

Northward Expansion of High-Stature Vegetation Reveals Net Surface-Cooling Feedbacks in the Majority of Canadian Boreal-Tundra Ecozones

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This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

Northward Expansion of High-Stature Vegetation Reveals Net Surface-Cooling Feedbacks in the Majority of Canadian Boreal-Tundra Ecozones
Amaogu et al.

Comments to authors

This paper by Amaogu and colleagues adds another layer to the fascinating and important ongoing discussion around climate mitigation solutions at high latitudes. Previous work, including our own, was mainly aiming to nuance the narrative that planting trees across the world, with increasing warming-driven potential at high latitudes, is the cheapest way to mitigate global climate warming, due to the carbon stored in their (aboveground) woody biomass, i.e. to warn against 'carbon tunnel vision'. Amaogu and colleagues take the next step by combining empirical evidence from various sources to critically evaluate the context dependencies of central aspects of the argument underlying the recommendation against tree-planting at high latitudes, and find that specific ecosystems, i.e. broadleaved and mixed forests as well as treed wetlands, show a cooling trend rather than the anticipated warming acceleration.

The expected warming acceleration is typically based on an expected development from low stature vegetation or bare ground towards taller, dark coniferous vegetation, and the relatively simple assumption about albedo being mainly the consequence of the colour of a 2D surface. That this particular transition actually causes a warming acceleration seems to be confirmed by the present study. Hence, the main novelty of the present study is that it presents empirical evidence from the last four decades suggesting that transitions to broadleaves and shrubs in the boreal and arctic zone, particularly after natural (fire) and human (logging) disturbances have not been sufficiently accounted for in previous modelling work or perspectives, which has led to the general narrative around tree-planting being a poor climate solution at high latitudes. Another important contribution is that albedo is not only a consequence of brightness of a 2D surface, but also the consequence of energy scatter by a 3D surface, which makes vegetation structure important, yet hitherto underappreciated. Finally, the paper presents some interesting context dependencies based on the hydrological regime, which has also not been fully accounted for previously. These represent important overlooked mechanistic links between the biosphere and atmosphere. Together, this makes this paper an important contribution of broad interest that warrants publication in a reputable outlet, such as Nature Communications.

With the caveat that I am no remote sensing expert, I do not have major concerns with the overall methodology of the paper.

Yet, I have my reservations with the implied recommendations for future management of the CBT. While I welcome the proposition that boreal afforestation should not be framed as a true or false solution, but more conditional, I do not think the fact that naturally expanding and (re)generating forests and wetlands show a cooling trend justifies recommending afforestation as a natural climate solution in this region for a number of reasons.

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There is a difference between natural tree-expansion and tree-planting, particularly if for forestry. The previous work referred to in the intro, including our own, does not argue against natural ecosystem development, but warrants caution about the risk of climate-motivated tree-planting schemes being effectively greenwashing of forestry. Hence, there is a bit of a strawman argument here.

- The way forestry is done also has fundamentally different consequences for biodiversity, so I miss at least a mentioning of potential consequences for biodiversity, which is central to nature-based solutions.

- Further, part of the motivation in the intro seems to be that natural migration of trees northwards can contribute to Canada's net-zero commitments. Such natural ecosystem developments do not fulfil the additionality requirement – it would have happened without human intervention – and hence should not be counted towards NDCs.

- How much of current afforestation is trying to establish the kinds of ecosystems that have the cooling benefit? According to some readily available statistics on Google, afforestation in Canada is still heavily dominated by black spruce and jack pine. Hence, it should be clear that the recommendation should be to move away from the fast-growing softwood needleleaf species towards the broad-leaved-dominated communities with the acclaimed climate mitigation benefits. Is it even feasible to do forestry in the remote regions of Canada in other ways than clear cut?

- Further the climate benefits only really exist as long as the trees are on the land, hence, there needs to be a clear recommendation against clear-cut forestry.

- It is pretty clear that forestry in wetlands has a net warming impact, due to decomposition of soil C, and sometimes increased methane emissions. So this needs to be clear from the recommendations too, unless you have solid evidence to suggest otherwise.

- I am not convinced by the use of palaeo-evidence to justify afforestation for several reasons:

- o First, the fact that similar vegetation communities were present during a period in deep history that was a lot warmer (11-19°C above current MAT) than even the most pessimistic climate projections to end of century (around 8°C), due only to natural processes, is a poor argument for promoting similar vegetation today when our climate system is pushed significantly out of its natural balance, and we are trying to aim for 1.5-2°C.

- o Secondly, I am not certain how similar any of the communities studied here really were to what I and my colleagues found in the palaeo-studies referred to, i.e. Kjær and Willerslev. One of the main findings from the Kjær et al. paper was that there is no modern analogue for the Late Pliocene-Early Pleistocene ecosystem the sedaDNA revealed. Hence, it is difficult to make direct inferences about how well this would have compared biophysically with present day planted tree communities. And again, it was a 11-19°C warmer Arctic than present, which I am not sure is something to aspire for. I doubt there was more permafrost during those times – permafrost is estimated to disappear completely from the Northern Hemisphere around 4.5°C above preindustrial levels - which makes these palaeo-analogues a little misplaced if one of the main messages is that these analogue communities can protect permafrost.

- While the method used in the paper captures the local heat fluxes, which is what immediately determines the permafrost thaw, one also needs to account for the greenhouse gas emissions in addition to the energy fluxes to claim a net climate benefit. This is not really captured by the methods, as far as I can see. This is okay, as one cannot capture everything, but warrants a bit of discussion in relation to long-term impact on the global energy balance.

Below, I list some minor suggestions for improvements

- Introduction

- o I am not convinced by the use of palaeo-evidence to justify afforestation, see points above.

- o I miss an introduction of the 'disturbed/undisturbed' terminology that is used in the results section. Is it fire and logging, just fire, or what is the 'disturbed pixel' category?

- Results

- o I am no remote sensing expert, but it is not completely clear to me how the land cover transitions are validated to create the true and false positives in table 1. Are they based on an observational dataset of land cover change recorded on the ground at specific sites, or is it validated against another remotely sensed dataset?

- o Not clear from the main text why you both present the exponential and linear trends. This may have to be explained a bit better, as many will not read the methods until the end of the paper.

- o Figure 3 caption: Wetland is misspelled. Also unclear which y-axis in the insets the cumulative black curve (total) relates to.

- o Figure 5g: You call this 'energy imbalance' in the caption. I can see how ground heat flux can be seen as the energy imbalance, but it has not really been explained in the text. And just to be sure I understand it correctly: You show that there is a positive ground heat flux throughout, i.e. energy is moving into the ground to either warm (or freeze which also requires energy) the soil and/or soil water, but that it is significantly lower in 2016-2023 at >50 deg. Lat – correct? Hence, the idea is that the energy imbalance at the surface is decreasing, but still positive..?

- o Figure 7: Panels are a little small and therefore difficult to read.

- Discussion

- o P19: This sentence seems incomplete: 'While Rotbarth et al. [24] reported no tree cover expansion at the boreal-tundra boundary, attributing it to demographic lags and stressors such as dispersal limits, nutrient scarcity, permafrost disturbance, and herbivory.'

- o P21: Sentence seems incomplete: 'Negative correlations for outward (cooling) fluxes and positive for inward (warming) fluxes (Table 6)'.

- o While there is a focus on summer albedo, I miss a discussion on winter and particularly spring albedo effects, which is when snow plays the strongest role. Generally, snow is missing from the discussion. Can you please elaborate (or speculate) a bit on the role of snow in your findings?

- o P21: 'not certainty' seems odd. 'uncertain' perhaps?

- Conclusions

o I think you need to rephrase some of the conclusions based on my comments above.

- Methods

o P28: I wonder if 'natural afforestation analogue' is a relevant term, and if it is, it needs ex-planation. Having now read the supplementary material as well, it is still not clear to me how exactly you used this dataset and whether what the rationale behind it is. Please ex-plain.

- Supporting material

o Sorry if this comes from an ignorant non-expert standpoint and/or if I misunderstood, but can you please explain to me why you mask out the snow albedo? A good chunk of the ra-tionale behind why high-stature veg has lower albedo has to do with veg-snow interac-tions, so it seems odd to me

(Remarks on code availability)

Reviewer #2

(Remarks to the Author)

Overall, I think that the core of the paper – forest expansion in Arctic boreal-tundra ecosystems could lead to surface cooling, depending on the type of forest vegetation – is a very interesting one and worthy of publication. The suggestion here is that if afforestation takes the form of mixed forests, broadleaf trees, and treed-wetlands, that would lead to increased albedo and more negative latent heat fluxes, and therefore greater surface cooling relative to needle-leaf, evergreen forests. However, this paper presents several component analyses that are not relevant to that core idea, and therefore detract from the main message of the paper.

Specifically, the analyses and text related to 1) “greening” and NDVI dynamics and their climatic controls, and 2) the existence of paleo-forests in the region, whereas potentially interesting, are not really relevant at all to the core message of the paper. The paper needs to be focused on 1) that afforestation in the Canadian boreal-tundra ecozones is likely to lead to increases in mixed forests, broadleaf forests, and treed-wetlands, and 2) these forest types will lead to surface cooling, as opposed to the surface warming that might result from increases in low-albedo (dark) and low-transpiration evergreen forests.

I would consider a much shorter and streamlined paper to be potentially publishable in Nature Communications.

Other comments:

1) The authors are quite bold in their assertion of using boreal afforestation as a strategic approach for mitigating warming. Studying afforestation, as a process that is occurring in tundra systems, makes sense, but I have not seen afforestation of tundra being discussed as an active management strategy for constraining climate change. I question whether this is even a feasible approach.

2) While I do think that the NDVI analysis should be dropped from this particular paper, I did have several issues with the analysis and presentation (Fig. 1). I assumed that the areas for disturbed and undisturbed pixels should sum to the total areas, but they do not. Why was it reasonable to use linear fits for all disturbed pixels and non-linear fits for all undisturbed pixels? In Figure 1c, the coefficients presented are linear; are these the linear slopes of the log relationships? I don't understand why there are sometimes lines connecting the seasonal data points in Figs. 1d-g); the lines don't make any sense, since you are not assuming a continuous relationship

3) I don't understand how you developed the scenario-simulated thresholds for shifts among Barren-Sparse-Moderate-Dense. Why is Barren-Sparse the same value as Moderate-Dense?

4) I know that the Methods need to appear after the Discussion, but I think that somewhere in the main body of the paper, it needs to be stated that landcover transitions are determined from a Canadian landcover dataset (VLCE2) and a global landcover dataset (GLFC V3). It was very hard to understand how you were conducting this analysis without that basic information.

5) Related to my first comment, it might be premature to call the prevailing narrative a “false solution.”

6) On page 21 – of course it is true that vegetation can act as a thermal buffer between ground and atmosphere and potentially protect permafrost from thaw; however, that is not really relevant for (consistent with) your findings as you suggest. The former is related to ground heat flux, whereas your results address the net flux. If greening buffers against thaw (i.e. reduces the ground heat flux), then it could also increase net radiation (heating not cooling).

7) Paleo-ecological evidence does not support your conclusions that vegetation structure determines the climate consequences of afforestation. It only suggests that this type of vegetation structural changes have occurred in the past – we do not have any evidence suggesting that this led to either cooling or warming.

(Remarks on code availability)

Reviewer #3

(Remarks to the Author)

I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

(Remarks on code availability)

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

Thanks for your revisions.

Most of my concerns have been addressed, but one general thing remains. I find it inconsistent that the authors claim to have reframed the paper to not provide management guidelines, as their study is observational in nature, and doesn't target management while still maintaining the word 'afforestation' throughout the paper. This terminology is from forest management. The meaning of the sentences would still be consistent with the study by changing to 'tree-expansion' or similar, but the hints to management would disappear. I believe changing this throughout would be necessary to make it acceptable for publication. I give some specific examples below.

With a few further adjustments, I think it can be accepted.

- 38: I would still much prefer if the authors would avoid the term 'afforestation', particularly in the abstract, as it implies active management that establishes tree-cover in areas that has not previously/recently been forested. According to the framing, which was partly implemented to address my (and partly reviewer 2's) concern in this regard in the first round of reviews, you no longer wish to provide guidance on management. Even if not directly written, the current wording still indicates that afforestation in the CBT may be a NBCS ('viable pathway to reduce ground warming'). I would be more comfortable with just stating the main novel contribution of the paper, by changing the last sentence of the abstract to something like: Here, we provide the first empirical evidence to show that mixed vegetation assemblages with high latent heat and outward energy export may reduce ground warming, suggesting that the spread of such assemblages in the Canadian boreal-tundra can moderate the positive climate feedback previously found as a result of conifer-driven fire-albedo feedbacks to the Earth's energy balance.
- 57: Again, I would avoid 'afforestation', and just write 'tree-expansion' or similar. If you want to include the term, you could write '...tree-expansion – including afforestation and natural tree-expansion ('borealisation')' or similar. You do it quite well in line 60-64
- 80-81: If you do not want to provide management guidance, why is it important that the structures can resemble afforested conditions? And if you want it in, it would need a ref (unless part of your results, in which case it would need to be moved to the appropriate section).
- 89: demonstrating misspelled
- 203-208: As far as I am concerned, this is as close to providing direct recommendation for CBT afforestation as a NBCS as one can get, without writing it explicitly. I do not think your data supports that, so I think you need to tone it down significantly, and moved to the discussion. It is not a result from this study, as far as I am aware.
- 420-422: I would also include natural tree-expansion, but otherwise, this way of mentioning afforestation and the tone of it is fine and appropriate. I think this, i.e. the discussion, is the right place to bring in the potential implication of your observational findings for how we should understand the climate mitigation potential of any tree-expansion into the Arctic, including due to active management, i.e. assisted regen or active planting/afforestation.

(Remarks on code availability)

Reviewer #2

(Remarks to the Author)

The authors have done a commendable job addressing our initial concerns with the manuscript. We have only a few minor comments:

- 1) On lines 73-74, the authors note that it has been predicted that "vegetation expansion into tundra" would enhance warming. This makes it seem that tundra is a barren ecosystem. I think the authors might mean to say "forest expansion into tundra."
- 2) In the caption for Figure 1, it says that for panels a-c the "horizontal" line is the median - I think it should be the "vertical" line.
- 3) For Figure 2, the order in which the colors appear vary from bar to bar, and I'm not sure if there is a reason for that. It seems as though the bars should be in order from low to high NDVI trend, from bottom to top.

(Remarks on code availability)

Reviewer #3

(Remarks to the Author)

I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

(Remarks on code availability)

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Introduction

We sincerely thank the Reviewers for their thorough assessment of our manuscript and for the constructive comments and recommendations. We appreciate the reviewers' careful reading of the study and their detailed feedback, which has greatly helped us refine both the presentation and interpretation of our results.

We have systematically addressed all comments and have made substantial revisions throughout the manuscript. These include clarifications of the study scope, improved methodological transparency, refinement of figures and terminology, and adjustments to the framing of the conclusions. We are grateful for the opportunity to revise the manuscript and believe the revised version more clearly communicates the study's contributions.

Key: The reviewers' comments are presented in blue, our responses in black, and the corresponding revisions in both the revised manuscript and the marked version are highlighted in purple.

1 Addressing Reviewer 1's comments

Comment 1: *“With the caveat that I am no remote sensing expert, I do not have major concerns with the overall methodology of the paper.”*

We sincerely thank the reviewer for this careful reading and for acknowledging the overall methodological framework. We appreciate the opportunity to clarify the interpretation and scope of the Discussion and Conclusion sections. Our study is observational and analytical in nature, and we have revised the manuscript to ensure that no statements can be interpreted as prescriptive management recommendations.

Comment 2: *“Yet, I have my reservations with the implied recommendations for future management of the CBT. While I welcome the proposition that boreal afforestation should not be framed as a true or false solution, but more conditional, I do not think the fact that naturally expanding and (re)generating forests and wetlands show a cooling trend justifies recommending afforestation as a natural climate solution...”*

Response: We respectfully clarify that the manuscript does not present afforestation as a recommendation or management strategy. As already addressed in Reviewer 2, Comment 33, our study is strictly observational and analytical, aimed at quantifying surface-energy flux responses associated with vegetation structural transitions across the CBT. References to afforestation are used solely as a conceptual vegetation-transition analogue within the broader scientific debate concerning vegetation structure and biophysical feedbacks. Our conclusions concern the conditional nature of surface-energy responses to vegetation structural change and do not constitute guidance for land management, tree planting, or forestry practices. To prevent potential misinterpretation, we have revised the Discussion to more explicitly state that the results should not be interpreted as prescriptive recommendations, but rather as empirical evidence describing observed vegetation–energy interactions.

See Discussion lines 342-347: *“As taller vegetation increasingly becomes a dominant feature across the CBT through ongoing natural vegetation transitions, these transitions provide a basis for examining how shifts in vegetation structure are associated with coupled changes in ground–plant–atmosphere energy flux partitioning under current climatic conditions, thereby offering insights that may be relevant for anticipating the impacts of afforestation in the CBT.”*

Comment 3: *“There is a difference between natural tree-expansion and tree-planting, particularly if for forestry. The previous work referred to in the intro, including our own, does not argue against natural eco-system development, but warrants caution about the risk of climate-motivated tree-planting schemes being effectively greenwashing of forestry. Hence, there is a bit of a strawman argument here.”*

Response: We agree with the reviewer that natural vegetation expansion and intentional tree planting represent fundamentally different processes. The original wording was *“Although the prevailing fire–albedo paradigm continues to justify excluding BAF from mainstream climate mitigation pathways by framing it as a ‘false climate solution,’ it simultaneously raises a critical question about observational validation.”*. We have revised this statement and the manuscript throughout to ensure that afforestation does not equate these processes or advocate planting activities.

This revision is now reflected in the Introduction (lines 60–66): *However, a number of recent studies that are conceptualized through fire–albedo constraints argue that increasing tree dominance in tundra regions north of the boreal treeline, whether through natural vegetation transitions or assisted ones toward taller, darker evergreen vegetation, can reduce surface albedo and modify ground and turbulent heat fluxes. These changes, in turn, may elevate wildfire risk, accelerate permafrost thaw, and increase carbon emissions [9–15].*

We have also reframed related statements and ensured that referenced studies are used in their correct context. Vegetation transitions analogous to afforestation are therefore considered only as structural land surface states within the surface energy framework, rather than as prescribed interventions. The Introduction has been revised accordingly to remove wording that could imply policy advocacy or contrast natural ecosystem development with forestry interventions.

Comment 4: *“The way forestry is done also has fundamentally different consequences for biodiversity, so I miss at least a mentioning of potential consequences for biodiversity, which is central to nature-based solutions. - Further, part of the motivation in the intro seems to be that natural migration of trees northwards can contribute to Canadas net-zero commitments. Such natural ecosystem developments does not fulfil the additionality requirement – it would have happened without human intervention – and hence should not be counted towards NDCs.”*

Response: We agree with the reviewer. Biodiversity implications are indeed central to ecosystem management. However, biodiversity responses are outside the analytical scope of the present study, which is confined to surface-energy flux dynamics. The manuscript was not intended to propose that naturally occurring vegetation transitions could be credited within mitigation accounting frameworks or Nationally Determined Contributions (NDCs). Our reference to natural vegetation change is biophysical rather than policy-oriented. The study quantifies observed surface-energy responses and does not address carbon accounting or additionality considerations. The revised manuscript introduction now explicitly does not contain any concept or wording related to policy implications in a way that would imply naturally occurring processes toward NDCs.

Comment 5: *“How much of current afforestation is trying to establish the kinds of ecosystems that have the cooling benefit? According to some readily available statistics on google afforestation in Canada is still heavily dominated by black spruce and jack pine. Hence, it should be clear that the recommendation should be to move away from the fast-growing softwood needleleaf species towards the broad-leaved-dominated communities with the acclaimed climate mitigation benefits. Is it even feasible to do forestry in the remote regions of Canada in other ways than clear cut? -”*

Response: We thank the reviewer for this insightful observation. We agree that contemporary afforestation and reforestation practices may differ substantially in species composition, structural characteristics, and management objectives. Our analysis does not evaluate existing planting programs or recommend specific silvicultural strategies.

The focus of this study is the surface energy behaviour associated with vegetation structural states observed across the CBT, irrespective of whether such states arise from natural expansion, regeneration, or hypothetical management interventions. The cooling and warming tendencies we report are therefore linked to vegetation structure and associated energy flux partitioning, rather than to specific planted species or forestry systems.

We acknowledge that operational forestry decisions, including species selection and harvesting methods, involve ecological, economic, and logistical constraints beyond the scope of the present work. Accordingly, we have clarified in the revised Discussion that our conclusions concern biophysical energy balance responses to vegetation transitions and should not be interpreted as prescriptions for forest management or afforestation policy. At the same time, we note that the revised Discussion addresses the reviewer's point at a process-based level:

See Discussion lines 418–425: *“Nevertheless, our results provide a structural and process-based basis for understanding how vegetation pathways influence surface energy balance and thermal responses in northern environments. In this context, the biogeophysical outcomes of afforestation or assisted natural regeneration in the CBT are likely to vary significantly depending on the resulting vegetation composition. Where mixedwood, broadleaf, and wetland-treed systems dominate, surface cooling tendencies have been observed, whereas conifer-dominated expansion is more consistently associated with warming responses”.* ” This statement shows that broadleaf and mixedwood systems can be likely associated with cooling tendencies, whereas conifer-dominated systems are associated with warming, thereby implicitly supporting a shift away from fast-growing softwood needleleaf species toward broadleaf-dominated communities. Our discussion framing therefore aligns with the reviewer's point while remaining within process-based interpretation rather than prescriptive recommendation.

Comment 6: *“Further the climate benefits only really exist as long as the trees are on the land, hence, there needs to be a clear recommendation against clear-cut forestry.”*

Response: We acknowledge the reviewer's point regarding land-use practices. The present study does not evaluate forestry systems or harvesting strategies. Its objective is limited to quantifying observed relationships between vegetation structural transitions and surface-energy flux behaviour. The manuscript has been revised to avoid language implying forestry guidance or management prescriptions.

Comment 7: *“It is pretty clear that forestry in wetlands has a net warming impact, due to decomposition of soil C, and sometimes increased methane emissions. So this needs to be clear from the recommendations too, unless you have solid evidence to suggest otherwise.”*

Response: We agree that vegetation change or forestry activities in wetland environments can have distinct greenhouse gas implications, particularly through soil carbon decomposition and methane emissions, as well established in the literature. The cooling tendencies reported for treed wetland transitions therefore refer strictly to energy balance behaviour and should not be interpreted as an assessment of net climatic consequences. We have added key sentences to the Discussion to acknowledge this limitation explicitly:

See Discussion lines 413–418: *“The cooling tendencies identified here reflect outward surface energy export and reduced ground heat uptake, contributing to reduced surface warming, yet are not directly equatable to net climate forcing benefit, particularly in wetland-dominated systems transitioning to treed landscapes, where disturbance-driven methane emissions, not accounted for in this study, may limit the magnitude of the climatic benefit of vegetation change for climate warming.”*

See Discussion lines 426–430: *“It is important to note, however, that this study does not resolve the full climate implications of vegetation structural change across the CBT. In particular, the coupled energy and greenhouse gas consequences, especially methane dynamics, where carbon uptake and emissions may diverge, must be better understood before the observed surface cooling tendencies can be extended to net climate conclusions.”*

These revisions directly address the reviewer’s concern by clarifying that wetland-related cooling tendencies are interpreted within an energy balance framework and do not imply net climate benefit without accounting for methane and soil carbon feedbacks.

Comment 8: *“I am not convinced by the use of palaeo-evidence to justify afforestation for several reasons..”*

Response: We agree that palaeo-ecological evidence should not be used to justify or prescribe contemporary afforestation strategies or management decisions. To avoid any potential over-interpretation, we have removed the palaeo-ecological discussion from the revised manuscript. Our conclusions are based entirely on contemporary observational analyses of vegetation change and surface-energy flux behaviour.

Comment 9: *“While the method used in the paper captures the local heat fluxes, which is what immediately determines the permafrost thaw, one also needs to account for the greenhouse gas emissions in addition to the energy fluxes to claim a net climate benefit. This is not really captured by the methods, as far as I can see. This is okay, as one cannot capture everything, but warrants a bit of discussion in relation to long-term impact on the global energy balance. ”*

Response: We thank the reviewer for this important observation. Our analyses focus on surface energy-balance components (net radiation, latent heat, sensible heat, and ground heat flux) that directly govern land–atmosphere energy exchange and ground thermal behaviour. We acknowledge that greenhouse-gas feedbacks represent an additional dimension of climate impact that is not explicitly resolved within this framework (as we have already addressed in your Comment 7. Accordingly, we have revised the Discussion to state this limitation more clearly and to emphasize that our results describe biophysical surface-energy responses rather than a complete assessment of net climate forcing. A full evaluation of the climate implications of vegetation transitions in the CBT would require coupled treatment of both energy-balance dynamics and carbon–methane feedbacks, which lies beyond the scope of the present study.

Comment 10: *“ I miss an introduction of the ‘disturbed/undisturbed’ terminology that is used in the results section. Is it fire and logging, just fire, or what is the ‘disturbed pixel’ category?”*

Response: We have added a clear definition of “disturbed pixels” to refer to those that, within the 38-year study period, were affected at any point by wildfire, harvest, or both.

See Main Paper Figure 1 caption: *“(a) Linear NDVI trend summary for all statistically significant pixels (both disturbed and background); (b) linear NDVI trend summary for disturbed pixels (harvest and fire).”*

See Results lines 135–139: *“Greening trends were consistent across both disturbed (fire- and harvest-affected) and background CBT conditions (i.e., full landscape conditions without explicit disturbance masking)...”*

Comment 11: *“I am no remote sensing expert, but it is not completely clear to me how the land cover transitions are validated to create the true and false positives in Table 1. Are they based on an observational dataset of land cover change recorded on the ground at specific sites, or is it validated against another remotely sensed dataset?”*

Response: We thank the reviewer for this important question. The landcover transitions are not validated against ground-based observations or an independent external landcover dataset. Instead, we apply a two-step consistency and validation framework. First, the landcover transition dataset is quality-controlled using a temporal consistency criterion (“meaningful ecological change”), whereby only pixels with stable end states and ecologically consistent transition trajectories are retained.

Second, NDVI trends is used as an independent observational proxy to evaluate the consistency of these transitions. NDVI trend slopes are reclassified into greening and browning categories and intersected with the 2022 final pixel-wise landcover transition raster. True positives (TP) correspond to pixels where NDVI-derived trends agree with landcover transitions, while false positives (FP) represent cases where NDVI indicates change not supported by the landcover transition. Precision is then computed as $TP/(TP + FP)$, as reported in Table 1 (Main paper)

In this way, NDVI provides an independent continuous benchmark for assessing the plausibility and consistency of categorical landcover transitions, rather than serving as an external validation dataset. We have also revised the Methodology subsection 4.6 Line 574-589 and Supplementary Information (Section 6) to explicitly describe this validation framework and improve clarity.

Comment 12: *“Not clear from the main text why you both present the exponential and linear trends. This may have to be explained a bit better, as many will not read the methods until the end of the paper.”*

Response: We thank the reviewer for this helpful comment. We agree that the rationale for presenting both linear and exponential NDVI trends should be clearly stated in the main text. Accordingly, we have revised the Results section to explicitly clarify their complementary roles. See Results lines 141–145: *“Adopting both linear and exponential NDVI trends in this study proved relevant for capturing complementary vegetation dynamics, with linear trends representing steady change and exponential trends capturing localized rapid expansion that would otherwise be underestimated by a linear-only approach.”*

Comment 13: *“Figure 3 caption: Wetland is misspelled. Also unclear which y-axis in the insets the cumulative black curve (total) relates to.”*

Response: We thank the reviewer for noting this. The spelling of “Wetland” has been corrected in Figure 3 (main paper). We have also revised the figure caption to clarify that the cumulative black curve (Fire + Harvest) is plotted on the same primary (left) y-axis as the fire curve, representing cumulative fire-related area, while harvest is shown on the secondary (right) y-axis.

Comment 14: *“Figure 5g: You call this ‘energy imbalance’ in the caption. I can see how ground heat flux can be seen as the energy imbalance, but it has not really been explained in the text. And just to be sure I understand it correctly: You show that there is a positive ground heat flux throughout, i.e. energy is moving into the ground to either warm (or freeze which also requires energy) the soil and/or soil water, but that it is significantly lower in 2016-2023 at ̳50 deg. Lat – correct? Hence, the ideas is that the energy imbalance at the surface is decreasing, but still positive..?”*

Response: We thank the reviewer for this careful reading and clarification request. In our study, the term “energy imbalance” refers to the ground heat flux (G), which is computed as the residual of the surface energy balance and represents the portion of energy transferred into the ground. A positive ground heat flux therefore indicates net energy transfer into the soil system. As correctly interpreted by the reviewer, our results show that ground heat flux remains positive throughout the study period but decreases in magnitude during 2016–2023 at latitudes greater than 50°N. This indicates that while energy continues to be transferred into the ground, the amount of energy entering the subsurface has reduced over time. We agree that this interpretation was not sufficiently clear in the original text. Accordingly, we have revised the Figure 5 caption to explicitly define “energy imbalance” as ground heat flux (G) and to clarify that the observed trend reflects a reduction in the magnitude of positive ground heat flux rather than a reversal of its direction.

See Figure 5 caption: *“...Latitudinal gradients of sensible heat flux, latent heat flux, ground heat flux (which we assumed also as energy imbalance (G), i.e., the residual required to close the energy budget)...”*

Comment 15: *“Figure 7: Panels are a little small and therefore difficult to read.”*

Response: We thank the reviewer for this observation. We agree that readability is an important aspect of figure presentation. In the revised manuscript, we have improved Figure 7 by increasing font sizes (axis labels, tick labels, and annotations), repositioning the legends within the panels, and adjusting subplot spacing to better utilize the available figure width. We note that the current rendering in Overleaf may appear constrained due to compilation scaling. However, the original figures are generated at high resolution, and the final published version will be provided in full-resolution format in accordance with journal guidelines, ensuring improved clarity and readability.

Comment 16: *“P19: This sentence seems incomplete: ‘While Rotbarth et al. [24] reported no tree cover expansion at the boreal–tundra boundary, attributing it to demographic lags and stressors such as dispersal limits, nutrient scarcity, permafrost disturbance, and herbivory.’”*

Response: We have completed the sentence in rephrased version of as seen in line 335-338: *This interpretation contrasts with Rotbarth et al. [23], who reported limited tree expansion at the boreal tundra boundary and attributed it to demographic and environmental constraints*

Comment 17: *“P21: Sentence seems incomplete: ‘Negative correlations for outward (cooling) fluxes and positive for inward (warming) fluxes (Table 6)’.”*

Response: We have rephrased the sentence as seen in line 391-393: *Across ecozones, outward (cooling) flux tendencies are associated with reduced warming tendencies, whereas inward (warming) tendencies correspond to stronger warming (Table 6).*

Comment 18: *“P21: While there is a focus on summer albedo, I miss a discussion on winter and particularly spring albedo effects, which is when snow plays the strongest role. Generally, snow is missing from the discussion. Can you please elaborate (or speculate) a bit on the role of snow in your findings?”*

Response:

Response: We thank the reviewer for highlighting the importance of winter and especially spring albedo dynamics, and the role of snow in modulating vegetation–climate interactions. In response, we have explicitly incorporated seasonal snow effects into our analysis by computing ecozone-specific seasonal and annual decadal albedo changes under both snow-free and full seasonal conditions (Supplementary Table 17).

To distinguish vegetation-driven responses from those modulated by seasonal snow, and to isolate the radiative contribution of vegetation structure from snow–vegetation interactions, we introduced a comparative framework between snow-free and snow-included conditions. This is now presented in the Results (lines 271–276): *To distinguish vegetation-driven responses from those modulated by seasonal snow, and to isolate the radiative contribution of vegetation structure from snow–vegetation interactions, we calculated mean percent decadal albedo changes...* The Discussion has also been expanded (lines 364–374) to interpret these findings: *Comparing snow-free conditions, which isolate vegetation structural effects, with full seasonal conditions shows that vegetation change alone does not consistently produce surface darkening...* These additions explicitly address winter and spring dynamics. In particular, our results show that snow-free albedo responses are predominantly positive. However, the inclusion of snow albedo introduces strong seasonal variability, with the largest divergence occurring during the spring transition period.

Comment 19: *“P21: ‘not certainty’ seems odd. ‘uncertain’ perhaps?”*

Response: This has been removed.

Comment 20: *“I think you need to rephrase some of the conclusions based on my comments above.”*

Response: Our conclusion has been revised to reflect the reviewer’s comments.

Comment 21: *“P28: I wonder if ‘natural afforestation analogue’ is a relevant term, and if it is, it needs explanation. Having now read the supplementary material as well, it is still not clear to me how exactly you used this dataset and whether what the rationale behind it is. Please explain.”*

Response: We appreciate this comment. The term “natural afforestation analogue” and its description have been removed, as this dataset was part of a preliminary analysis and not used in the final methodology. The revised manuscript now includes only relevant datasets to avoid confusion.

Comment 22: *“Sorry if this comes from an ignorant non-expert standpoint and/or if I misunderstood, but can you please explain to me why you mask out the snow albedo? A good chunk of the rationale behind why high-stature veg has lower albedo has to do with veg-snow interactions, so it seems odd to me”*

Response: We thank the reviewer for this important clarification. In the initial submission, the explicit role of seasonal snow in modulating albedo was not fully resolved, and we agree that this represents an important gap. Following the reviewer’s comment, we have now incorporated snow effects directly into our analysis. As also described in our response to Comment 22, we evaluated albedo under snow-free conditions to isolate vegetation structural effects, and compared these with albedo responses across the full seasonal cycle where vegetation–snow interactions are active (Supplementary Table 17; Results lines 271–276). The methodological rationale and its implications are now clarified in the Results and Discussion (lines 364–374).

2 Addressing Reviewer 2's comments

Comment 23: *“Overall, I think that the core of the paper – forest expansion in Arctic boreal-tundra ecosystems could lead to surface cooling, depending on the type of forest vegetation – is a very interesting one and worthy of publication. The suggestion here is that if afforestation takes the form of mixed forests, broadleaf trees, and treed-wetlands, that would lead to increased albedo and more negative latent heat fluxes, and therefore greater surface cooling relative to needle-leaf, evergreen forests. However, this paper presents several component analyses that are not relevant to that core idea, and therefore detract from the main message of the paper”*

Response: We thank the reviewer for this important observation. We agree that, in the previous version, the breadth and sequencing of methodological components may have obscured their direct relevance to the core objective. In the revised manuscript, we have (i) restructured the Methods into a clear progression from NDVI extraction → NDVI trend fitting → landcover transition → energy flux attribution, (ii) removed or de-emphasized components with limited contribution (e.g., logistic trend fitting, now excluded), and (iii) clarified the role of NDVI as a continuous observational basis for detecting vegetation change and cross-validating landcover-derived transitions prior to linking them to surface-energy flux dynamics (see Sections 4.2–4.8) in the main paper. These revisions explicitly align all analytical steps with the central focus of our paper on vegetation-type-dependent surface cooling and warming driven by ongoing high-stature vegetation intensification across the CBT.

Comment 24: *“The paper needs to be focused on 1) that afforestation in the Canadian boreal-tundra ecozones is likely to lead to increases in mixed forests, broadleaf forests, and treed-wetlands, and 2) these forest types will lead to surface cooling, as opposed to the surface warming that might result from increases in low-albedo (dark) and low-transpiration evergreen forests”*

Response: We thank the reviewer for this suggestion and agree that vegetation type dependent surface energy feedbacks and surface thermal (cooling versus warming) responses are central to the manuscript. In the revised version, we have clarified the paper objective in the Introduction and revised the manuscript to align more explicitly with this focus:

See lines 80–84: *“Natural transitions toward treed landscapes modify surface energy fluxes in ways that can structurally resemble afforested conditions. They therefore can provide a useful empirical basis for understanding how vegetation change, whether natural or assisted, influences surface energy dynamics across the CBT.”*

See lines 114–121: *“Therefore, in this study, we present a multi-decadal observation constrained assessment of how vegetation structural transitions influence surface energy dynamics across the CBT. By integrating satellite derived vegetation structure information with meteorological and energy flux records spanning 1986 to 2023, we evaluate whether transitions toward higher stature vegetation states are consistently associated with surface warming or cooling responses, thereby providing an observation based perspective on the biogeophysical consequences of treed landscape development in the CBT.”*

We have also streamlined the Results and Discussion to explicitly emphasize how different vegetation pathways, including mixed forests, broadleaf dominated systems, and treed wetlands versus evergreen forests, are associated with contrasting surface energy responses, with the former generally linked to cooling and the latter to potential warming. While afforestation is not directly evaluated as a management scenario, our analysis is based on observed vegetation structural

transitions across the CBT derived from NDVI and independent land cover datasets. These transitions provide an empirical basis to inform potential afforestation outcomes, as they represent vegetation pathways that modify surface energy fluxes and thereby determine the resulting cooling or warming responses.

Comment 25: *“While I do think that the NDVI analysis should be dropped from this particular paper . . .”*

Response: We thank Reviewer 2 for the suggestion and agree that the manuscript should remain tightly focused on vegetation type dependent surface energy feedbacks. We have carefully reconsidered the role of NDVI and respectfully retain it because it is methodologically integral to the study design, the causal chain evaluated, and the interpretation of the reported results. Specifically, NDVI uniquely provides continuous detection of vegetation change, independent validation of land cover transitions, and separation of vegetation change directionality required for surface energy flux attribution.

Our analysis is built on “30 m Landsat based NDVI trends from 1986–2023 linked with land cover transitions, meteorological records, and surface energy fluxes,” where NDVI serves as the continuous observational basis for detecting vegetation change. Because the study evaluates multi decadal structural transitions, climatic trends (temperature and precipitation) are required to define environmental context and identify drivers of the northward high stature vegetation shifts reported in Tables 2 and 3.

Land cover data alone identifies discrete transitions but cannot capture gradual vegetation intensification, within class structural change, or continuous ecozonal and latitudinal dynamics (Tables 2–3; Methods Subsection 4.5–4.8.). NDVI therefore provides the analytical starting point for resolving these transitions before linking them to surface energy trends.

NDVI also provides an independent consistency check on land cover transitions derived from VLCE2 and GLFC V3 datasets (Supplementary Subsection 6.2, Table 3, Fig. 5). NDVI trend exceedance patterns align with observed transitions, with precision metrics reported in Table 1, confirming that detected greening reflects persistent structural change rather than classification artefacts.

Importantly, NDVI enables separation of greening and stable vegetation regimes, which is required to evaluate their associations with net energy flux trends (Supplementary Tables 18–19). Without this separation, it would not be possible to robustly link increasing high stature vegetation to enhanced outward energy export and surface cooling tendencies.

Removing NDVI would eliminate the continuous basis required to define vegetation change dynamics, validate transitions, and quantify their energy impacts. For these reasons, we retain NDVI, while revising the manuscript to reduce its interpretive prominence and present it strictly as an observational and validation component supporting the primary surface energy analysis.

Comment 26: *“.... I did have several issues with the analysis and presentation (Fig. 1). I assumed that the areas for disturbed and undisturbed pixels should sum to the total areas, but they do not. Why was it reasonable to use linear fits for all disturbed pixels and non-linear fits for all undisturbed pixels? In Figure 1c, the coefficients presented are linear; are these the linear slopes of the log relationships? . . .”*

Response: We believe that a typographical error may have contributed to the reviewer’s interpretation. In the original manuscript, we inadvertently used the word “on” instead of “and” in the phrase: *“Linear NDVI trend summary for all statistically significant (both disturbed on undisturbed) pixels”*. This wording may have led to the assumption that disturbed and undisturbed pixel areas were expected to be additive. We have corrected this to “and” for clarity. We clarify that the area values shown in Figure 1a and Figure 1b were not intended to sum to total ecozone areas. The

panels represent different masked subsets rather than additive partitions. Specifically, Figure 1a shows the remaining areas of each ecozone after masking pixels with insignificant NDVI trends, water bodies and built-surface, whereas Figure 1b represents the remaining proportion of the ecozone after masking insignificant NDVI trend pixels, water bodies, built surfaces, and disturbed pixels (fire and harvest, See Supplementary Fig. 1). Figure 1c further isolates a subset of undisturbed pixels for which the linear trend either underestimated NDVI trend or did not reach statistical significance, but where the exponential model later detected significant trends. Consequently, Figure 1b and Figure 1c are internally consistent subsets, but they are not expected to reproduce the total ecozone areas reported in supplementary Table 1. The differences arise from the necessary masking procedures applied to exclude pixels not suitable for trend analysis, including disturbed regions, water bodies, and built surfaces, as described in the Methods and illustrated in Supplementary Fig. 1.

Comment 27: *“Why was it reasonable to use linear fits for all disturbed pixels and non-linear fits for all undisturbed pixels?”*

Response: Figure 1a and Figure 1b are derived from linear NDVI trend fits, as stated in the figure descriptions, with Figure 1b representing statistically significant trends within the undisturbed pixel domain. We clarify that nonlinear fits were not restricted to undisturbed pixels, nor were linear fits limited to disturbed pixels.

See line 474–517: *From the yearly peak NDVI time series stack derived in subsection 4.2, we fitted sequentially three models: nonparametric linear (Mann–Kendall with Theil–Sen slope), exponential (Equation 4-3, and logistic (7) to derive the pixelwise NDVI trends across the study area. For linear trend...”*

Nonlinear analyses were conducted independently of disturbance classification using the log-linear formulation of the transformed exponential growth model (Equations 4–6) and the logistic model (Equation 7). These fits were evaluated across both disturbed and undisturbed pixels; however, statistically significant nonlinear (log-linear regression) trends were predominantly detected within undisturbed pixels, while they were negligible for disturbed pixels at the ecozone scale. This motivated the focus of nonlinear interpretation on the undisturbed domain.

The comparison between Figure 1a (all pixels) and Figure 1b (undisturbed pixels) was designed to assess whether disturbance effects altered the detectability of greening trends. The similarity between both figures indicates that disturbance-related variability does not obscure dominant ecozone-scale greening patterns, supporting the use of the full NDVI pixel population in subsequent analyses. Figure 1c further illustrates that vegetation dynamics within undisturbed regions exhibit nonlinear behaviour detectable by alternative trend models. Because undisturbed pixels are less influenced by abrupt disturbance-driven spectral changes, these nonlinear patterns reflect non-constant rates of vegetation change consistent with temporally varying responses within the broader climatic context examined in this study.

Comment 28: *“In Figure 1c, the coefficients presented are linear; are these the linear slopes of the log relationships?”*

Response: The coefficients presented in Figure 1c do not represent linear NDVI trend slopes in the original NDVI domain. Rather, they correspond to the slope parameter r obtained from the log-linear regression used to estimate the exponential NDVI model. Because the Google Earth Engine platform does not currently support direct nonlinear regression, the exponential model was fitted by linearizing NDVI trajectories in logarithmic space. The formulation applied in this study as also presented in Methods section 5.1 is:

$$\ln(\text{NDVI}(t)) = \ln(\text{NDVI}_0) + r (t - t_0), \quad (1)$$

where t_0 denotes the baseline year (1986) and $\text{NDVI}_0 = \text{NDVI}(t_0)$. Under this formulation, the estimated coefficient r is the linear slope in log space (i.e., the slope of $\ln(\text{NDVI})$ with respect to time), rather than a linear NDVI change rate in the original NDVI domain. For interpretability, we back-transformed this parameter into a multiplicative per-year NDVI change rate:

$$r' = \exp(r) - 1. \quad (2)$$

This back-transformed parameter r' represents the proportional rate of NDVI change per year relative to the baseline state, such that NDVI trajectories evolve multiplicatively through time according to $\text{NDVI}(t) = \text{NDVI}_0(1 + r')^{(t-t_0)}$.

Comment 29: *“I don’t understand why there are sometimes lines connecting the seasonal data points in Figs. 1d-g); the lines don’t make any sense, since you are not assuming a continuous relationship”*

Response: We thank the reviewer for this observation regarding the graphical representation in Figures 1d–g. The seasonal data points are indeed discrete observations, and no continuous temporal interpolation was intended. The connecting lines were included solely to aid visual tracking of seasonal progression across categories and were not meant to imply continuity or functional dependence between adjacent seasonal values. To avoid any potential misinterpretation, we have revised the figures by removing the connecting lines and retaining only the discrete seasonal markers.

Comment 30: *“(3) I don’t understand how you developed the scenario-simulated thresholds for shifts among Barren-Sparse-Moderate-Dense.”*

Response: We thank the reviewer for this important question. The scenario-simulated thresholds were introduced because no benchmark exists in the literature defining the NDVI trend magnitude required for a land-cover transition within a fixed timeframe. To address this, we defined NDVI ranges based on empirically reported vegetation density ranges: barren (−0.01–0.10), sparse (0.10–0.30), moderate (0.30–0.60), and dense (0.60–0.80).

Using these NDVI ranges, we performed 5000 Monte Carlo simulations per originating class (excluding dense), drawing random starting NDVI values and computing the slope required for trajectories to cross into the next class over 38 years (1986–2022), as shown in supplementary Fig 4. Thin lines represent individual simulations, and thick lines represent median trajectories, with reported slope values corresponding to median required slopes that define inset equations in supplementary Fig. 4.

Among adjacent transitions, Sparse → Moderate yields the smallest median slope ($\sim 0.0028 \text{ NDVI yr}^{-1}$, Supplementary Fig. 4d), which we adopt as a conservative reference for a full class transition ($\sim 0.003 \text{ NDVI yr}^{-1}$). We then define threshold criteria as half of this minimum full-transition slope, yielding $0.0015 \text{ NDVI yr}^{-1}$ (linear) and $0.00132 \text{ NDVI yr}^{-1}$ (exponential). These represent infilling thresholds, indicating persistent within-class vegetation change rather than full class transitions (Supplementary Table 3). The minimum value ($0.00132 \text{ NDVI yr}^{-1}$) defines the lower bound of the binning interval for NDVI trend slope reclassification, while thresholds of $\geq 0.003 \text{ NDVI yr}^{-1}$ and $\leq -0.003 \text{ NDVI yr}^{-1}$ define greening and browning limits, respectively (Supplementary Table 3).

Comment 31: *“(3) Why is Barren-Sparse the same value as Moderate-Dense?”*

Response: The apparent similarity between transitions such as Barren–Sparse and Moderate–Dense arises from the normalized NDVI space and the fixed temporal constraint of the simulations, not from equivalent ecological processes.

Because class intervals are defined on a comparable NDVI scale, adjacent transitions require similar NDVI changes over the same 38-year period, resulting in similar slope magnitudes. In contrast, transitions spanning multiple classes require substantially larger slopes, as illustrated in supplementary Fig. 4a (Barren → Sparse) compared to Fig. 4c (Barren → Dense).

To avoid ambiguity, we have revised Supplementary Section 6.2 and the caption of Fig. 4 to clarify the simulation framework, trajectory interpretation, and slope derivation:

See Page 11: *Old caption: "Fig. 4: NDVI Monte-carlo simulation, estimating landcover transition slope. Blue lines are the linearly simulated trends while green is the exponential simulated trend."*

New caption: Fig. 4: NDVI Monte Carlo simulations estimating land cover transition slopes. Linear trends are shown in blue and exponential trends in green. Thick solid (blue) and thick dashed (green) lines indicate the mean of each simulation set, with corresponding equations shown in the inset of each panel.

We now explicitly describe the representation of individual and median trajectories, the basis of slope similarity across adjacent transitions, and the larger slope requirements for multi-class transitions (Supplementary Fig. 2).

Comment 32: *"3) I know that the Methods need to appear after the Discussion, but I think that somewhere in the main body of the paper, it needs to be stated that landcover transitions are determined from a Canadian landcover dataset (VLCE2) and a global landcover dataset (GLFC V3). It was very hard to understand how you were conducting this analysis without that basic information."*

Response: We agree that explicitly identifying the land-cover datasets in the main body improves clarity. In the revised manuscript, we now state directly that land-cover transitions were derived from two independent products: the Canadian Virtual Land Cover Engine (VLCE2) and the Global Land Cover Facility dataset (GLFC V3). This clarification has been inserted at the point where land-cover transitions are first introduced to ensure that the analytical framework is immediately clear to readers.

See lines 125–127: *"To quantify vegetation structural change across the CBT, we combined Landsat-derived NDVI trends (1986–2023; Supplementary Tables 2–3) with vegetation type transitions from independent land-cover datasets (VLCE2 and GLFC; Methods subsection 4.3–4.6)"*

Comment 33: *"1) The authors are quite bold in their assertion of using boreal afforestation as a strategic approach for mitigating warming. Studying afforestation, as a process that is occurring in tundra systems, makes sense, but I have not seen afforestation of tundra being discussed as an active management strategy for constraining climate change. I question whether this is even a feasible approach. 5) Related to my first comment, it might be premature to call the prevailing narrative a "false solution."*

Response: We thank the reviewer for this thoughtful observation. We fully agree that large-scale boreal afforestation in tundra environments is not currently an established climate management practice. In response to the reviewer's concern, we have revised the manuscript to ensure that afforestation is no longer presented as a prescriptive mitigation strategy. Rather, afforestation is now examined within the broader scientific debate concerning vegetation structural change and its potential surface-energy consequences in high-latitude systems.

As outlined in the Introduction, the study is motivated by contrasting hypotheses in the literature. Several recent studies suggest that increasing tree cover in tundra regions may enhance warming through albedo reduction and altered heat partitioning, while observational evidence across the Canadian Boreal–Tundra transition remains limited. Our objective is therefore analytical and empirical: to evaluate whether large-scale vegetation structural shifts, including those analogous to afforestation, are associated with systematic warming or cooling responses in observed surface-energy fluxes.

See Introduction *lines 80–84*: “Natural transitions toward treed landscapes modify surface energy fluxes in ways that can structurally resemble afforested conditions. They therefore can provide a useful empirical basis for understanding how vegetation change, whether natural or assisted, influences surface energy dynamics across the CBT”

See Introduction *lines 116–121*: “By integrating satellite-derived vegetation structure information with meteorological and energy flux records spanning 1986 to 2023, we evaluate whether transitions toward higher-stature vegetation states are consistently associated with surface warming or cooling responses, thereby providing an observation-based perspective on the biogeophysical consequences of treed landscape development in the CBT.”

See Discussion *lines 342–347*: “As taller vegetation increasingly becomes a dominant feature across the CBT through ongoing natural vegetation transitions, these transitions provide a basis for examining how shifts in vegetation structure are associated with coupled changes in ground–plant–atmosphere energy flux partitioning under current climatic conditions, thereby offering insights that may be relevant for anticipating the impacts of afforestation in the CBT.”

See Discussion *lines 418–425*: “Nevertheless, our results provide a structural and process-based basis for understanding how vegetation pathways influence surface energy balance and thermal responses in northern environments. In this context, the biogeophysical outcomes of afforestation or assisted natural regeneration in the CBT are likely to vary significantly depending on the resulting vegetation composition. Where mixedwood, broadleaf, and wetland-treed systems dominate, surface cooling tendencies have been observed, whereas conifer-dominated expansion is more consistently associated with warming responses.”

Within our study framework, afforestation is now interpreted as a vegetation-transition analogue rather than a policy recommendation. The manuscript evaluates how shifts toward higher-stature vegetation states influence radiative and turbulent energy exchanges, irrespective of whether such transitions arise from natural greening processes or hypothetical management interventions. Our conclusions concern the conditional nature of biophysical feedbacks and do not assert afforestation as a universal climate solution. We have also revised the manuscript to avoid presenting afforestation in the CBT as an active management strategy for constraining climate change.

We also appreciate the reviewer’s comment regarding the phrase “false solution.” We recognize that this wording may convey a stronger normative interpretation than intended. In the revised manuscript, we have moderated this language to ensure that the discussion remains focused strictly on biophysical mechanisms, observational evidence, and energy-balance responses rather than policy characterization.

Comment 34: “(6) On page 21 – of course it is true that vegetation can act as a thermal buffer between ground and atmosphere and potentially protect permafrost from thaw; however, that is not really relevant for (consistent with) your findings as you suggest. The former is related to ground heat flux, whereas your results address the net flux. If greening buffers against thaw (i.e. reduces the ground heat flux), then it could also increase net radiation (heating not cooling).”

Response: We thank the reviewer for this important comment. The concern appears to arise from a perceived mismatch between ground heat flux (G) and the net surface-energy trends discussed in the manuscript, particularly the statement that “greening is not only a carbon sink but can also be a biophysical buffer.” We clarify that no such mismatch exists

within our analytical framework and that the interpretation is consistent with how buffering and cooling tendencies are defined in this study.

Ground heat flux (G) is explicitly computed as the residual required to close the surface-energy budget (Main paper; Methods, Eq. 9), derived from net solar and thermal radiation minus latent (LE) and sensible (H) heat fluxes, and is analyzed alongside LE and H. The net energy-flux trend is not defined as net radiation, but as the combined contribution of all energy-balance trend components as computed below:

$$F_{\text{net}} = LE_{\text{trend}} + H_{\text{trend}} + G_{\text{trend}} \text{ (Methods Subsection 4.8; Eqs. 9–14).}$$

Thus, “net outward surface flux cooling or warming tendencies” refer to the integrated behaviour of turbulent and ground heat fluxes rather than any single radiative component. In this context, the reviewer’s statement that reductions in G could coincide with increases in net radiation is not inconsistent with our framework. We agree that such behaviour can occur depending on albedo and energy-partitioning dynamics. In our results, increases in net radiation were observed only in the Hudson Plain and Taiga Shield East ecozones during 1986–2023 (Table 5).

Within our framework, reduced ground heat uptake (decreasing or less positive G) represents the thermal buffering mechanism directly relevant to thaw-sensitive surfaces, while concurrent changes in LE and H regulate energy export from the surface. Accordingly, the cooling tendency discussed in the original draft (in Page 21) refers to the tendency of the net surface-energy flux (LE + H + G) trend to become more outward (more negative) relative to the 1986 baseline across most ecozones (except Hudson Plain and Taiga Shield East), indicating increased energy export away from the CBT surface system.

This interpretation is supported by Table 5, which shows approximately 9 to 30% reductions in net annual available energy by 2023 relative to 1986 across most ecozones, and by Table 6, where decreasing surface and near-surface air temperatures are associated with negative net energy-flux trends, while increasing temperatures correspond to positive trends. These relationships confirm the physical consistency of the combined LE + H + G framework with the thermal tendencies described.

Thus, under our sign conventions and analytical definitions of cooling tendencies (surface buffering) and negative net energy-flux trends when negative therefore indicate surface-cooling tendencies, or equivalently, reduced warming tendencies relative to the baseline state. Taking the reviewer’s valid concern into consideration, we now clarify that the buffering interpretation in the Discussion is linked specifically to the behaviour of the ground heat flux (G).

The broader surface-energy reduced warming tendencies are evaluated through the direction of the net energy flux trend (F_{net}). When F_{net} is outward (negative), energy is increasingly exported away from the surface, which can further enhance the effective buffering role of the ground heat-flux component. In addition, we have revised the Discussion to state explicitly that cooling and warming tendencies are assessed using the combined LE–H–G trend framework, while thaw relevance is interpreted primarily through the ground heat-flux component, particularly where the directions of G and F_{net} are physically consistent.

Comment 35: *“(7) Paleo-ecological evidence does not support your conclusions that vegetation structure determines the climate consequences of afforestation. It only suggests that this type of vegetation structural changes have occurred in the past – we do not have any evidence suggesting that this led to either cooling or warming. ”*

Response: We thank the reviewer for this thoughtful comment. We agree that paleo-ecological evidence does not directly constrain the sign or magnitude of surface-energy feedbacks associated with vegetation structure. Because the central objective of this manuscript is to quantify contemporary energy-flux responses using observational datasets, we have now

removed the paleo-ecological discussion to avoid implying mechanistic support beyond the empirical framework of the study.

Introduction

We sincerely thank the Reviewers for their thorough assessment of our manuscript, as well as for their constructive follow-up comments and recommendations after the first round of review and rebuttal. Following these comments, we have systematically addressed all concerns and made the required revisions throughout the manuscript. These revisions include clarification of terminologies, reduction of management-oriented framing, and adjustments to the framing of the conclusions, particularly within the Abstract. We are grateful for the opportunity to further revise the manuscript and believe the revised version now more clearly communicates the study's contributions.

Key: The reviewers' comments are presented in **blue**, our responses in **black**, and the corresponding revisions in the rebuttal are highlighted in **purple**. Also, in rebuttal **red** colour represent text quoted verbatim from the manuscript that has also been revised. In the marked manuscript, revisions retained from the first review round are highlighted in **purple** for continuity, while the key additional changes implemented during this second review round are highlighted in **cyan**.

1 Addressing Reviewer 1's comments

Comment 1: *"Most of my concerns have been addressed, but one general thing remains. I find it in-consistent that the authors claim to have reframed the paper to not provide management guidelines, as their study is observational in nature, and doesn't target management while still maintaining the word 'afforestation' throughout the paper. This terminology is from forest management. The meaning of the sentences would still be consistent with the study by changing to 'tree-expansion' or similar, but the hints to management would disappear. I believe changing this throughout would be necessary to make it acceptable for publication. I give some specific examples below.."*

We thank the Reviewer for the positive assessment of our revisions and for recognizing that most concerns have been addressed. We also appreciate this important clarification regarding the consistency between the observational nature of the study and the terminology used throughout the manuscript. In response, we have systematically replaced management-oriented terms such as "afforestation" and "BAF" with observationally consistent terminology including "tree expansion", "vegetation structural transitions", and "treed landscape development", while also removing or substantially toning down statements that could be interpreted as management recommendations outside the Discussion section.

Comment 2: *"38: I would still much prefer if the authors would avoid the term 'afforestation', particularly in the abstract, as it implies active management that establishes tree-cover in areas that has not previously/recently been forested. According to the new framing, which was partly implemented to address my (and partly reviewer 2's) concern in this regard in the first round of reviews, you no longer wish to provide guidance on management. Even if not directly written, the current wording still indicates that afforestation in the CBT may be a NBCS ('viable pathway to reduce ground warming'). I would be more comfortable with just stating the main novel contribution of the paper, by changing the last sentence of the abstract to something like: Here, we provide the first empirical evidence to show that mixed vegetation assemblages with high latent heat and outward energy export may reduce ground warming, suggesting that the spread of such assemblages in the Canadian boreal-tundra can moderate the positive climate feedback previously found as a result of conifer-driven fire-albedo feedbacks to the Earth's energy balance...."*

Response: We thank the Reviewer for this important clarification and agree that the previous wording in the Abstract retained unintended management implications through the use of "afforestation". In response, we revised the final sentence

of the Abstract to remove management-oriented framing and adopted the Reviewer’s suggested wording to more clearly emphasize the observational and mechanistic contribution of the study.

See Abstract lines 36-41: *“Here, we provide the first empirical evidence to show that mixed vegetation assemblages with high latent heat and outward energy export may reduce ground warming, suggesting that the spread of such assemblages in the Canadian boreal-tundra can moderate the positive climate feedback previously found as a result of conifer-driven fire-albedo feedbacks to the Earth’s energy balance.”*

Comment 3: *“57: Again, I would avoid ‘afforestation’, and just write ‘tree-expansion’ or similar. If you want to include the term, you could write ‘...tree-expansion – including afforestation and natural tree-expansion (‘borealisation’)’ or similar. You do it quite well in line 60-64”*

Response: We thank the Reviewer for this important clarification and agree that “tree expansion” provides a more observationally consistent framing than “afforestation” in this context. Accordingly, we revised the original sentence to reduce management-oriented terminology and align the framing more closely with the observational nature of the study. The original sentence: *In this context, boreal tundra afforestation (BAF) within the CBT has become a central focus in discussions of vegetation–climate change interactions, given the region’s role as a critical frontier for both scientific understanding and broader policy contexts concerned with climate change adaptation* has been revised.

See lines 58–61 *“In this context, tree expansion within the CBT, including both natural vegetation expansion (‘borealisation’) and afforestation, has become an important focus in discussions of vegetation–climate interactions, given the region’s role as a critical frontier for understanding high latitude climate–ecosystem feedbacks.”*

Comment 4: *“80-81: If you do not want to provide management guidance, why is it important that the structures can resemble afforested conditions? And if you want it in, it would need a ref (unless part of your results, in which case it would need to be moved to the appropriate section).”*

Response:

We thank the Reviewer for this important clarification and agree that the comparison to “afforested conditions” was not necessary within the revised observational framing of the study and could imply unintended management-oriented interpretation. Accordingly, we removed the afforestation analogy and revised the paragraph to focus strictly on observational vegetation structural transitions and their influence on surface energy dynamics across the CBT.

See line 68–72 *While these studies, framed within the fire–albedo paradigm and emphasizing positive warming feedback mechanisms, have strongly influenced current interpretations of vegetation–climate feedbacks in the CBT, their synthesis simultaneously raises a critical question regarding whether the hypothesized warming responses are consistently supported by observations across the broader CBT.*

See lines 79–91 *“Over the past four decades, large portions of the CBT have undergone persistent greening, primarily through unmanaged expansion of shrubs, treed wetlands, and mixedwood systems [1–8]. These transitions toward taller and more structurally complex vegetation modify surface energy fluxes and therefore can provide an empirical basis for understanding how vegetation change influences surface energy dynamics across the CBT. Within this framing, if transitions toward higher stature vegetation inherently and uniformly enhanced surface warming, widespread ongoing greening would be expected to produce detectable regional-scale energy imbalances, including persistent increases in radiative forcing, elevated ground heat fluxes, and thaw-related surface responses. However, such patterns have not been systematically*

confirmed across the CBT, demonstrating that the biogeophysical consequences of northward high-stature vegetation transitions may be more complex than the prevailing fire–albedo paradigm suggests.”

Although not specifically flagged by Reviewer 1, we further revised the following introduction statement to better align the manuscript with the observational and mechanistic framing recommended throughout the review process.

See Lines 101–106. *This perspective is further supported by Ofose et al. [32], Dsouza et al. [36], who, using the Carbon Budget Model of the Canadian Forest Sector (CBM-CFS3) together with remote sensing and climate physics modeling at the western boreal margin, showed that under appropriate species mixing and ecological–hydrological conditions, afforestation can yield substantial net carbon-sequestration benefits even after accounting for albedo-related offsets.*

has been revised to:

This perspective is further supported by Ofose et al. [32], Dsouza et al. [36], who, using the Carbon Budget Model of the Canadian Forest Sector (CBM-CFS3) together with remote sensing and climate physics modeling at the western boreal margin, showed that species mixing and ecological–hydrological conditions determine the balance between albedo-related warming effects and carbon uptake associated with shrub and tree expansion.

Comment 5: “89: demonstrating misspelled”

Response: This has now been corrected.

Comment 6: “ 203-208: As far as I am concerned, this is as close to providing direct recommendation for CBT afforestation as a NBCS as one can get, without writing it explicitly. I do not think your data supports that, so I think you need to tone it down significantly, and moved to the discussion. It is not a result from this study, as far as I am aware.”

Response: Response: We thank the Reviewer for this important clarification and agree that the previous wording extended beyond the direct observational scope of the Results section by drawing explicit analogies to afforested states and anticipated afforestation responses. In response, we removed the management-oriented and interpretive framing and revised the text to focus strictly on the observed vegetation structural transitions and associated energy exchange analysis as seen below:

See lines 202-206 *Natural transitions toward shrub- and tree-dominated landscapes are known to modify surface energy exchange across high latitude environments [50-54]. To assess how these vegetation structural transitions influenced ground–plant–atmosphere energy exchange, particularly ground energy reduction through outward heat export, we compared ecozonal energy exchange trends between 1986 and 2023.*

Also, we revised a section of the Discussion where we implicitly toned down aspects related to afforestation-oriented interpretation and repositioned the broader implications within a more observational and mechanistic context, as recommended by Reviewer 1.

See Lines 338–351: *As taller vegetation increasingly becomes a dominant feature across the CBT through ongoing natural vegetation transitions, these transitions provide a basis for examining how shifts in vegetation structure are associated with coupled changes in ground–plant–atmosphere energy flux partitioning under current climatic conditions. These observations may also provide broader insight into how future vegetation transitions and northward expansion pathways can influence surface energy redistribution across northern environments [50, 52, 54]. It is known, and as also found in our study, that borealisation in the CBT toward shrub and tree dominated states, and the associated vegetation-mediated energy fluxes, are accompanied by distinct shifts in surface energy exchange that strongly influence ecological*

feedbacks in the permafrost-underlain CBT [74-77]. A central question is whether, and which, vegetation structural changes can generate cooling through energy redistribution that offsets the warming effects commonly associated with reduced reflectivity and increased sensible heat flux in taller canopies.

Comment 7: *“420-422: I would also include natural tree-expansion, but otherwise, this way of mentioning afforestation and the tone of it is fine and appropriate. I think this, i.e. the discussion, is the right place to bring in the potential implication of your observational findings for how we should understand the climate mitigation potential of any tree-expansion into the Arctic, including due to active management, i.e. assisted regen or active planting/afforestation.”*

Response:

We thank Reviewer 1 for this clarification and agree that the Discussion section is the appropriate place to contextualize the broader implications of the observational findings. In response, we revised the text to explicitly incorporate both natural and managed shrub and tree expansion pathways while maintaining a cautious and observational framing of the climatic implications.

See Discussion lines 417–422: *“In this context, the biogeophysical outcomes of both natural and managed shrub and tree expansion pathways in the CBT are likely to vary significantly depending on the resulting vegetation composition. Where mixedwood, broadleaf, and wetland-treed systems dominate, surface cooling tendencies have been observed, whereas conifer-dominated expansion is more consistently associated with warming responses.”*

Furthermore, as recommended by Reviewer 1, we revised the corresponding statement in the Supplementary Information to remove afforestation-oriented interpretation and maintain a more observational framing focused on vegetation transition dynamics and ecosystem responses.

See Supplementary Lines 151–155: *Moreover, by noting the points and period at which pixels shift between PFT or land-cover categories (for example, from shrubland into young forest), we can attribute spectral greening and browning to vegetation-change dynamics and uncover natural “afforestation analogues” in the CBT as points of further analysis for understanding vegetation type driven natural afforestation feasibility assessments.*

has been revised to:

Moreover, by noting the points and periods at which pixels shift between PFT or land-cover categories (for example, from shrubland into young forest), we can attribute spectral greening and browning to vegetation-change dynamics and identify natural vegetation transition analogues in the CBT for further analysis of vegetation-driven surface energy and ecosystem responses.

2 Addressing Reviewer 2's comments

Comment 8: *"The authors have done a commendable job addressing our initial concerns with the manuscript. We have only a few minor comments:"*

Response: We sincerely thank Reviewer 2 for the positive assessment of the revised manuscript and for the constructive follow-up comments. We appreciate the Reviewer's recognition of the revisions made and have carefully addressed the remaining minor comments as detailed below.

Comment 9: *"1) On lines 73-74, the authors note that it has been predicted that "vegetation expansion into tundra" would enhance warming. This makes it seem that tundra is a barren ecosystem. I think the authors might mean to say "forest expansion into tundra.""*

Response: We thank Reviewer for this important clarification and agree that "forest expansion into tundra" more accurately reflects the intended meaning and avoids implying that tundra is a barren ecosystem. Accordingly, the text has been revised as follows:

See Lines 74–76: *"... predicted that vegetation expansion into tundra would enhance localized warming..."* to *"... predicted that forest expansion into tundra would enhance localized warming..."*

Comment 10: *"2) In the caption for Figure 1, it says that for panels a-c the "horizontal" line is the median - I think it should be the "vertical" line."*

Response: It is indeed vertical line. we have corrected this and change it "vertical line" in Figure 1.

Comment 11: *"For Figure 2, the order in which the colors appear vary from bar to bar, and I'm not sure if there is a reason for that. It seems as though the bars should be in order from low to high NDVI trend, from bottom to top."*

Response: We thank Reviewer for this helpful observation. The varying vertical arrangement of the colored NDVI trend classes reflects the intentional visual layering of lower-percentage classes over higher-percentage classes within each ecozone to preserve the visibility of low-area trend categories that would otherwise be obscured. To improve clarity and interpretability, we revised the Figure 2 caption to explicitly clarify this plotting approach while noting that each colored segment still represents the absolute percentage contribution of its corresponding NDVI trend class.

See revised Figure 2 caption as follows: *Relative NDVI trend-class area by ecozone. Panels (a) and (b) show, for each ecozone, the percentage of area falling into each NDVI trend bin from the linear and exponential NDVI trends, respectively. Percentages are computed relative to each ecozone's total area. Within each ecozone, NDVI trend classes are visually layered from lower to higher percentage contribution to preserve the visibility of low-area trend categories, while each colored segment represents the absolute percentage contribution of its corresponding NDVI trend class. In the legend, labels outside parentheses refer to linear bins; labels inside parentheses refer to exponential bins. Dots mark ecozone area (km²), plotted against the right-hand (secondary) y-axis.*