Forest and bioenergy expansion amplifies climate warming by accelerating regional cloud loss

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In this manuscript, the authors utilize the fully coupled Community Earth System Model (CESM) to investigate the long-term response of cloud cover to land use and land cover change (LUCC). Specifically, the study compares the impacts of idealized afforestation, realistic afforestation, and bioenergy expansion. The authors conclude that large-scale idealized afforestation and bioenergy expansion accelerate the loss of low- and mid-level clouds while enhancing high-level clouds, thereby amplifying regional warming through positive shortwave cloud radiative forcing. Conversely, they find that realistic afforestation yields a net cooling effect. To elucidate the physical drivers—namely changes in precipitable water, relative humidity, and albedo-driven surface heating—the authors employ an interpretable machine learning framework (SHAP).
While the topic is important overall, I have several major concerns that should be addressed before the paper can be considered for publication.

Major comments

- **LUCC Maps:** In the main text (e.g., Lines 192–197), different land-use simulations are described, but the spatial distribution of the LUCC forcing is relegated to the supplement (Fig. S23). Because the cloud response depends on the specific latitude bands and local background conditions (e.g., Duveiller et al. (2021), Xu et al. (2022)), understanding exactly where the afforestation and bioenergy expansion occur is critical to interpreting the paper's core findings. I strongly recommend moving the LUCC maps to the main text (perhaps as a new Fig. 1) and addressing two main limitations of the current Fig. S23. First, the terminology is inconsistent with the main text (e.g., is "ORI" essentially the "CTL" simulation?). Second, the figure plots absolute mean percentages rather than the difference relative to the control, which makes it visually challenging to identify where the land cover changes actually occur, particularly for the REAL scenario (comparing panels 'a' and 'c'). Crucially, these maps should be replotted as difference maps from CTL using standardized terminology, so the reader can immediately identify the geographic locations and magnitude of the land cover forcing driving the cloud responses.

- Building on the previous comment regarding the LUCC maps, the authors claim in Line 442 that the high Gini index is "...suggesting amplified local land use impacts." Without showing where the LUCC forcing actually occurred relative to these trends, a "local" interpretation remains speculative. To my understanding, highly concentrated cloud trends do not necessarily indicate local forcing; they could equally be driven by non-local effects. More generally, utilizing a fully coupled model like CESM provides an excellent opportunity to account for *both* local and non-local LUCC effects, but this capability is underutilized in the current analysis. To address this, the authors should explicitly map the LUCC forcing areas and physically link them to the cloud trends to clearly differentiate local impacts from large-scale dynamical responses.

- **Ambiguous Trend Reporting:** The discussion of cloud-cover trends (Lines 298–336) is difficult to follow. For example, the use of "respectively" in Line 303 when describing CLDTOT trends is ambiguous, making it unclear how the AF50 and REAL simulations compare with CTL and BE50. Furthermore, expressions like "1.14 times increase in loss rate" and "1.52 times" are confusing. I suggest reporting straightforward percentage changes and/or the actual trend values for each experiment.

- **High-Cloud Response Clarification:** The discussion of the high-cloud response (Lines 326–332) requires significant clarification. The manuscript argues that afforestation amplifies the positive trend in CLDHGH, enhancing climate warming. However, it is unclear which cloud populations contribute to this diagnostic. Line 161 states that the classification is based on cloud-top pressure thresholds. Is the classification based solely on cloud-top pressure/height? If so, deep convective clouds (rooted in the boundary layer) extending aloft could be classified as high clouds alongside thin cirrus. Because deep convection could respond to surface fluxes while cirrus may not do so directly, the physical mechanism linking LUCC to CLDHGH is ambiguous. Please clarify how CESM defines these cloud types and provide targeted mechanistic support for the high-cloud response.

- **Significance and Uncertainty:** A discussion of statistical significance and model uncertainty is largely missing from the core results and should be included to validate the claimed trends.

- **Figure Presentation and Interpretability:**

  - **Figure 1.** The shading representing the uncertainty is almost invisible, and the $\pm$ error bounds listed in the legend are massive (please see my separate major comment regarding the missing discussion on uncertainty). Furthermore, the text for the p-values and R-values is unreadable in several panels (e.g., Fig.

1n). Finally, the caption fails to explain what the different curves represent physically.

– **Figs. 2,3.** The captions are currently quite brief and would greatly benefit from more detail to guide the reader. Please describe the panels more explicitly. For instance, are these long-term trends or Δ values? If they are Δ values, what is the reference run (e.g., CTL)? It would also be helpful to clarify what positive and negative values represent physically.

– **Figure 6.** The application of the Lorenz curve and the Gini coefficient to cloud trends is innovative, but the current presentation lacks physical context and can be misleading. By isolating positive (+) and negative (−) trends into separate panels that are normalized to 100% on the y-axis, the reader cannot tell which trend direction actually dominates the hemisphere. For instance, if a hemisphere is overwhelmingly dominated by cloud loss, a high Gini coefficient calculated for a smaller area showing a positive (+) cloud trend would be physically minor but visually overemphasized as being equally important. I strongly suggest adding a table or text annotations within each panel showing the relative area fraction or the integrated absolute magnitude of the (+) vs. (−) grid cells. Additionally, the monochromatic orange/blue continuous colormaps make it highly challenging to distinguish between the latitudinal bands referenced in the text. I suggest replacing these smooth gradients with a discrete, stepped colormap that clearly delineates distinct zones (e.g., separate color steps for Tropical, Mid-latitude, and Polar regions).

– **Figure 7, SHAP Analysis.** The use of SHAP values needs to be more accessible. Please explain the intuition of the color bar ("Feature value: High vs. Low") in relation to the x-axis (SHAP value) in the text or caption. Furthermore, the inset bar charts in Fig. 7 are currently too small and unreadable. To save space, panels sharing variables (a, b, c) should share a single y-axis and color bar, allowing the insets to be enlarged or moved to the supplement. Finally, please state in the caption if the features on the y-axis are ranked by overall importance.

Specific comments

- **Title:** The title in the preprint contains a typo ("amplifiers") and does not match the corrected title in the online EGUsphere version.

- **Abstract:** The relative numbers provided are unclear. Relative to what baseline?

- **Acronyms & Variables:** The paper has a lot of acronyms, making it difficult to read. I suggest using far fewer acronyms and wherever possible use conventional acronyms some readers might already be familiar with.

- The term ”bioenergy” currently reads a bit like jargon and would benefit from a brief functional explanation. I recommend adding a short definition around Line 93 in the introduction, and perhaps including a few clarifying words in the abstract as well to ensure it is clear to all readers.
- Line 171: The first reference to a Supplementary Information (SI) figure jumps to Figs. S24–S27. Might be better to reorder the SI figures so they appear sequentially as mentioned in the main text.
- Line 180: ”landuse.timeseries” – Typo?
- Line 183: ”FAO” appears here for the first time but is not defined until Line 603. Please ensure it is defined upon its first use in the text.
- Line 230+: Please include units for all variables discussed in the text, including those related to Equation 6.
- Line 301: There appears to be a typo in the error bounds: ”-0.0083 $\pm$ 1037%/year”. Additionally, it would be much more intuitive to present these trends per decade rather than per year.
- Lines 364 & 373: I suggest avoiding subjective phrases like ”climate damage” or ”negative impacts on the climate.” Maintain descriptive, objective scientific language (e.g., ”amplified warming”).
- Lines 493–497: The ”Mesosphere clouds”– is this a terminology error? The authors discuss the loss of ”mesosphere clouds” due to boundary layer changes suppressing moisture supply. The CESM variable CLDHGH represents upper tropospheric clouds. Please clarify.
- Lines 596–597: The phrasing ”the land comes from the land of other bioenergy crops” is awkward and should be rewritten for clarity.

References

Duveiller, G., Filipponi, F., Ceglar, A., Bojanowski, J., Alkama, R., and Cescatti, A. (2021). Revealing the widespread potential of forests to increase low level cloud cover. *Nature Communications*, 12(1):4337.

Xu, R., Li, Y., Teuling, A. J., Zhao, L., Spracklen, D. V., Garcia-Carreras, L., Meier, R., Chen, L., Zheng, Y., Lin, H., et al. (2022). Contrasting impacts of forests on cloud cover based on satellite observations. *Nature Communications*, 13(1):670.