

Dear Reviewer,

we would like to thank you for thoroughly reading our manuscript. We were particularly pleased by your assessment of the question we address as important and timely. Following your suggestions, we will switch to the term “indirect impacts” and revise the title, abstract, and introduction to avoid any ambiguity regarding our use of the term. A shorter and more concise Results and Discussion Section will further contribute to the clarity and readability of the manuscript.

Please find our point-by-point responses below.

Major Comments

1. **The study does not explicitly represent land-atmosphere interactions.** My primary concern is that the manuscript repeatedly attributes its findings to the **interplay of land-atmosphere interactions** (e.g., Lines 20, 24, 161, 228, and 278), whereas the underlying simulations are performed using the offline land surface model LPJmL. An offline land surface model can only respond to prescribed atmospheric forcing and cannot feed land-surface changes back to the atmosphere. Therefore, the two-way land-atmosphere interactions implied throughout the manuscript are not explicitly represented. Consequently, attributing the simulated patterns to the land-atmosphere interactions is misleading. More importantly, this limitation is not merely a matter of wording, but may affect the validity of the manuscript’s central conclusions. For example, land-cover change modifies surface albedo, evapotranspiration, and surface energy fluxes, which in turn alter atmospheric circulation, precipitation, vegetation growth, runoff, and carbon storage through atmospheric teleconnections. Previous studies have even shown that these atmospheric feedbacks can generate remote impacts comparable to, or larger than, the local responses in some regions. Such processes are entirely absent from the current framework. Therefore, I do not believe that this issue can be adequately addressed by the brief discussion of study limitation in Lines 359-360. Instead, the authors should explicitly acknowledge that their analysis only quantifies land-surface responses under prescribed atmospheric conditions and discuss how the omission of land-atmosphere feedbacks may influence their conclusions.

In alignment with Lade et al. 2021, our results are based on a one-way coupling of LPJmL. This offline approach allows us to neatly isolate particular effects without the need to disentangle overlaying feedback effects. We tried to be very clear about this fact both in the Introduction and in the Methods section, visually supported by the conceptual diagram (Figure 2).

Nevertheless, we agree that in case of the other occurrences of the term “interplay/interaction” in l.161, l.228, Appendix L, a change of wording (e.g., to “effects”) will increase clarity. (Note that in l.20 and l.24, we refer to the interplay between different Earth system processes in a more general sense/in literature, in l.278, we refer to the interplay of *effects*.)

Furthermore, we will extend the corresponding paragraph in the Discussion, highlighting potential changes that the inclusion of feedback mechanisms could entail.

2. **The definition and scope of “second-order impacts” require clarification.** The terminology “second-order impact” is introduced very early in the manuscript (Line 20), yet its definition remains ambiguous throughout the paper. First, I am not convinced that this relatively uncommon terminology is preferable to the more widely used term **indirect impacts**, unless the authors intend to distinguish a more specific

concept. Second, the manuscript never clearly defines what processes are included within second-order impact and, equally importantly, which processes are intentionally excluded. For example, based on the modelling framework, atmospheric feedbacks discussed in Comment 1 are clearly beyond the scope of the study. Such boundaries should be explicitly stated when the term is first introduced to avoid confusion.

We chose the term “second-order impacts” to distinguish from higher-order impacts created by feedbacks. However, we are comfortable to change to the more widely used term “indirect impacts”. To avoid ambiguity, we will add the following definition to the first paragraph of the Introduction: “[...] leading to so-called *indirect* impacts. For example, while we consider the decrease in natural vegetation as the *direct* impact of deforestation, the effects of this decrease on carbon storage and water availability are considered *indirect* impacts. Hence, *direct* impacts involve human pressure and one Earth system process, while *indirect* impacts involve human pressure and two Earth system processes. In particular, we refer to impacts as one-way cause-and effect relations, not including feedback mechanisms.”

3. **Justification for the selected indicators.** The authors use **carbon storage density**, **surface runoff**, and **natural vegetation cover** as representative indicators of climate, water, and land impacts, respectively. However, the rationale for selecting these three variables is not sufficiently explained. For example, from a land-surface perspective, variables such as evapotranspiration or soil moisture may more directly characterize hydrological impacts than runoff, while GPP could arguably be a more representative measure of ecosystem functioning than vegetation cover. The manuscript would benefit from explaining the criteria used to select these indicators and discussing whether the major conclusions remain robust when alternative variables are considered.

The selected indicators align closely with variables representing human pressures (e.g., clearing land, extracting water), which becomes particularly important in case the results get implemented in an impact framework like the Earth System Impact Metric (ESI, Lade et al. 2021). We will clarify this rationale in the manuscript.

4. **The Title and Abstract do not adequately reflect the main findings.** The main contribution of this study is not only the investigation of the spatial patterns in second-order impacts, but also the comparison between top-down and bottom-up clustering approaches, with the latter potentially demonstrating superior performance. However, the Abstract devotes relatively little attention to these central findings and instead remains rather general. A similar issue exists in the title, which does not reflect the methodological comparison that forms the core contribution of the study. As a result, even after reading the title and Abstract multiple times, it remains difficult to immediately grasp the primary objectives and conclusions of the manuscript. I recommend revising both sections to better emphasize the novelty and principal findings.

We would like to point out that while the bottom-up approach is intuitively exceeding the top-down approach in clustering performance, it is inferior in explainability and communicability. Hence, while highlighting the limitations of the more established top-down approach, the results of our manuscript suggest to choose a clustering methodology depending on the context and scope of a study.

Nevertheless, we agree that the comparison of the two clustering approaches provides an essential part of our study and hence, we will highlight this aspect more directly in the title and in the abstract.

We suggest “Spatial patterns in indirect effects of human activity on climate, land, and water: comparing top-down and bottom-up clustering approaches” as a new title.

5. **Consistency among different impact estimation methods.** The manuscript adopts different methodologies to estimate the strength of second-order impacts for different components. For example, the effects on natural vegetation cover are quantified using one methodology, whereas climate-related impacts are estimated using another. The authors should clarify whether these different methods produce quantitatively comparable estimates. If the results are intended to be directly compared, the rationale for using different estimation methods should be justified. Alternatively, the authors should discuss the potential biases introduced by methodological inconsistency.
 Our study is based on the modelling framework used in Lade et al. 2021, whose rationale is based on the fact that atmospheric CO₂ is driving the model, while land use change can be switched on and off in LPJmL. Aligning with their methodology allowed for a direct comparison their vegetation-based clustering approach.
 Concerning the limitations of this approach, we mention that the different methodologies might be partly responsible for certain differences in the spatial layout of the two effect types (l.163-164).
 As correctly pointed out, the two estimation methods do not create quantitatively comparable results. Accordingly, we do not compare the absolute size of climate-induced effects and land use change-induced effects. We will clarify this fact in the manuscript.

Minor Comments

1. **Line 52:** I do not consider a spatial resolution of 0.5° to constitute “high spatial resolution”. This description should be moderated. *We will remove that term.*
2. **Line 71:** Please explain why only the last 30 years were selected for the analysis. *The differential quotient approach, in contrast to the linear regression approach, requires states of variables rather than time courses. Following Lade et al. 2021, we use averages over the last 30 years of the simulation as representatives of the recent state under different assumptions regarding human-driven land use change. The choice of 30 years aligns with the established use of 30-year climatological averages and reduces interannual variability.*
3. Both the Results and Discussion are considerably longer than necessary. I recommend reducing repetitive descriptions, removing some regional examples, and emphasizing the overarching conclusions. In addition, the Results should explain why the observed patterns emerge rather than only describing them. For example, in Lines 195-203, why does natural vegetation exert a negative influence on runoff primarily in Asian tropical rainforests? Does this relationship not exist in tropical rainforests elsewhere? *We will follow your suggestion, aiming for a shorter and more concise Results and Discussion Section. This procedure will be facilitated by the inclusion of a table on main mechanisms underlying the positive and negative effect sizes (see Minor Comment 6).*
Concerning the negative effect of land use change on water runoff: as mentioned in l.197-198, four out of the five clusters exhibiting the strongest negative impact are rainforests (Asian, African, North American, Oceanian), hence, this phenomenon is not restricted to the Asian rainforest. In order to avoid such misunderstandings, we will follow your suggestion and focus on the main findings rather than individual examples.
4. Since the bottom-up clustering consistently outperforms the top-down approach in most evaluation metrics, whether the extensive discussion of the top-down results is necessary. The manuscript would benefit from placing greater emphasis on the more successful clustering framework. *As mentioned above, we do not consider the bottom-up clustering approach as superior in all aspects. It outperforms the top-down clustering with respect to typical clustering validity indices, but the resulting clusters are less intuitive and hence, the results are more difficult to communicate. Besides, most existing studies are based on a top-down approach and our study aims to build*

awareness for the caveats that this approach entails, independently of the chosen natural partition. We will emphasize these aspects in the Conclusions.

5. Under the bottom-up approach, different impact pathways (e.g., climate -> vegetation versus vegetation -> water) would likely produce substantially different spatial clusters. If so, how comparable are the resulting cluster systems? This issue deserves further discussion. Yes, based on the maps presented in Figure 4, we expect a univariate clustering to substantially differ depending on the impact pathway defining the feature space. This expectation is further supported by the large differences in clustering performance of top-down partitions depending on the impact pathway (Figure 6). In the manuscript, we use multivariate clustering as a tool to find integrative partitions under these exact circumstances. And still, as Figure K2 demonstrates, there is no “one-size-fits-all”-clustering. We are happy to highlight this fact in the Conclusions.
6. Besides Figure 8, I strongly recommend adding a summary figure or table explaining the dominant mechanisms responsible for the identified high positive and high negative impact clusters. This would substantially improve readability and reduce the need for lengthy textual descriptions. We will include a little table summarizing the main mechanisms leading to positive/negative effect size for the four impact pathways. By placing it at the beginning of Section 3, it will allow for more conciseness throughout the whole Results and Discussion section. Nevertheless, we would like to point out that the underlying mechanisms can depend on the location being considered and hence, further explanations in the text cannot be fully avoided.
7. **Line 393:** Please explain why a decrease in natural vegetation cover leads to **increased evapotranspiration** in these regions. This result appears counterintuitive and requires further explanation. Removing vegetation can lead to a more exposed soil and thereby to an increase in soil evaporation. On the other hand, crop transpiration might be higher than transpiration on formerly largely barren soil, like e.g., in parts of central Australia. We will add these explanations to the manuscript.