

Review of “*Effect of future forest management on carbon and energy budgets in pine forests on mineral soil in southern Finland*” by Leskinen *et al.*

The study by Leskinen *et al.* examines the climate impact of different forest management scenarios in Finnish pine forests, considering carbon sequestration, surface albedo, and surface turbulent heat fluxes. The main finding is that carbon sequestration is the primary driver of the total climate impact, with lower harvest intensity resulting in the highest carbon sequestration and the most beneficial overall climate change mitigation. While changes in albedo and heat fluxes had minor counteracting effects, they only slightly reduced the climate benefits of the high-sequestration scenarios, confirming that all studied management practices maintained the forests as a net climate benefit.

I have several major criticisms, the first of which is related to the scoping. The research motives are unclear, and research hypotheses are missing. Why was this research carried out or needed? What do the authors seek to discover? How does this build on or compliment existing works? Related to this is the chosen time horizon for the analysis. No argument is made for the short (45-yr) time horizon, which is less than half the age of a typical Finnish pine rotation. If the rationale was to align with some climate policy target (which I suspect wasn't given the odd end year of the time series of 2054) then this is not communicated. It is rather obvious that a scenario in which forests are allowed to age/grow will build up higher C stores relative to those with higher harvest rates over such a short time horizon.

Another major criticism I have surrounds the methods, specifically, the use of the CO₂-equivalent metric for forcings occurring at the Earth's surface, and particularly for the turbulent heat flux changes. CO₂ emissions impact global climate by altering the radiative energy balance at the top of Earth's atmosphere, so characterizing the surface forcings in terms of “CO₂-equivalent” effects is flawed and misleading. This is especially the case for the turbulent heat flux changes since these are associated with energy re-distributions at the surface only, which do not directly impact Earth's radiative energy balance. The correct and fair comparison to CO₂ would be to quantify the effective radiative forcing (ERF) connected to the surface forcing, which requires looking at how the turbulent heat flux changes affect atmospheric profiles of temperature, humidity, and clouds (i.e., the radiative adjustments), and how the atmosphere attenuates the instantaneous albedo change forcing at the surface.

Quantifying ERF is challenging and requires coupled simulations, which I acknowledge is probably not an option available or feasible to the authors. Thus a reasonable compromise I think would be to quantify the top-of-atmosphere (TOA) radiative forcing from the surface albedo change and discuss how the turbulent flux change results might counteract or reinforce this via their impact on the TOA radiative adjustments. Looking at [Bright et al. \(2025\)](#) I find some statistical models (presented in Table S2 of the Supporting Information) that could be useful here.

In general the paper is long and not very well written and organized, and includes a lot of sections/text that are not so pertinent to communicating the core methods and key results. The Discussion reads more like an extended Results section and lacks important discussion

elements (e.g., study caveats/limitations, uncertainties, climate policy relevance, avenues for future research, etc.).

Other, more detailed comments are provide below.

Focused comments

Abstract: It is way too long. Aim for 200-250 words (it is currently ~430 words).

P2, L38-39: Forest albedos are not constant. For which timeframe do these values correspond (I presume growing season)?

P2, L38-49: There's a lot of detail here, but it's unclear what the takeaway message is. Given the scoping of the paper I would limit the focus of this section to the influence of forest composition and structure on albedo, since this is what is impacted by forest management.

P2, L50-56: Why the sudden shift in focus on harvest impacts? Up until this point there has been no linking of surface biophysics with forest management. Any why the focus on hydrology only, when harvests also impact the full suite of energy budget mechanisms under scope in the paper?

P2, L57-58: The reader learns here abruptly for the first time that this is a modeling study (it should not be assumed that the reader understands the methods/tools of the study based on information contained in the Abstract). It is better to build the case surrounding the value/need for models as a research tool in the previous paragraphs prior to stating that the present research is supported by them.

“Climate change mitigation potential” should be clearly defined somewhere here or in the succeeding paragraph.

P3, L64-66: Can the authors justify a 45-yr time horizon, which is relatively short for (even-aged) boreal forestry where rotations are typically 70-120 years?

P3-4, Sections 2.1.1 – 2.1.3: I suggest to move many of these details surrounding the JSBACH_FOM model mechanics to the Supporting Information, and consider first introducing the harvest scenarios (i.e, move section 2.4 up to here), since these are currently referred to on multiple occasions throughout sections 2.1 – 2.3.

P4, Yasso: Missing in the Yasso-related paragraphs is detail surrounding model initialization and spin-up. What efforts were taken to ensure that the model's initial state did not unduly influence the simulation results?

P4, Section 2.1.3 “Modeling of Carbon Balance”: JSBACH details are presented in terms of fluxes, whereas Yasso and C-cycle results (section 3.1) are presented as stocks/pools. Somewhere these should be reconciled, perhaps with an equation.

P5, Section 2.2: Is the described calibration procedure documented somewhere else? If so, please state that clearly here, and if not, please elaborate on what PFT parameters have been updated or modified. Was any spin-up of JSBACH_FOM executed? A spin-up period (for soil moisture) is important and 6-12 months seems pretty standard in offline studies.

P5, L132: Why the coarser EUR-44 and not EUR-11?

P6, L133-135: Which RCMs were used for the downscaling? How was the bias-correction implemented?

P6, L134-136: Is this somehow a rationale or justification for why the three aforementioned driving GCMs were chosen? Given your use of JSBACH as your land model, some argument for why none of the MPI-driven runs were utilized, since this is the only GCM/ESM in CMIP5 using the same land model.

P6, Sections 2.2.2-2.2.6: Suggest to move to Supporting Info.

P7, L171-172: At what temporal resolution were the outputs benchmarked? Was surface albedo not benchmarked?

P8, L208-209: “RF_d_abl” should be the radiative forcing at TOA not surface.

P8, L210-212: I don't follow what has been done here and why it has been done. What is the rationale of NOT using the temporally-explicit annual mean RF values here, especially since these were available from JSBACH? A very good argument for why the 45-yr average is used here instead (assumed to be repeated for all 45 time steps?), since the whole benefit of the TDEE metric is its accommodation of a temporally explicit albedo (and d_albedo RF) scenario.

P11, L262-264: I had a difficult time understanding these two sentences and only did so through inference from the subsequently presented results. Consider re-writing for clarity.

P12, Figure 5 caption: Use of the word “change” is confusing here as it often implies a difference between scenarios. I suggest something like “temporal evolution in...”.

P12, L274-278: It is unclear how the LAI results relate to or explain the C-cycle results presented in Figure 5. I'm wondering if showing the breakdown into the various pools (i.e., living biomass, deadwood, litter, soil, etc) is more helpful in interpreting the aggregate trends (of Fig. 5).

P13, L281-283: Again, why are all 45 years of the results time series averaged? Myself and probably many readers would find the interannual albedo dynamics relevant and interesting. You could consider aggregating months into “winter” vs. “summer”, or “snow-covered” vs. “snow-free” to reduce the number of plots.

P14, Figure 7: I feel the breakdown of albedo into its broadband constituents is unnecessary and distracting. Only the entire shortwave broadband albedo is of relevance/interest in this study (i.e., “NIR+VIS”), and most readers would probably rather see this plotted here instead for the three harvest/management scenarios, which would align with the presentation style of the previous results figures.

P16, Figures 9-10: The snow cover results help explain the albedo results and could be moved to the Supporting Info in effort to shorten the manuscript.

P17, Figure 11: Same comment as previous regarding the choice not to show the interannual dynamics. Also, consider harmonizing all difference plots so that differences are consistent across plots. In Figure 8 differences are defined as “delta = XX - EC”, so doing the same for Figure 11 (instead of of “delta = EC – XX” as here) would make it easier for the reader to connect this result to the delta albedo result.

P21, Section 3.3 “Water Balance”: These results are not a major result and can be moved to Supporting Info.

Sections 3.2 – 3.4 and Figures 13-15, 17, Table 3: This content should be updated if choosing to carry out the major revision I argue for above, with climate impacts quantified in terms of TOA ERF (albedo IRF + Adjustments). I have therefore refrained from commenting on the remaining results of these sections.

Discussion: I also refrain from making detailed comments given the likelihood that the content will change should the authors choose to implement the suggested major revisions. I will though make a general comment, however, that the Discussion is missing important elements such as: i) the study’s limitations and/or uncertainties ii) benchmarking of main findings to those of other, similar studies (there have been a lot of similar studies published for Fennoscandian region in the past 10 years); iii) recommendations for future research.

P28, Conclusions: That the EC scenario is “*the most beneficial in terms of climate change mitigation*” is not surprising given that differences in biogeophysical effects were negligible and since forests were allowed to grow – particularly since the analytical time horizon was constrained to 45 years. And while this conclusion may be valid at the ecosystem level (i.e., when the system boundaries are limited to the forest), it may not actually hold when one takes an expanded system perspective that includes the fate of C in the wood that is harvested and exported from the forest as well as the fossil fuel C substitution occurring when the harvested wood displaces more emission-intensive non-wood products. I strongly encourage the authors to bring these elements into the discussion and recommend that they clearly state that their main conclusion is only valid at the ecosystem level.

Bright *et al.* (2025), Biogeophysical radiative forcings of large-scale afforestation in Europe are highly localized and dominated by surface albedo change, *GRL* 52(1):
<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2024GL112739>