

Note to Editor

- We have implemented all changes as suggested by the reviewers. Please find our detailed responses below.
- We restructured the discussion, separating between vegetation and cryosphere parameters and a discussion of “Similarities and causalities”. The latter considered the additional references suggested by the reviewers. In case of the issue on Fram strait ice export, we included a reference to the reviewer 1 comment (Anonymous, 2025) in order to acknowledge that this was his/her suggestion.
- Although not requested by the reviewers, we decided to graphically improve the workflow diagram (Figure 2): change of boxes with color to black & white and using different box forms instead; Moving repeated entries outside of the boxes to the left (sea ice basins, land parameter); extending the output text description (not just ‘statistics’, but also ‘mean correlations’ etc.).
- We also revised the text for grammar, repeated phrasings etc.

Reviewer 1

The manuscript by Bartsch et al. describes how pan-Arctic datasets of mean annual ground temperature at 2m depth (MAGT), snow water equivalent (SWE) and NDVI (as a proxy for plant growth) correlate with sea ice area (SIA). Sea ice loss is one of the main causes of the amplified warming of the Arctic, and together with changes in atmospheric humidity this influences MAGT, SWE and plant growth. Such links have been shown previously from observations, remote sensing and models (see e.g. Bhatt et al., 2010, 2014, 2017; Buchwal et al., 2020; Macias-Fauria et al., 2012, 2017; Parmentier et al., 2015; Rehder et al., 2020; Screen et al., 2012; Yu et al., 2021). This study aims to differentiate itself from this previous work by using satellite data where possible, and by focusing more on regional correlations rather than those made across the whole Arctic.

- Comment: Note, further key novel points are the consideration of vegetation and cryosphere at the same time, previous studies have focused on one parameter only. A comparison through a consistent setup for vegetation and land cryosphere parameters was so far not made. Also the temporary extension, evaluating all months in combination with consideration of sea ice basins goes beyond past studies. There has been an example for NDVI versus monthly sea ice (Yu et al. 2021), but without considering different sea ice basins.
- We now added in the objectives “only” at the end of the first sentence to emphasize this better: “...have been recently documented for selected cryosphere parameters only”

While I appreciate the attempt by the authors to look further into this topic, I feel that the manuscript in its current form is a missed opportunity to learn something truly novel. In particular, I had hoped that this study would go beyond mere correlations by identifying causal links, and by showing more detail. More detailed regional analyses between sea ice and the terrestrial environment have been done for example by Parmentier et al. (2015) who performed a pan-Arctic pixel-wise correlation between local sea ice conditions and temperature and modeled methane emissions, and who argued a causal link in autumn but not in spring. Rehder et al. (2020) used causal-effect networks to identify temporal links to the land near the Laptev Sea, and showed that spring-time correlations in sea ice and atmospheric

variables were both related to large scale atmospheric circulation, not to each other, although sea ice loss had a weak effect on the near coastal environment in summer.

- Reply: Thanks for pointing out the two studies. Parmentier et al. (2015) show that linkages might exist even further south than for the extent we have chosen plus the inclusion of sea ice across the Hudson bay and Canadian Archipelago (unfortunately this was not covered in the sea ice basin data set that we used), what has been now addressed in the discussion (new subsection Similarities and Causalities) in addition to comparison of the findings and discussion of potential causal links of Parmentier et al. (2015) as well as Rehder et al. (2020).

Regional links between NDVI and sea ice have also been shown before (see e.g. Yu et al. 2021 and the paper by one of the co-authors of this study, Macias-Fauria et al. 2017). In addition, see also chapter 10 of the 2017 AMAP report (the authors incorrectly state on line 45 that this report did not include vegetation trends). Btw, reverse links have also been argued, where terrestrial vegetation growth lowers surface albedo, affecting climate and subsequently sea ice loss (Zhang et al., 2020).

- Reply:
- Please note that we acknowledge the extensive previous work on NDVI in the paper, including the mentioned studies Yu et al. 2012 and Macias-Fauria et al. 2017 (see section 2.1 and Table 3). In addition, an in depth analyses has been, however, so far missing for the cryosphere parameters.
- Thanks for pointing out the misplacement of the phrasing on vegetation and AMAP. This sentence relates to Comiso and Hall (2014) mentioned before, so it has now been moved before the AMAP mentioning. Note, that we correctly list in Table A1 that the AMAP study considered vegetation.
- Thanks for pointing out Zhang et al., 2020, we now added in the discussion:
- “Vegetation change driven sea ice decline (albedo change caused surface heating imbalances, leading to sea level pressure anomalies) has been also suggested based on Earth system modeling Zhang et al. (2020).”

While many of these previous studies relied on models or reanalysis datasets, this study aims to use remote-sensing datasets as much as possible. However, the authors use the TTOP model to determine soil temperature at 2 m depth. While this model uses land surface temperature (LST) from MODIS as an input, it also uses reanalysis data when MODIS LST is unavailable. Moreover, it models the soil temperature depending on for example land cover and surface wetness. While the TTOP model is probably the best estimate we have for permafrost extent at the moment, it is still a (hybrid) model. If the authors wanted to compare to satellite data only, rather than reanalyses or models, it would have made more sense to compare to MODIS LST directly. Moreover, 2 m depth is rather deep in the Arctic, where the active layer is typically shallower than 1 m. Any warming signal would be strongly attenuated and lagged at 2 m depth, which makes it difficult to make instantaneous correlations.

- Reply:
- Indeed, the signal at 2m depth is attenuated. The reason to anyway use it comes from (1) that 2m depth is commonly used to represent permafrost presence (e.g. Obu et al. 2021, and product documentation) and (2) that initial observations in Bartsch et al.

(2023) pointed to potentially high correlations with sea ice. And our results in the new manuscript have confirmed these and demonstrate a significantly higher linkage with sea ice than all other parameters, despite of the attenuation. But we agree that the issue of attenuation should be discussed. Now added:

- “Correlations for MAGT were stronger than for vegetation parameters and had the highest significant fraction among all parameters despite the chosen depth of 2 m.”
- Note, that the datasets used come from ESA Permafrost CCI which were created using a transient model version of CryoGRID in order to obtain time series. A TTOP model was used in the past for the creation of the ESA DUE GlobPermafrost dataset representing equilibrium conditions 2000-2016, what cannot be used for our trend analyses.
- We now added:
- “The transient modeling approach allows for quantification of temperature change over time a
- have been used as it is also recommended to be used for permafrost extent estimation (Obu et al. 2021) and initial tests have shown variation from year to year despite attenuation (Bartsch et al. 2023).

The current study also shows correlations at short and long distances, but it is not clear whether these correlations have a common distant cause or whether they represent an internal dynamic in the Arctic. Are they due to large scale atmospheric circulation affecting both sea ice and the terrestrial variables? Or are they due to local feedbacks dominated by sea ice decline? Unfortunately, the answer to this question is left in the middle by the authors, who present the dataset as a baseline for further analyses of drivers and dependencies. The paper would have been much stronger if it included a proper discussion on the underlying causes for the apparent correlations.

- Reply: Literature suggests that the underlying dynamics differ between the analysed parameters. For example, as mentioned in the discussion, Sasgen et al. (2025) suggest large scale atmospheric circulation playing a role for ice sheets and permafrost. The linkage between sea ice in the proximity and SWE as well as NDVI has been pointed out regionally before. We agree that the discussion should be restructured to cover these aspects better and in more detail, also considering the above suggested references. We have now split the discussion into several parts and added a subsection “Similarities and causalities”.

While I generally appreciate the effort by the authors, they could have done a more detailed spatial analysis given the high spatial detail of the source data, and they should have provided better causal insights rather than showing correlations with little context. Unless the authors go beyond their basic presentation of the data, and given the fact that they urge others to take their dataset further, I feel like this manuscript in its current form would work better as a data paper (e.g. in ESSD) rather than as a research article.

- Reply: Please note that with the present setup new results are shown for all parameters, and specifically for the cryosphere this type of correlation analyses was not done to date.

Some further comments:

- Line 45: Vegetation is extensively discussed in the 2017 SWIPA report (see chapters 8 and 10).

- Reply: As mentioned above, the placement of the sentence is a mistake. This sentence refers to Comiso & Hall. We state the consideration of vegetation in AMAP/SWIPA in Table A1.

- Line 81: wouldn't frequent cloud cover be a problem for MaxNDVI as well? Easy to miss the peak season in frequent cloudy parts of the Arctic, adding uncertainty to interannual variability in peak NDVI values.

- Reply: Yes, this is why we have used TI-NDVI which is bi-monthly (16 days periods) instead of growing season summed NDVI (GSSNDVI) which is constructed on a daily basis (Park et al. 2016). See introduction, line 80 (original manuscript).

- Line 126-128: did this model result match the observations well?

- reply: it matches satellite based observations. We added: "A similar pattern was found through satellite observations by Pulliaianen et al. (2020)"

- Line 150: which parameters? Reference?

- reply: we now extended the sentence: "... cryosphere as well as vegetation parameters 2000 onwards, due to specifically the availability of MODIS and denser coverage by Landsat (see e.g. Tables 2 and 3)"

- Line 157: what's the pixel size?

- reply: 25km, but we have been using for sea ice basins aggregated values

- Line: 164-165: This dataset appears to contain only the trend over the entire time series, not the original high temporal data used for the correlations, and there are no details on how the underlying dataset was processed. Is there a reference describing this?

- We added the following texts in the data and methods sections:

- The following parameters related to the growing season as the yearly maximum NDVI (MaxNDVI), and time integrated NDVI (TI-NDVI) were computed and analyzed from these data series. Maximum NDVI (MaxNDVI) is the annual maximum NDVI value observed during the period of peak phytomass during growing season, typically in late July and early August for the Arctic (Frost et al. 2025), and somewhat earlier for the Boreal region below 60° N. The following datasets were processed:

- Moderate Resolution Imaging Spectroradiometer (MODIS), 2000–2019, using 16-day NDVI product from the Terra (MOD13A1, v. 6.1) with a spatial resolution of 500 m. Pixels with a Summary Quality Assurance value ≥ 3 (indicating cloudy and compromised observations) were removed.
- Time-integrated NDVI (TI-NDVI) is the sum of maximum NDVI values within set compositing periods during May–September, calculated for datasets with daily temporal resolution. TI-NDVI includes phenological variations throughout the growing season; therefore, it better represents gross primary production (Tucker and Sellers, 1986). NDVImax and TI-NDVI may exhibit different correlations with climatic parameters like temperature (Yan et al. 2022). No filtering processes or other amendments have been done to the datasets and we have pretty much followed the procedure in Frost et al. (2025).
- Line 177-178: This assumption appears reasonable, but did you test whether it's true?
 - Reply: Considering the whole Northern hemisphere (as well as Eurasia and North America separately), the average March SWE was found to be the closest monthly average to peak SWE by Pulliainen et al., (2020) [see extended data Table 1)]. We added the following text::
 - Average SWE in March was selected for this study and interpreted to represent maximum SWE conditions, following Pulliainen et al., (2020).
- Figure 1: could you give the area north of 60°N a different color from the one south of 60°N? Makes it easier to see the domain instead of just highlighting a latitudinal band.
 - Reply: adjusted
- Table 4: what's the resolution of the source data for sea ice area?
 - reply: 25km, added
- Line 201: entire northern hemisphere or only north of 60°N?
 - Reply: Thanks for pointing out, it should be north of 60°N
- Line 255: I can imagine that correlations to more distant basins may show up but here we need better argumentation for why these correlations appear because at these large distances it may just as well be large scale atmospheric circulation affecting both (e.g. teleconnections related to Rossby wave propagation).
 - Reply: Note that we come back to that in the discussion
- Line 269: a positive correlation may not be that surprising. For example, the Fram Strait is an area of sea ice export, which can actually be enhanced in warm summers because the strong sea ice melt makes the ice thinner and more mobile, subsequently leading to more

export through this area. As such, a warmer Arctic leads to more sea ice in the Fram Strait, explaining positive correlations. These kinds of internal dynamics need to be considered when interpreting correlations to sea ice.

- Reply: Thanks for pointing out, we added in the discussion, also referring to this reviewer comment: “Positive correlations for MAGT and vegetation indices were specifically found for the Fram Strait in the summer months. This region is an area of sea ice export (Smedsrud et al. 2017) with in cases higher sea ice fractions in warm summer months (Anonymous 2025).

- Line 275: do you mean figure A7 instead of A2?

- reply: thanks for spotting, changed

- Line 279-280: This is unclear. Relevant for TI-NDVI in what way?

- reply: added: “across the adjacent land area”

- Line 336: Please specify why solar absorption trends are increasing in this region. Is it less cloud cover or changes in surface albedo from e.g. earlier snow melt and/or shrubification?

- Reply: This observation comes from Letterly et al. 2018 (based on reanalyses), who discuss earlier snow melt timing as a general reason for reduction of albedo in some areas across the Arctic.

-Added: “potentially due to earlier snow melt timing”

- Line 376-377: This sounds interesting, but what would be the reason for this temporal lag?

- Reply: Thanks for spotting. There is a mistake here. ‘following’ is now replaced with ‘preceding’. The reasoning is provided at the end of the paragraph.

- Line 379-384: I’m not sure I’m following this. Why would warmer summers and increased absorption of radiation lead to regional cooling in the autumn?

- Reply: The observation is that there are summers with high NDVI followed by earlier sea ice formation in the autumn. The processes listed are rather speculation, thus we removed that part ‘(such as 4.1)’ and changed ‘turn into’ to ‘relate to’ and add a comment that more investigations are needed for clarification.

- Line 407: Unclear. Which “following ones”?

- reply: added: ‘after June’

- Line 433-435: I’m not sure how this agrees with Sasgen et al. (2024) since they explicitly state that they did not look at the influence of sea ice.
 - Reply: We refer here to long distance linkages in general, not specifically sea ice.
 - changed to: ‘Such long distance linkages across the Arctic were also found ice sheets and permafrost ...’

- Line 449: why define the abbreviation FT for “freeze-thaw” if you only use it one more time?
 - reply: changed

- Figure A2: please replace “source” in the caption with the actual reference.
 - reply: thanks for spotting. Added: Tommervik (2025)

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- Table A2: what does the “2000?” mean in the table caption?
 - Reply: Thanks for spotting. This column is now removed, this was from an old version of the table.

References in replies:

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Tucker, C. J., and Sellers, P. J. (1986). Satellite remote sensing of primary production. *Int. J. Remote Sens.* 16, 1395–1416. doi:10.1080/01431168608948944

Yan, J., Zhang, G., Ling, H., & Han, F. 2022. Comparison of time-integrated NDVI and annual maximum NDVI for assessing grassland dynamics, *Ecological Indicators*, 136, 2022, 108611 <https://doi.org/10.1016/j.ecolind.2022.108611>.

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Bhatt, U. S., Walker, D. A., Raynolds, M. K., Comiso, J. C., Epstein, H. E., Jia, G., Gens, R., Pinzon, J. E., Tucker, C. J., Tweedie, C. E., and Webber, P. J.: Circumpolar Arctic Tundra Vegetation Change Is Linked to Sea Ice Decline, *Earth Interact.*, 14, 1–20, <https://doi.org/10.1175/2010EI315.1>, 2010.

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Reviewer 2

The manuscript "Similarities between sea ice variations and satellite-derived terrestrial biosphere and cryosphere parameters across the Arctic" presents an analysis of primarily satellite-derived variables to examine the correlation between sea ice area in different basins across the Arctic, and NDVI, MAGT (mean annual ground temperature) and SWE. The objectives of the work were to extend earlier analyses to a pan-Arctic perspective and identify correlations between not only adjacent basins but also distant basins for all months of the year. A dataset has been produced (Arctic Sea Ice and Land Parameter Correlations - ASILaC), which appears to be the main product of the study and the authors state that this dataset will provide a baseline for future studies on common drivers of essential climate parameters and causative effects across the Arctic.

My main impression of the manuscript is that the work is essentially describing the results of a dataset that has been produced but it doesn't really go any further to examine or discuss the processes or mechanisms driving the correlations that have been found - for example why there can be simultaneously negative and positive correlations between sea ice in adjacent and distant basins and the terrestrial variables and what this means. A lot of the discussion comes across as a repeat description of the results, but with some comparison to previous studies, but it doesn't really shed new insight or provide suggestions for how the dataset could be utilised in future work to actually delve deeper into the "why" behind the results presented.

- Reply: we agree that the discussion needs to be restructured and extended with reference to more relevant studies (see also reviewer #1 comments and response). We have now added a subsection "Similarities and causalities"

I think this fine if it is the main objective of the work (as has been stated to a certain degree), and I think the title of the manuscript is appropriate, but as already mentioned by Reviewer 1, I feel that at the moment the manuscript, as it is, would work better as a data paper in a journal such as ESSD. I recommend that the main output of the analyses - the ASILaC dataset - be mentioned as one of the objectives in 1.2.4 as this appears to be the baseline that could be used in future studies.

- Reply: We agree that the creation of the dataset would be useful to add as an additional objective.
- Added "The results of the statistical analyses were combined into an open access dataset of Arctic Sea Ice and Land Parameter Correlations (ASILaC) to facilitate further analyses."
- Note, beyond the creation of the dataset, key novel points are the consideration of vegetation and cryosphere at the same time, previous studies have focused on one parameter only. A comparison through a consistent setup for vegetation and land cryosphere parameters was so far not made. This resulted in the identification of so far undocumented potential linkages, which led us to the decision to submit to The Cryosphere instead of ESSD.

Minor comments

Line 32 - missing references in "MORE"?

- Reply: thanks for spotting, removed

Line 44 - remove "thus" before "therefore"

- Reply: removed

Line 88 - first time CAVM is introduced. It is referenced but I think the full description of the abbreviation should be provided, as it was still not clear in Fig.1 what CAVM was referring to.

- Reply: now spelled out in both

Line 194. I did not fully understand why the trend analysis was only applied to the number of frozen days between 1 March to 31 July. Is this a period when the number of frozen days is likely to be most variable over long time series?

- Reply and text added: "This period characterizes spring thaw on the northern hemisphere (e.g. Mortin et al. 2012). A change in number of frozen days in this period represents a change in spring timing."

Mortin, J., T. M. Schröder, A. Walløe Hansen, B. Holt, and K. C. McDonald (2012), Mapping of seasonal freeze-thaw transitions across the pan-Arctic land and sea ice domains with satellite radar, *J. Geophys. Res.*, 117, C08004, doi:10.1029/2012JC008001.

Line 202 - The SWE data are provided at monthly time resolution but the authors have chosen to only use the March SWE as a proxy for the max SWE. Is there some reason for not analysing correlations between monthly SWE and monthly SIA? The authors discuss that high negative correlations of March SWE with SIA may have implications for wildlife and reindeer herding (Line 423-424) but presumably the variations of SWE throughout the whole winter period (and not just March) would also be just as important from this perspective?

- Reply: We have chosen March SWE for consistency with the annual representation of the other land parameters (mean annual ground temperature, TI-NDVI and Max-NDVI are annual measures). But we agree, it would be useful to also look into monthly SWE in a follow on study.
- Added text in data section: "For consistency with the annual representations of vegetation and permafrost,..."
- Added in the discussion: "SWE phenology as well as structure changes may also potentially affect wildlife (Bartsch et al. 2023). The analyses of additional months as well as further related observables might be therefore of added value."

Line 237. change "are" to "area"

- Reply: changed

Line 250. "statistiscal" to "statistical"

- Reply: changed

Fig.4, 5, 6, 7. Perhaps add into the caption that the numbers eg. Beau-1, Beau-2, Beau-3, are referring to the correlations for the different months of the year. I appreciate this might be quite obvious to most readers but I didn't get it immediately.

- Reply: added 'by basin for specific month: {basin name}-{month}' in all cases