns, with SWEMAX and RainJJA having the highest explanatory power.

27. L 320 – 350 are altogether quite speculative and many of the claims here need to be supported either by a figure, statistics or literature (and in the latter case, it is better suited for the discussion). I would advise to back up your statements much more. And please carefully evaluate whether reported drivers are really drivers, or just represent the overall role of warming (e.g. increases in rainratio cannot really be teased apart from warming effects so I do not see how you would attribute change to rainfall patterns specifically, especially if total rainfall is not included in the analysis). I think this paragraph needs a thorough rewriting.
Thank you for your comment. We rephrased the entire sub-section *Bio-climatic indicators interlinked with greenness*, moving the interpretations to *Key findings and interpretation in the context of the current literature* with proper citations of experimental studies. Rain is in the revised version included as a bio-climatic indicator as indicated in minor point 13.
Changes in several bio-climatic indicators, such as near-surface air temperature and RainRatio in summer, are indeed related to tropospheric warming. However, warming is not uniform with elevation. While changes in RainRatioJJA are generally related to elevations, changes in T2mJJA are not so clear with elevation in ice-free Greenland. This comment ended up encouraging us on the inclusion of RainJJA as a PCA feature.

28. 349-350, please consider how this relates to the aims of the study (oscillations are not introduced anywhere), report the approach in the methods, and report the test statistics either here or in the appendix.
Thank you for the comment. Climate oscillation indices are now in the *Introduction* and described in minor point 15, with a dedicated section in the *Data* and described in minor point 16.

29. 380, at this point the different terms used (here: spectral vegetation expansion) become a bit confusing. It would be nice to have a single, consistent term for each of the various manifestations of greening that you study in this paper, and present all of these early on.
Thank you for the notice. Only three terms are used along the manuscript: spectral greenness, green vegetation extent and green vegetation distribution. We added these definitions prominently upon first occurrence in *Spectral greenness*.

30. 382, see also major comments, here I was very unsure whether the differences in bandwidths and quality filtering might introduce artefacts into the comparison. Perhaps also good to remind the reader that 'greenness' here refers to the 0.15 threshold (related to comment above).

Thank you for the comment. The revisions stated in major point 1 and minor point 6 apply here, too.

31. 417-420, How can you demonstrate that soil ice has an additional role, additive to warming and rainratio? Aren't they just all sides of the same coin? Could it also be, for instance, that the northern regions still feature most frozen ground conditions in summer and that in southern regions, soils were already mostly above 0 degrees in the summer season, and that hence this dynamic is mostly evident in northern regions? I would carefully read this part of the discussion and evaluate which claims can be made with certainty, and which ones just reflect collinearity within the bio-climate variables.
Thank you for your reflection, which we implemented by more carefully discriminating between cause and effect. Now in *Key findings and interpretation in the context of the current literature*: Our study found that in the northern ecoregions, areas with "greening" in recent decades have experienced a rise in soil water content during the spring (SoilWaterMAM) along with declines in both springtime soil ice content trends (SoilIceMAM) and maximum snow depth (SWE_MAX). The rise in SoilWaterMAM is also accompanied by higher spring temperatures (T2mMAM) and earlier onset of the thermal growing season (Onset).
Despite regional trends on higher summer rainfall amounts (RainJJA), we did not find a clear link between greening and changes in RainJJA. Interestingly, summer soil water content (SoilWaterJJA) and soil ice content (SoilIceJJA) are negatively related to near-surface air temperatures in summer, which results as a consequence of surface thawing and subsequently increased evaporation caused by higher vapor pressure deficits in these northern areas.
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. Conversely, ecoregion 5 is more closely connected with SoilWaterJJA, likely due to a later onset of the GrowDays.

32. Overall, the discussion would really benefit from a thematic subdivision, for instance into different sets of climate variables, or into driving mechanisms and a section on how they differ among regions? Right now the reader easily gets lost between different lines of argumentation.
Thank you for your suggestion. We are confident that our attempt of separating our interpretations into thematic paragraphs could have improved the revised version. For instance, in *Key findings and interpretation in the context of the current literature*, with 1. Changes in green vegetation extent; 2. PCA performance and basis for interpretation; 3. Northern ecoregions; 4. Southern ecoregions; 5. Common features across ecoregions, 6. Drying in the interior of ecoregion 2; 7. GrowDays elevation dependence explained; 8. Changes in green vegetation distribution and in bio-climatic factors reported in literature.

33. 426 – 435, I found the descriptions of slower melt of shallower snowpacks very difficult to follow (and frankly, counterintuitive, but then I am not a snow physics

expert). Even if the melt rate is lower, wouldn't the timing of complete snowmelt still be earlier for shallow snow than for deeper snow? What then is the exact role of the slower melt rate and potentially better water absorption within the context of your findings? I have a feeling that similar claims could be made about the role of deeper snow and its impact on soil temperature and microbial activity (as you also state in the introduction), so I am still in the dark about the role of melt rate in nutrient availability. I would recommend rewriting this in a way that is more accessible to readers without a background in snow physics and staying closer to your own results.
We hope that the improved explanation on minor point 4 clarifies the relationship between snow depth and snowmelt rate better as well as it is addressed in *Key findings and interpretation in the context of the current literature* it reads that For most ecoregions in ice-free Greenland, we find that snowpacks are becoming shallower, and consequently melt slowly, but earlier in the season.
This feature was mentioned by Musselman et al. (2017) and is attributed to global warming. Musselman et al. (2017) explains that in Western North America regions with shallower snow are experiencing snow season contractions. Shallower snow is susceptible to snow season contraction because shallow snow requires less energy to initiate melt than deeper snow. This earlier start of the ablation period occurs at a slower rate due to a combination of near-surface warming with relatively low solar altitude angles.
In contrast, for deep snowpacks that require more energy to initiate runoff, it is also more likely for the snowmelt water to refreeze within the snowpack (Dingman, 2015). Therefore, early season slow snowmelt rates in shallow snowpacks allow for efficient soil water percolation and subsequent water storage (Stephenson and Freeze, 1974). The successful percolation of liquid water into soil plays a key role in tundra regions during the snow ablation period and start of the growing season, as during this time soils are generally dry due to high drainage (Migała et al., 2014).
Increased water availability in the soil could stimulate dormant microbial communities and thus increase the decomposition of soil organic matter, releasing soil nutrients (e.g., Glanville et al. 2012; Salmon et al. 2016; Xu et al. 2021). This in turn could prime the soil for earlier and more efficient vegetation growth and colonization. The increased spring soil water content (SoilWaterMAM), spring air temperature (T2mMAM), and thermal growing season days (GrowDays) indicated in our results could therefore improve conditions for plant growth and colonization, especially in the southern ecoregions. Therefore, it is expected that vascular plants are more developed in early summer. Such conditions in conjunction with summer weather patterns that favours increased T2mJJA and longer periods of solar radiation (Barrett et al., 2020), allowed for greener summer vegetation. The same summer weather patterns also brought more drought and heat days, without an immediate negative impact on greenness.

34. 428, Heijmans et al (2022) doesn't deal with the release of nutrients in relation to spring water availability. Perhaps we cite others in our review that have relevant findings on this topic, but to me this doesn't seem to be an appropriate reference here.

Thank you for the remark. Heijmans et al (2022) is a very relevant reference for us to better understand the links between tundra vegetation and permafrost changes, but in the revised version we rather refer to Glanville et al. 2012 and Salmon et al. (2016), which carries our point in a more central role.

35. 463-465, maybe you can back up this hypothesis about the role of shrubs or potentially other species groups by checking your greenness trends against the CAVM or Karami et al. (2018)?
The recommended sources, the Circumpolar Arctic Vegetation Maps and Karami et al. (2018), are static maps based on the collection of data over several years with different approaches. Our trend perspective is thus not directly comparable, which is why we prefer to remain closer to our focus.

36. 475, what exactly do you mean by "validating bio-climatic indicators"? I think you could explain your proposed course of action a bit better, and also explain how that would help understand future trends.
We wrote in the revised version in *Significance and implication*, that: Our study determines a set of bio-climatic indicators that have been shown relevant for spectral greenness. The statistical interlink among these indicators is confirmed in experimental studies across the Arctic (e.g., Chen et al. 2023; Gamm et al. 2018; Grimes et al. 2024; Huai et al. 2022; Migała et al. 2014; Musselmann et al. 2017; Opala et al. 2018; Schmidt et al. 2023; Stephenson et al. 1974; van der Schot et al. 2023), allowing the interpretation of our outcome to be expanded to large-scale, with apparent features dependent on the ecoregion and latitude. Such insights can now be used to validate whether the same bio-climatic indicators interdependence is captured by climate models. A consistent representation of past conditions would provide a sound basis for the use of such indicators for the study of future vegetation changes across Greenland under a changing climate.

37. The implications section reads like a rather surprising selection of several implications, of which I am not really sure if all the main ones are represented, and whether the ones that are now discussed most extensively are in fact the most important ones. For example, a lot of attention is dedicated to PBAPs and fog, but no mention is made of carbon dynamics or surface energy balance feedbacks. Even if this is deliberate, it would be good to highlight why specific implications are discussed while others are not. You do mention some of these aspects in the limitations, but they are of course also relevant from an implications perspective.
Thank you for the remark! We find relevant to keep recent literature that links primary biological aerosol particles (PBAPs) with the cloud formation in the Arctic and the potential of generating fog conditions due to decreasing sea ice as part of the Discussion. We mentioned other implications although not directly such as the feedback of the vegetation canopy on the surface feedback, shifts in cloudiness due to increased PBAPs, the cooling of the surface due to surface evaporation and ecological shifts on the animal community. However, we acknowledge that other important implications such as carbon dynamics and surface albedo feedback have not been addressed previously and are therefore included in the revised manuscript.

Longer thermal growing seasons are shown across Greenland between 1991 and 2023. Longer thermal growing seasons with higher air-temperatures favoured general vegetation growth and expansion have in the studied period. However, further investigation is required to comprehend the impacts on vegetation and ecosystem functioning in regions that have been facing freezing conditions due to earlier onset of the thermal growing season, exposed to heat stress conditions and experiencing changes in precipitation patterns. Given the reportedly significant decreases in snow cover, the surface albedo is lower for longer periods, facilitating more energy absorption and enhancing surface warming. The observed wide-spread greenness changes intensify the surface albedo feedback with varying effects that extend beyond the growing season and depend on the vegetation type (e.g., Blok et al. 2011; Loranty et al. 2011).

The surplus of the surface energy budget leads to surface warming and promotes surface thawing, particularly in the northern ecoregions. However, depending on the vapour pressure deficit and the vegetation canopy, the excess of surface energy can be used for latent heat release, which in turn will cool the surface (Heijmans et al., 2022). The increase in green vegetation drives at first to greater carbon sequestration. However, if the increase in vegetation causes substantial surface thaw, the net effect could trigger the release of carbon, offsetting the compensation of carbon sequestration from vegetation (Glanville et al., 2012).

38. 506 – 510, I would expect that such episodes of warm, humid conditions should be evident from your PCA analysis, so I do not see the point of mentioning the role of this particular episode as a limitation?

Indeed, the warm and humid episodes should be depicted in the PCA from the reanalysis output, but cloudiness does not allow surface reflectance retrievals, and therefore, partly hinders potential vegetation development.

39. 517-520, needs references for the claims made. I would like to add that while permafrost thaw can indeed release moisture or lead to ponding, deeper thaw fronts also often lead to deeper infiltration and surface drying (Liljedahl et al., 2016). This section could use more nuance and backing.

Thank you for mentioning the possibility of deep thaw fronts which could lead to deep infiltration and surface drying as described by Liljedahl et al. (2016). We added in *Study limitations and future research directions* that: better representation of the permafrost extent and active layer thickness together with the inclusion of dynamic tundra vegetation models within CARRA could be beneficial to deepen our knowledge on interactions among atmosphere, vegetation, carbon and nitrogen cycling, water and permafrost dynamics.

Permafrost areas will continue to likely be locations for future vegetation expansion (Chen et al., 2023), especially under the current trend of decreased summer precipitation. Moreover, permafrost thawed areas are also susceptible to fast drying (Liljedahl et al., 2016) and potentially sudden vegetation changes. Ultimately, plants can fixate along streams and small lakes as future land ice melt will continue to provide sediments and nutrients through runoff (Migała et al., 2014).

40. 525 – 530, I do not want to send you back to the drawing board, but I am interested why elevation was added to your analysis, while aspect and slope were not. You rightfully stress their importance and I would (perhaps naively!) assume that it would not be such an enormous effort to include them in your analysis as well?
Good point and indeed, for a wide perspective it would be useful to add slope and aspect for which we have the data and the analysis ready. We added these results to the revised version. The relationship between GrowDays and topographical features such as slope and aspect was further explored. As the surface slope is highly correlated with surface elevation, trends in GrowDays tend to significantly decrease with steepness. The dependence between GrowDays and surface aspect is rather complex, without a predominant slope orientation promoting GrowDays, in general. However, latitudes immediately south of Maniitsoq Ice Cap show increases of GrowDays in slopes with southwest orientation. On the East coast, a western slope orientation is particularly pronounced along Jameson Land, whereas northeast exposure appears favourable north of ecoregion 5.
The dependence of the slope orientation for greenness changes is partly in alignment with the dependence of the slope orientation for GrowDays. Greenness trends increased in two latitudinal bands facing southeast in ecoregion 1 and 2. In Jameson Land a similar tendency for more greening is found towards southwest, while east facing slopes are preferred towards the northern part of ecoregion 5.
Also, important to mention that Surface slope is transformed into sine aspect (west-east orientation) and cosine aspect (north-south orientation), given its circular orientation. Positive values in sine (cosine) aspect indicate how much the slope is facing east (north), whereas negative values indicate how much the slope is facing west (south). was added in Bio-climatic factors.

41. Rather than reiterate what you did, you could summarize the actual findings and try to align better with the original aims (perhaps mention which set of variables or which variables show the strongest associations?) and mention the key advance you have made? This would make the conclusion more informative.
Thank you for the suggestion. We briefly summarize our findings and keys advancements in the field in the revised version. This is answered in minor point 2.

Technicalities & Language

1. 10 “summer spectral vegetation”. This is an unusual term, it would be good to rephrase it or explain it so that there can be no ambiguity about what it means.
This term comes from Myers-Smith et al. (2020), who refers to NDVI as a spectral vegetation index. Therefore, summer spectral vegetation (a.k.a. spectral greenness) is the seasonally averaged result mentioned in *Bio-climatic factors*.
2. 18 “by 22.5% increase” should be “by 22.5%”. I also recommend to be more explicit about what you mean by “the distribution of vegetation”. Do you mean that the vegetated area of Greenland (determined here as summer NDVI > 0.15?) expanded in area by 22.5%? Perhaps you want to rewrite this sentence.
Thank you. This sentence was rephrased.
3. 25, what do you mean by “regional Greenland”? Perhaps that specific regions of Greenland are warming three times faster.

Not the entire Greenland is warming. Therefore, “regional” gives emphasis only to warming locations.

4. 31, add “and” instead of comma between “composition” and “alterations”.

Thank you. Done!

5. 48, is it really necessary to mention the specific methods of Gamm et al (”using [..], [...] and [...]”)? This is not done for other papers that you cite?

Not really. Thank you. Done!

6. 53-55, this reads like a repetition of L. 43-44.

Agreed. Thank you. Done!

7. 62, I do not think “snow cover melt” is a very generally used term. Maybe write “snowmelt timing” or “snow melt rate”, depending on what you mean exactly?

Thank you. Done!

8. 71, maybe write “large amounts of snow” rather than “large amounts of snow coverage”, since from what I understand snowpacks were also very deep, not just spatially extensive.

Thank you. Done!

9. 81-82, example of a grammatically confusing sentence.

Thank you. This and many other sentences were rephrased.

10. 83-86, implications for phytoplankton seem beyond the scope of your study system and I do not see the added value of discussing it here (it seems more of an implication rather than an example of the importance of subsurface flow to terrestrial vegetation).

Thank you. This was removed.

11. 105, add “the” between “to” and “CARRA”.

Thank you. Done!

12. 132, “and thereafter is then continued” should be “and is thereafter continued”.

Thank you. Done!

13. 133, add “is” between “mask” and “spectrally”.

Thank you. Done!

14. Figure 5) Final sentence in the caption: Do you mean that the trend was considered significant if the 90% CI of the estimate did not overlap 0? This is what I am used to. Similar for Fig. 6

Correct. It was added to Figure 6 and to other figures where M-K trend test is used that the null hypothesis is that the slope is equal to zero.

15. 376, replace “evidence” with “shows”?

Thank you. Done!

16. Table 2) perhaps a no brainer, but it would be good to explain what the fraction mean; is this % of total area of that ecoregion?

Thank you. Done!

17. 446, change “favourable areas” into “a more favourable area”.

Thank you. Done!

18. 498, change “as” into “as in”

Thank you. Done!

Thank you for these valuable edits which we incorporated in the revised manuscript.

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RC2: 'Comment on egusphere-2024-2571', Anonymous Referee #2, 21 Oct 2024

Dear Silva et al. & the editors of Copernicus Biogeosciences,

Thank you for the invitation to review this manuscript. It's a great privilege to contribute to our scientific community. Please see the text below for my review of the manuscript, "Bio-climatic factors drive spectral vegetation changes in Greenland" by Tiago Silva et al. This study seeks to identify bioclimatic drivers of changes in greenness and greenness distribution across Greenlands ice-free terrestrial ecosystem. Understanding the impacts of climate change on this ecosystem is extremely important, particularly in the context of recent studies highlighting changes in vegetation ("Arctic greening") and permafrost dynamics. The authors do a good job summarizing the major points of current literature in these regions and highlighting the importance of their study.

The authors seek to assess these drivers by combining remotely sensed NDVI as observed from AVHRR and VIIRS between 1991 - 2023 with a gridded climate data set, the Copernicus Arctic Regional Reanalysis (CARRA). The authors use Principal Component Analysis (PCA) to identify correlations between "greenness" and a matrix of bioclimatic variables. Additionally, they use non-parametric methods to identify trends in bio-climatic indicators and assess their directionality in magnitude over time across 5 sensibly delineated ecoregion across the terrestrial Greenland ecosystem.

General Comments

I commend Silva et al. for their ambitious analysis of a substantial amount of data from a sensitive ecosystem of broad scientific interest. For this reason, it is my opinion that the study's aim is well suited for the readership of Copernicus Biogeosciences and is an important undertaking. However, I have major concerns about the implementation of methods and the interpretation of results. Most

importantly, there is a critical misalignment between the stated goals of the study and the methods used to achieve these goals (as well as the title of the paper).
To summarise my concerns: The authors sought "to gain a deeper understanding of the spatio-temporal patterns of spectral vegetation changes across ice-free regions of Greenland (ln 90)" and "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 (ln 91 - 95)." However, the authors provide contradictory statements about the goals of the PCA. Throughout the paper they explicitly state that they use PCA to assess drivers of *changes* in NDVI over time within a pixel, as well as having used PCA to assess drivers in changes of greenness *distribution*. Reviewing the methods and results of the PCA, it seems that the dimensionality reduction algorithm was actually used to assess bio-climate indicators that correlate with average summer spectral greenness ("greenness distribution"). I elaborate on these concerns below.

We would like to thank the reviewer for the general appreciation of our work. We can understand the confusion from the reviewer's point of view regarding the general objectives of our work. These are clearly not well reflected in the paper. We have therefore clarified exactly these points in the revised *Abstract*:
We use principal component analysis (PCA) to examine key sub-surface and above-surface bio-climatic factors influencing ecological and phenological processes preceding and during the thermal growing season in tundra ecosystems. Subsequently, we interpret spatio-temporal interactions among bio-climatic factors on vegetation and investigate bio-climatic changes dependent on latitude and topographical features in Greenland. Ultimately, we identify regions of ongoing changes in green vegetation distribution.
While we derive the spatio-temporal change in spectral greening from time series of the NDVI (trend analysis), PCA is used to reduce the dimensionality of the vector space of the explanatory bioclimatic indicators. We can then use the bio-climatic indicators with the greatest explanatory value to explain and try to understand changes in spectral greening from the temporal changes among indicators on regionalised scales of ice-free Greenland. The causal PCA outcome is treated with caution, and therefore, we added to the *Statistical Methods*:
We attempt a careful causal interpretation of the loading vectors from the first two principal components (PCs) of the PCA through biplots (Gabriel, 1971). Although these PCs account for most of the explained variance, their interpretation in terms of causality is limited by the nature of PCA as a descriptive statistical technique. For a cautious interpretation of the PCs, we examined not only the magnitude and direction of the loading vectors, but also trend maps of the involved bio-climatic indicators and literature on experimental studies
In the *Discussion*, we present and expand our interpretations in *Key findings and interpretation in the context of the current literature* and in *Significance and implication*, that: Our study determines a set of bio-climatic indicators that have been shown relevant for spectral greenness. The statistical interlink among these indicators is confirmed in experimental studies across the Arctic (e.g., Chen et al.

2023; Gamm et al. 2018; Grimes et al. 2024; Huai et al. 2022; Migała et al. 2014; Musselmann et al. 2017; Opala et al. 2018; Schmidt et al. 2023; Stephenson et al. 1974; van der Schot et al. 2023), allowing the interpretation of our outcome to be expanded to large-scale, with apparent features dependent on the ecoregion and latitude.

Specific comments

Major Concerns

Thank you for the comprehensive statement. We will break the explanation of point 1 into several sub-points.

1) There is a critical misalignment between the stated goals of the study and the methods used. In Section 3.4, the authors mention that "PCA was used to investigate the combined influence among bio-climatic indicators on summer greenness _changes_" (ln 249-250; _emphasis added_). However, in the Results section, it is stated that "PCA was used to investigate the combined influence among bio-climatic indicators with summer greenness" (ln 307), which suggests an analysis of greenness levels rather than _changes_ in spectral greenness.

This is a very valid point, and we apologize for having caused confusion. Our intention was the same in both sentences. The PCA encompasses data since 1991 to 2023. This means that the PCA outcome is a statistical result of the biosphere-atmosphere-cryosphere interactions over the three decades in study. By colouring each score with its corresponding greenness, we show that the densely vegetated/greenest regions are clustered by the first two principal components as a result of "the combined influence among bio-climatic indicators with summer greenness". This outcome is only achieved by considering the spatio-temporal changes of all bio-climatic indicators, where greenness is included. We changed both instances in the revised document to the combined influence among bio-climatic indicators with summer greenness

This discrepancy is further supported by the caption for Figure 4, which notes that the biplots' scores "are colour-coded based on the summer spectral greenness as in Figure 1," where spectral greenness is defined as the "averaged spectral greenness (based on the period 1991-2023) for June, July, and August."

We apologize for the misunderstanding caused in this specific sentence from the Figure 4 caption. We removed the reference to Figure 1 and simplified the sentence to The scores are colour-coded based on the summer spectral greenness, with different scales to enhance greenness. While Figure 1 shows 32 years of monthly averaged spectral greenness, the coloured scores in Figure 4 correspond to summer spectral greenness for each year between 1991 and 2023. We also added colour scales in Figure 4 to better distinguish greenness across ecoregions, as later suggested by the referee.

Additionally, greenness is included in the PCA but defined differently as "seasonally averaged monthly NDVI," a quantity briefly mentioned in Section 3.1.

Monthly averaged NDVI is used in Figure 1 to show the evolution of spectral greenness in summer in each ecoregion. This is particularly important for readers without knowledge of the greenness dynamics over summer across ice-free Greenland.

In *Spectral Greenness* we state that "we calculated a seasonally averaged NDVI, hereafter referred to as spectral greenness and interchangeably as green vegetation." And in *Bio-climatic factors* we write "Spectral greenness, T2m, RainRatio, the volumetric soil water and ice (SoilWater and SoilIce) and vapour pressure deficit (VPd) are seasonally averaged, whereas precipitation, snowfall (Snow) and rainfall (Rain) are seasonally accumulated." And later in the same section, we write "Spectral greenness was compiled for summer, in order to capture the period with maximum solar radiation in Greenland and avoid snow-covered patches"

The authors highlight that PC1 and PC2 "largely capture and explain Greenness distribution" (ln 320-321), suggesting a focus on greenness levels rather than changes.

The orientation of the loading vectors along with the greenness distribution in the biplots and the supplementary maps (Fig. S10-S14) show that the scores with high spectral greenness are grouped in areas of low elevation (PC1>0) with varying degrees of influence in precipitation and snow patterns (PC2).

It may be possible that the inclusion of "changes" in lines 249-250 was unintentional. However, the broader context suggests that the issue extends beyond a simple wording error. The title ("Bio-climatic factors drive spectral vegetation changes in Greenland"), the abstract (ln 10-15: "GrowDays... emerged as the pivotal factor across all ecoregions...to promote vegetation growth."), and the discussion (e.g., ln 417-419: "Our [PCA] results suggest that in the northern ecoregions, the reduction in soil ice during summer...is enabling vegetation growth, leading to northward expansion of vegetation." and ln 433-435: "The combined effect of soil nutrients with increased soil water availability in spring (SoilWaterMAM) and T2mMAM, promotes early plant growth. Therefore, leaves are more developed in early summer, which in association with increased T2mJJA and longer periods of solar radiation, allow for greener vegetation.") all imply a focus on changes in greenness values over time.

Thanks for highlighting this confusing point. Indeed, we look both at the state as well as the changes. Our *Results* aim to investigate spatio-temporal changes in greenness and its co-variability with atmospheric and snow indicators from 1991 to 2023. The monthly averaged greenness state is only shown in Figure 1.

The statement raised "GrowDays emerged as the pivotal factor across all ecoregions" in the comment is based on the interpretation of the biplot loading vectors and the relative importance of the loading vectors shown in the supplementary Figures S10-S14. We developed our reasoning in *Key findings and interpretation in the context of the current literature*. There, we write The rank of relative importance of individual bio-climatic indicators depends on ecoregion, with the number of days of the thermal growing season (GrowDays) being the most relevant across all ecoregions, followed by soil ice during summer (SoilIceJJA) in the northern and SoilWaterMAM in the southern ecoregions.

And later:

the early onset of GrowDays allows vegetation to be potentially more active and responsive to solar radiation, particularly in the ecoregions in lower latitudes with longer sun exposure.

We also improved our explanations on how biplots are interpreted in *Key findings and interpretation in the context of the current literature*. There, we write: For most ecoregions in ice-free Greenland, we find that snowpacks are becoming shallower, and consequently melt slowly, but earlier in the season.

This feature was mentioned by Musselman et al. (2017) and is attributed to global warming. Musselman et al. (2017) explains that in Western North America regions with shallower snow are experiencing snow season contractions. Shallower snow is susceptible to snow season contraction because shallow snow requires less energy to initiate melt than deeper snow. This earlier start of the ablation period occurs at a slower rate due to a combination of near-surface warming with relatively low solar altitude angles.

In contrast, for deep snowpacks that require more energy to initiate runoff, it is also more likely for the snowmelt water to refreeze within the snowpack (Dingman, 2015). Therefore, early season slow snowmelt rates in shallow snowpacks allow for efficient soil water percolation and subsequent water storage (Stephenson and Freeze, 1974). The successful percolation of liquid water into soil plays a key role in tundra regions during the snow ablation period and start of the growing season, as during this time soils are generally dry due to high drainage (Migała et al., 2014).

Increased water availability in the soil could stimulate dormant microbial communities and thus increase the decomposition of soil organic matter, releasing soil nutrients (e.g., Glanville et al. 2012; Salmon et al. 2016; Xu et al. 2021). This in turn could prime the soil for earlier and more efficient vegetation growth and colonization. The increased spring soil water content (SoilWaterMAM), spring near-surface air temperature (T2mMAM), and lengthening of the thermal growing season (GrowDays) indicated in our results could therefore improve conditions for plant growth and colonization, especially in the southern ecoregions.

As the analysis currently stands, PCA is used to assess the variation in climate variables, which is then visually compared to average summer greenness from 1991-2023 with biplots. Separately, the authors explore trends in vegetation expansion using Mann-Kendall tests and thresholds of NDVI between two discrete periods (1991 – 2007 and 2008 - 2023). Despite a lack of generative or predictive models linking these two goals, the authors then interpret PCA loading vectors as "explaining" changes in greenness and greenness distribution. It is also not clear to me how the authors made these interpretations; I speculate this was done by visual comparison of the maps of PCs in the supplementary material with the maps of greenness distribution and greenness change over time in Figure 6.

The referee is partly correct on the speculation on how the results are interpreted. Additional to the referred procedure, we also use the information in Figure 6c to mask trend maps and better understand trend directions among bio-climatic indicators. This was essential to better understand how temporal changes among bio-climatic indicators are interlinked in the newly emerged vegetated areas, as described in *Key findings and interpretation in the context of the current literature*: Our study found that in the northern ecoregions, areas with "greening" in recent decades have experienced a rise in soil water content during the spring (SoilWaterMAM) along with declines in both springtime soil ice content trends (SoilIceMAM) and maximum snow depth (SWE_MAX). The rise in SoilWaterMAM is also accompanied by higher spring temperatures (T2mMAM) and earlier onset of the thermal growing season (Onset).

Despite regional trends on higher summer rainfall amounts (RainJJA), we did not find a clear link between greening and changes in RainJJA. Interestingly, summer soil water content (SoilWaterJJA) and soil ice content (SoilIceJJA) are negatively related to near-surface air temperatures in summer, which results as a consequence of surface thawing and subsequently increased evaporation caused by higher vapor pressure deficits in these northern areas.

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. Conversely, ecoregion 5 is more closely connected with SoilWaterJJA, likely due to a later onset of the GrowDays.

The use of generative or predictive models goes beyond the scope of this study.

We hope that with the explanations provided and the revisions made are clearer to the referee, particularly on how PCA and trend analysis were used and interpreted.

2) Loading vectors should not be interpreted causally in the way the authors have. While it is true that alignment between two loading vectors indicate correlation and orthogonal vectors are uncorrelated, PCA is a function purely on a matrix of features without explicit regard for response variables. Since PCA is generally used for dimensionality reduction, data compression, or exploratory analysis, its application to infer causal relationships between bio-climatic factors and greenness requires further qualification. If the goal is to assess the relative importance of climate variables on changes in greenness, a causal (or at least an interpretable predictive) model is required.

Thank you for the remark. We agree that Greenness is not a response variable due to the reasons mentioned in point 1. While we were overconfident in some of the formulations in the first submission, we adopted a more defensive wording – alluding to evidence that is in line with literature. The careful interpretation of the loading vectors from the first two principal components was mentioned in the General Comment of the referee.

3) The inclusion of "seasonally averaged" spectral greenness as a feature in the PCA and then coloring the scores in the biplots of Figure 4 based on average summer spectral greenness over the growing seasons (1991-2023) raises concerns about circular reasoning. Further clarification on how this aspect was handled could help alleviate these concerns.

Indeed, there is room for clarification. We rewrote the caption and implemented scalebars in every subpanel to avoid further misunderstandings. The PCA is performed for summer greenness pixels available in every ecoregion between 1991 and 2023. The corresponding colouring indicates the greenness of each pixel in a particular year. The co-variability of greenness with the remaining components is shown on loading vector 5 and the colouring of the scores helps to better understand how greenness is distributed along PC1 and PC2 space, and geographical space with the support of supplementary figure as mentioned in the General Comment.

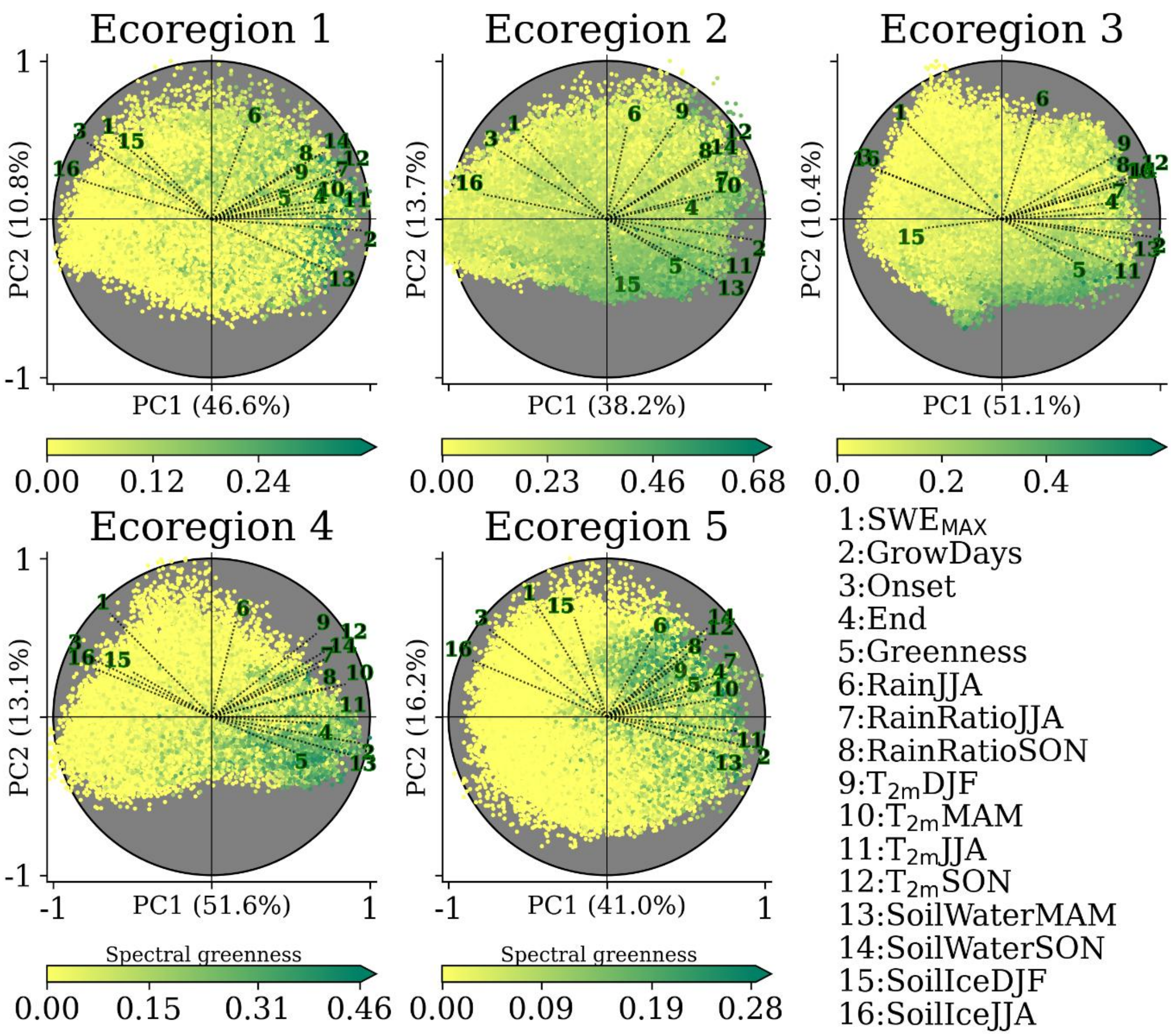

Figure 1 Biplot for scores between 1991 and 2023 for each ecoregion. The loading vectors are labelled and scaled by the maximum of each principal component. The scores are colour-coded based on the summer spectral greenness, with different scales to enhance greenness. The explained variance of the first (PC1) and second (PC2) component is labelled in the corresponding axis of the subplot. The 16 bio-climatic indicators are 1: maximum snow water equivalent (SWEMAX); 2: total number of thermal growing days (GrowDays); 3 and 4: start (Onset) and termination (End) of GrowDays; 5: summer spectral greenness (Greenness); 6: rain in sumer (RainJJA); 7 and 8: averaged rain ratio in summer (RainRatioJJA) and autumn (RainRatioSON); 9, 10, 11, 12: averaged 2-m air-temperature in winter (T2mDJF), spring (T2mMAM), summer (T2mJJA) and autumn (T2mSON) 13 and 14: volumetric soil water in spring and (SoilWaterMAM) autumn (SoilWaterSON); 15 and 16: volumetric soil ice in winter and (SoilIceDJF) summer (SoilIceJJA). The abbreviations of the bio-climatic indicators are described in Section 3.2 and in Table 1. The spatial pattern of the averaged 1991–2023 scores for both components in every ecoregion, including their corresponding loadings, are shown in Fig. S10-S14.

4) Generally, the methods are not described in enough detail. In addition to my confusion about the methods as described above:

4a) I agree with a note from another reviewer, the calibration procedure addressing potential systematic biases between AVHRR and VIIRS NDVI should be elaborated.

Thank you for raising these concerns.

This is a very important point, namely the homogeneity of the two NDVI time series to each other and what this means for a calculated trend.

The NOAA Climate Data Record (CDR) of AVHRR NDVI - Version 5 and the NOAA CDR of VIIRS NDVI - Version 1 are developed by Eric Vermote and colleagues (Vermote et al. 2018 and 2022) for NOAA's CDR Program. Both records have been processed considering the same atmospheric characteristics as in Miura et al. (2012) and both processed records are posterior to Miura et al. (2012) proposed correction. However,

the correction proposed by Miura et al. (2012) is not assessed in polar regions, which may contribute to additional uncertainties in our study.
Unfortunately, no overlap periods are available for the parallel measurements of the two satellite sensors. Therefore, no systematic differences can be determined. However, as we state in *NOAA Climate Data Record for Normalized Difference Vegetation Index* there is work that has calibrated the utilized AVHRR product with MODIS (e.g. Franch et al., 2017) and thus improved the internal homogeneity of AVHRR, as well as work that has established the homogeneity of VIIRS with MODIS (Skakun et al., 2018) to improve the consistency of the NDVI datasets. This does not yet achieve perfect homogeneity, which we explain in the description of both products in *NOAA Climate Data Record for Normalized Difference Vegetation Index as follows*:
According to AVHRR and VIIRS technical reports, the NIR channel is centred at different wavelengths (830 nm vs. 865 nm). As there is no overlapping period available in the NOAA CDR, potential mismatches between AVHRR and VIIRS NDVI cannot be discarded.
We also investigate at the start of *Results* how summer spectral greenness statistically relates with climate oscillations (e.g., Greenland Blocking Index) for AVHRR, VIIRS and the entire study period. We use these climate oscillation time-series, that are homogenous and independent of spectral greenness, as a reference to evaluate systematic inconsistencies that may arise due to sensor change.
It should be noted that prevailing weather patterns during summer months, like the North Atlantic Oscillation (NAO) and the Greenland Blocking Index (GBI), are highly correlated with spectral vegetation (Fig. S7). Therefore, summer weather patterns can accelerate or delay the maximum green vegetation extent given their link with temperature and precipitation. 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. Positive and significant correlation coefficients ranging between 0.5 and 0.8 are found between ecoregion 1 and 4, generally with higher correlations for VIIRS than for AVHRR period. Green vegetation extent in ecoregion 5 is poorly correlated with the prevailing weather patterns during summer.
While the AVHRR 22-year trend evidence general expansion of green vegetation, the VIIRS 9-year trend evidence decreases, particularly in West Greenland (Table S2). However, due to high variability and small sample size, most trends in both periods are not significant.
We address in *Study limitation and future research*, our concerns about the reliability of long-term time integrated NDVI analysis
The NDVI datasets employed in this study are sourced from two satellite products processed by NOAA, each utilizing a different type of sensor. Due to the absence of a temporal dataset overlap, the assessment of uncertainties was limited and potential for mismatches between the datasets cannot be discarded. This lack of a common calibration period raises concerns about the reliability of long-term time integrated NDVI analysis.
In the end, we follow similar approaches of recent literature (e.g., Madson et al. 2023, Pourmohamad et al. 2024) that make use of the full AVHRR NDVI and VIIRS NDVI without additional corrections.

4b) The calculation of "seasonally averaged NDVI" is somewhat unclear. I assume this involves averaging monthly NDVI across the growing season, but further explanation would be helpful.

We have also revised the same subsection in general to better understand the processing of two NDVI data sets and the problems of data homogeneity. We also reformulate in *Spectral greenness* how spectral greenness is derived from the AVHRR and VIIRS NDVI to better explain how the shaded area later shown in Figure 2 is calculated:

As estimates integrated through time are less likely to be influenced by temporal sampling artefacts at high latitudes than metrics based on maximum NDVI (e.g., Myers-Smith et al. 2020), we started by calculating monthly integrated NDVI. Also, since our focus is on green vegetation, only daily NDVI pixel values with higher or equal to 0.15 are considered. Then, we divide the monthly integrated NDVI by the total number of monthly observations (n, see Figure S1 for the interannual variability of n) to obtain the monthly NDVI. However, before 2014 and as described in Subsection 2.2, the AVHRR algorithm was less strict in its data quality control compared to VIIRS from 2014 onward, resulting in higher n before 2014 that lowers monthly NDVI. To address temporal heterogeneities, we adjusted n from the AVHRR period with the number of monthly observations acquired during the VIIRS period. From 2014 to 2023, we identified the minimum, maximum and average number of observations for each month. Hence, using these three quantities, we generated a consistent variability range from 1991 to 2013 to recalculate monthly NDVI, considering a similar number of observations as from 2014 to 2023. This procedure assumes that the environmental conditions (i.e. snow-cover, clouds and shadow) between 1991 to 2013 are similar to those between 2014 and 2023. The maps for the average number of monthly observations and the associated standard deviation for AVHRR and VIIRS period before and after the adjustment regarding n are shown in Figures S2-S5, respectively.

4c) Given the potential impact of cloud cover and other factors on NDVI observations, more information on how observation frequency (described as "n" in Section 3.1) was used to assess uncertainty and uneven sampling would strengthen the analysis. This seems like it was at least tangentially covered given the brief mention of this in Section 3.1 and the first figure in the Supplementary Materials -- but more explanation of the procedures is needed.

The point is addressed and answered in the previous sub-point.

4d) More details on the PCA and Mann-Kendall implementations would also be valuable. For example, when using scikit-learn for PCA, describing the optimizer and input data shape would help ensure transparency, as some solvers are better optimized for particular data configurations. Similarly, the choice of the standard Mann-Kendall test variant in pyMannKendall should be justified, especially regarding serial autocorrelation, which is an important consideration in trend analysis. While MK tests are the current state of the art for landscape-scale analysis like this, pyMannKendall offers options that seek to account for autocorrelation, and discussing whether this was assessed in the data would clarify the robustness of the trend analysis.

Thank you for the request. We acknowledge that we did not include all the necessary information for reproducibility, with indication of the optimizer and input data shape, but now in the *Statistical Methods* we write The PCA (Pedregosa et al., 2011) solver was selected based on the input data shape. As the number of features in the input data is much less than the number of samples (geographic pixels), a classical eigenvalue decomposition on the covariance matrix was run. and We used the non-parametric Mann-Kendall (M-K) trend test (Hussain and Mahmud, 2019) to assess trend monotonicity and significance among bio-climatic indicators. However, to acknowledge autocorrelation in the greenness data, we computed the Hamed and Rao modified M-K test (Hamed and Rao, 1998), with a variance correction approach considering all significant lags to improve trend analysis.

Minor Concerns & Technical Corrections.

In addition to minor concerns pointed out by another reviewer, there are some instances of speculation that are not supported by the PCA analysis in the results section which should removed, or moved to the discussion section and include citations. These are also specific examples of where I think a inappropriate causal interpretation of loading vectors has occurred (Major Concern 2). For example:

- (ln 326) "The decreasing trend of snow rates (SnowDJF and SnowMAM) has led to SWEMAXDOY to occur earlier. Despite the increasing trend in T2mMAM, the still-low solar elevation and the still-low near-surface air-temperatures result in low melting rates of the snowpack (MeltRate). These slow melt rates favour slow meltwater percolation (SoilWaterMAM loading vector opposite to MeltRate loading vector)."

We reformulated this statement in the *Results*, referring to the trend maps (seasonal accumulated snow, SWE_MAX DOY, MeltRate and SoilWaterMAM) in the Supplementary Material of the revised manuscript. The speculative part of this statement was rephrased and moved to *Key findings and interpretation in the context of the current literature*.

- (ln 329) "Additionally, the earlier onset of the thermal growing season allows vegetation to produce energy via photosynthesis, particularly in the ecoregions in lower latitudes with adequate 330 sun exposure (Onset loading vector opposite to Greenness loading vector)."

Thank you for the remark. This speculative sentence was written and moved to *Key findings and interpretation in the context of the current literature*: the early onset of GrowDays allows vegetation to be potentially more active and responsive to solar radiation, particularly in the ecoregions in lower latitudes with adequate sun exposure (Opala et al. 2018).

- (ln 333) "Therefore, increases of RainRatioJJA promote high greenness (aligned loading vectors), as vegetation in such environmentally harsh places likely developed mechanisms to effectively retain/absorb liquid water whenever possible."

Thank you for the remark. This speculative sentence was rewritten and moved to *Significance and implications*: Water droplets from fog can effectively be retained by tundra vegetation and are not accounted as a water source. This interaction between fog, vegetation and soil conditions should be better investigated particularly for coastal tundra vegetation.

This sentence is a tautological argument:
(ln 465) "The wide-spread summer spectral greening occurs as a result of greener vegetation as certain sites."
Thanks, this was misleading indeed. We adapt to: The widespread summer spectral greening could be due to encroachment of vegetation on previously bare surfaces and changes in plant community composition at certain sites (Grimes et al. 2024).

The importance of solar radiation exposure is described as important in several places, including the conclusions, but are not included explicitly in the PCA or other analyses (ln 327, 435, 534).
The exposure to solar radiation is not considered as the NDVI is only available when there is solar exposure. However, we make use of relevant metrics of the atmospheric circulation patterns in the vicinity of Greenland that promote cloudless conditions (e.g., positive phase of the Greenland Blocking Index, GBI). That is why in Figure 1, we correlated summer greenness with summer GBI, where we report high positive correlations across all ecoregions. Therefore, we cannot discard the role of the interannual variability of atmospheric circulation patterns on greenness, as the previous decade (since the 2010s) was composed by more frequent cloudless conditions in summer (Silva et al. 2022).

Figure 4 - It would be helpful to readers if the PC1 axis was flipped for Ecoregion 2 and 4 so that the quadrants with higher greenness scores were all in the same vicinity in the biplots across Ecoregions.
Thanks for the remark! We flipped the axis for the same orientation across ecoregions.

The color palettes in Figures 5 and 6 rely on a reader's ability to distinguish red and green, which is a common color-blindness.
Thanks for this advice. We value inclusivity and attempt to open up wherever we can. The colormaps were checked prior to submission following the Copernicus manuscript preparation style and Coblis – Color Blindness Simulator. All figures are supposed to be colour-blind friendly, except for monochromacy.

Grammar checks needed throughout.
Thanks for the remark! We revised and improved the grammar.

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