First of all, we would like to express our sincere thanks to the editor and the reviewer for their constructive comments. Following the comments and suggestions, we have updated the manuscript accordingly. Our detailed response is provided below, with the editor's comments highlighted in red and our response in blue italics.

Reply to Editor's comment:

... your manuscript has been seen by a third referee now and it requires major revisions before it can be reconsidered for publication. First, I would like to refer back to Referee #1 about the lack of validation. Even if the 'paper is a model description to be used for future reference' the model output still needs to be validated to illustrate that the model has some skill. Furthermore, the manuscript presents an effort to include ecological trait-based vegetation concepts into a land surface model in an Earth system modeling framework relying heavily on some key ecological concepts that are not used precisely as pointed out by Referee 3#. Therefore, the manuscript requires some clarification on the interpretation of results, terminology used in the test, and the conceptual framing.

Thank you very much for highlighting the most important items. We have carefully considered the useful and constructive comments by Referee #3. First, we would like to emphasize that the inclusion of simulation results in this model description manuscript serves primarily to illustrate the feasibility of our method, as explained in our response to Referee #1. Given that our new land-surface model JeDi-BACH is implemented into a preliminary and not yet tuned configuration of ICON-ESM at coarse-resolution (~3° ≈ 320 km), the coupled model is not expected to perform realistic simulations. Hence our simulations currently serve as a proof-of-concept. The simulations in our paper demonstrate that the number of plant growth strategies—representing different levels of diversity—has an impact on the terrestrial climate. Furthermore, we show that, upon a parameter change in a high-diversity simulation, the functional redundancy stabilizes ecosystem function and that the local climate is rather insensitive to the parameter change. These results demonstrate: (i) that with further development JeDi-BACH may become a useful tool to investigate the interaction between functional trait diversity and climate, and (ii) that because of the stabilizing effect of high diversity ecosystems, JeDi-BACH parameters likely cannot be used for tuning, so that tuning must be done by physical process parameters.

These are rather qualitative results for which an in-depth validation to assess the "skill" or realism is not necessarily needed. Our paper focuses on the new implementation of the JeDi model into an Earth system model and on the question of how the coupled model system behaves qualitatively. We draw no conclusions from our simulations on how ecosystems or their interactions with climates behave in reality. We understand that the inclusion of these simulation results implies the risk of viewing the simulations as investigation of coupled ecosystem-climate behavior. To avoid any potential misunderstanding, we have added several clarifying disclaimers throughout the manuscript. Furthermore, to nevertheless address the editor's concerns and facilitate the review process, we have prepared a Supplementary Information document. This supplement includes several additional plots comparing the JeDi-BACH simulation results against benchmarks for a range of key variables.

With best regards,
Pin-Hsin Hu and coauthors.

Reply to Reviewer 3:

We thank the reviewer for the significant amount of time invested in reviewing our paper. We feel that our paper has very much benefited by his/her very careful and insightful remarks.

A point-by-point response follows. Reviewer comments are in black; our response is written in blue italics.

The study addresses an important and timely question: how biodiversity may influence climate. The coupling of JeDi with JSBACH is a valuable and innovative step, and the work clearly has the potential to open new research directions.

We are glad to read this.

However, in several parts of the manuscript the interpretation goes beyond what the model results can support. In particular, statements such as “climate strongly depends on biodiversity” are quite strong and would need to be phrased more cautiously. In the current formulation, the climate–diversity relationship emerges from the specific configuration of JeDi-BACH (e.g., how plant strategies are selected and how ET feeds back to climate), and not necessarily as a general ecological pattern. Making this explicit would help avoid overgeneralization.

By the reviewer’s comment, we now realize that due to our excitement about the simulation results we indeed tended in some places toward overly strong formulations. This reviewer notes this below in particular for lines 988-94, and 1164-1169 — please see our replies there. In addition, in the revised manuscript we changed for this reason lines 12-13, which now read:

> *“… climate depends strongly on the particular composition of vegetation”*

to

> *“local climate varies with the particular composition of vegetation”.*

There are also some issues with the use of ecological terminology. For example, the term “resilience” is used to describe low sensitivity of climate to parameter variation, which differs from the established ecological meaning related to post-disturbance recovery and regime stability.

This comment refers to the reviewer’s remarks below to lines 13, 1044-50, and 1144 concerning the term “resilience”, the remarks to lines 163-165 on our usage of the term “trait”, and to line 292 concerning the term “allometry”. Please see our replies there.

In addition, the manuscript relies exclusively on environmental filtering to determine community composition, whereas in real ecosystems competition, dispersal, disturbance

regimes and historical processes also play key roles. Acknowledging this limitation in the Discussion would strengthen the ecological framing of the results.

This is indeed true — and is one of the most interesting aspects of the JeDi-concepts: As shown in standalone simulations with JeDi (Kleidon and Mooney,2000) and (Pavlick et al., 2013) the global distribution of actual diversity (number of surviving PGSs) looks fairly similar to the observed global distribution of the diversity of vascular plants, even though JeDi has no representation of competition or dispersal, and ignores natural and human disturbances. In Pavlick et al, (2013) it is argued with reference to (Kraft et al., 2008; Swenson and Weiser, 2010; Freschet et al., 2011; Kraft et al., 2011) that at the spatial scales of the simulations, trait selection and trait convergence due to environmental filtering are the dominant processes shaping plant communities. Hence it may be that at the scale considered, "competition, dispersal, disturbance regimes and historical processes" don't play key roles as suggested by the editor and their omission is thus not — as one might think — a deficiency of the modeling concept, but just the appropriate simplification emerging at large scales that makes it possible to model ecosystem behavior at those scales. — Nevertheless, the reviewer is right that we should address this point in the discussion — and we now add the following in the revised manuscript (see line 1176):

"Nevertheless, one should keep in mind that environmental filtering is only one process that shapes the structure of plant communities, in addition also competition, dispersal and natural and human disturbances play a role. On the other hand, there are indications that at the large spatial scales considered in a global model like ours (grid cells up to 320 km wide), trait selection and trait convergence due to environmental filtering are the dominant processes (Kraft et al., 2008; Swenson and Weiser, 2010; Freschet et al., 2011; Kraft et al., 2011). And indeed, Kleidon and Mooney (2000) and Pavlick et al. (2013) demonstrated in offline simulations with JeDi that the global distribution of actual diversity (number of surviving PGSs) compares well to the observed global distribution of the diversity of vascular plants, even though JeDi invokes only environmental filtering and ignores other processes that might shape plant communities."

Finally, in some sections the presentation mixes results and interpretation, which makes the logical flow a bit difficult to follow. Separating more clearly what the model shows from how those results are interpreted would improve readability and help highlight the main contributions.

When describing models and their results it is indeed always a risk to get imprecise concerning the realms of real processes and their representation, because the latter is meant to stand for the former. We hoped to have largely avoided making that blunder. We carefully scanned the paper for text passages with such categorical mistakes. We have carefully reviewed the manuscript and have made the following changes to ensure the distinction is clear:

L. 903: "Another difference concerns ..." → "Another difference between low and high diversity simulations concerns ...".
L. 913-14: "This similarity again implies that environmental filtering generally results in similar diversity patterns and gradients worldwide, ..." → "This similarity again implies

that in our model world environmental filtering generally results in similar diversity patterns and gradients worldwide, ..."
L. 977: "explains the observed convergence ecosystem functions at high potential diversity" → "explains the convergence of ecosystem function found in our simulations"

L. 985: "Accordingly, one can expect that regional climates converge with increasing potential diversity– and thus also global climate." → "Accordingly, one can expect that in our simulations not only regional climates converge with increasing potential diversity, but also global climate"

L1162: "...also in that region climate would be resilient to parameter changes." → "also in that region climate would be insensitive to parameter changes. "

1. l. 7: The term "novelty of JeDi" may be confusing, since JeDi itself is not new and its framework on incorporating diversity of traits in a DGVM was described 12 years ago (PAvlick et al., 2013). The novelty is the JeDi-BACH coupling. It may help to clarify that in the abstract.

   Yes, this may be easily misunderstood, indeed: Our use of the term 'novelty' was intended to contrast the JeDi approach with the classic Plant Functional Type (PFT) approach, as JeDi allows successful strategies to emerge dynamically rather than being prescribed. Therefore we changed:
   "The novelty of JeDi is ..." → "The key novelty introduced by the implementation of JeDi into JSBACH is ..."

2. l. 13: The manuscript uses the term "resilience" to describe the weak sensitivity of climate to vegetation parameter changes. I think the term "resilience" might be misleading here. What is being evaluated is not resilience in the ecological sense (capacity to absorb disturbance and maintain function), but rather the degree of determinism / sensitivity of climate to vegetation parameter changes in the model. It may help to rephrase this as "low climate sensitivity to vegetation parameters" or "robustness of climate to vegetation parameter variation" to avoid confusion with the established ecological meaning of resilience.

   l. 1044-1050: Again, I think the use of the term "resilience" is problematic here. The results show a compensation mechanism (reorganization of strategies abundance/survinving), which keeps functioning stable, but this isn't true ecological resilience, as it lacks demonstration of recovery post-disturbance. Furthermore, the idea of climate resilience emerging from diversity is a very strong claim and might not be supported. So I suggest avoiding the expression "climate resilience", because it may be interpreted as a causal claim that is not supported.

   l. 1144: I suggest changing "less resilient" to "more sensible". I also suggest along all the text try to replace the word "resilience"

We thank the reviewer for this careful clarification regarding the ecological definition of 'resilience.' We agree that our study evaluates the sensitivity of climate to vegetation parameter changes rather than the rate of recovery post-disturbance. To align with established ecological terminology and avoid misleading claims, we have replaced 'resilience' with more appropriate terms such as 'robustness' or 'low sensitivity' throughout the manuscript.

Specifically:

In L13, we now refer to the robustness of climate to vegetation parameter variation.

L14, we have rephrased to '... while at high diversity terrestrial climate proves to be rather insensitive due to a dynamic re-organization of the plant community structure. '

In L1044-1050 and elsewhere, we have rephrased the discussion to focus on the 'compensation mechanism' provided by the reorganization of strategy, removing the term 'climate resilience.' We will also change 'less resilient' to 'more sensitive' to reflect the degree of climate response to parameter changes.

In lines 1049-1050, we addressed the possible future usefulness of JeDi-BACH as a tool to investigate "... the links between the biodiversity-resilience relationship and climate interactions." Here the formulation "biodiversity-resilience relationship" might be taken as if we would claim a relationship between biodiversity and climate resilience. Instead we intended to refer here to the widely accepted relation between biodiversity and resilience of ecosystem function. To prevent this misunderstanding we have reformulated that part of the sentence to "... the potential links between biodiversity, ecosystem resilience, and interactions with climate".

We rephrase line 1144 from ".. the community at this location is much less resilient than in the Congo" to ".. the community at this location is much more sensitive to the parameter change than in the Congo."

3. l. 22: I think it would be important to clarify that biodiversity influences climate indirectly (e.g., through effects on carbon cycling and land–atmosphere exchanges), as highlighted in Díaz et al. (2007).

   Thank you for the suggestion. We will modify L.22 to: "However, the indirect influence of biodiversity on climate via carbon cycling and vegetation-climate interactions is also expected to exist (Begon et al. 1999, p. 917; Díaz et al. 2007)."

4. l. 33-36: "One strategy is to prescribe the parameter values of the PFTs from static global maps inheriting observed correlations between trait values. Another strategy is to make the parameters values of the PFTs flexible by accounting for statistical correlations among traits or between traits and environmental resources (temperature, moisture, nutrients, . . .) thereby equipping the PFTs with some "plasticity"." The model's references are necessary here. Which models use this approach?

In this text passage we follow the mentioned review by Berzaghi et al. (2020), containing extensive references. So instead of listing all these references it seems more appropriate to devise the reader to that review. So in the revised manuscript we have now added the remark: "For references to implementations of these approaches, see (Berzaghi et al., 2020, Table 1)."

5. l. 96 and l. 112: The citation to Rius et al. (2023) may need clarification. That paper does not implement JeDi itself, but uses a different vegetation model (CAETÊ) in which the plant strategies are inspired by the PGS concept. It might be more precise to specify that Rius et al. (2023) applies the JeDi rationale for representing trait-based strategy diversity, rather than a JeDi implementation.

   We classify the CAETÊ model of Rius et al. (2023) within the JeDi model series because it employs the JeDi modeling concept to achieve environmental filtering via functional trade-offs (see Section 2.1 of the manuscript for the definition of the JeDi modeling approach). But indeed, our original sentence may have implied that CAETÊ is merely another replicate of the JeDi-DGVM (Pavlick et al., 2013). So in the resubmitted manuscript we rephrase line 96 from "Existing implementations of JeDi (Kleidon and Mooney, 2000; Pavlick et al., 2013; Rius et al., 2023) are designed as full land models ..." to: "Existing implementations based on the JeDi concept (Kleidon and Mooney, 2000; Pavlick et al., 2013) or parts of it (Rius et al., 2023) are designed as full land models ..."

6. Figure 1: I found that the wavy borders around the component boxes make the visual structure slightly harder to interpret. A simpler box style could help emphasize the flows and the organization of the carbon pools more clearly.

   Figure 1 illustrates the flow of biomass carbon pools within the JeDi modeling framework. The decorative borders of the pool components are purely aesthetic and do not impact the interpretation of the carbon dynamics. Consequently, we have opted to retain the current design.

7. l. 163-165: The parameters t1–t4 are described as functional traits, but they look more like model control parameters regulating phenology and growth responses (e.g., thresholds and response times) rather than measurable plant traits in the classical functional trait sense. It might help to clarify that these are internal model parameters rather than traits directly linked to observed plant functional strategies. Otherwise, there is a risk of overinterpreting the biological meaning of these axes.

   Indeed, in a strict sense t1-t15 are model parameters, and — as noted a few lines earlier in the manuscript — only part of them refer to measurable properties of plants. The reason they have been called "traits" and not "parameters" — and we follow here the literature — is that they are not parameters in the traditional sense, because each of those t1-t15 represents for each PGS a different value, so that each of them may represent thousands of numbers. This feature makes those t1-t15 similar to traits. When writing the paper, we were aware of this, and introduced t1-t15 as "trait parameters". Only in order to shorten the wording we used — as explained in lines 159-160 — the

word “trait” instead. But maybe this was not such a good idea. Accordingly, whenever we refer to one of the t1-t15, we now denote them as “trait parameter” in the revised manuscript.

8. l. 178-185: The text uses phrases such as “the tree prefers to grow leaves rather than roots”. I would suggest rephrasing this, as plants do not actively “prefer” one allocation strategy over another. Using “prefer” may unintentionally imply agency. These are inherited functional strategies that emerge from evolutionary trade-offs and environmental filtering. Using terms such as “invests more carbon into leaves” or “follows a canopy-focused allocation strategy” would reflect this more accurately.

   Thank you for highlighting this subtle difference in our wording. We have revised lines 178–180 accordingly in the resubmitted version:

   “To illustrate this concept, imagine two types of trees with different allocation strategies: one invests more carbon in leaves than in roots; the other invests more in roots than in leaves. …”

9. l. 196: I suggest clarifying what is meant by “functional capabilities”.

   In the JeDi approach, all Plant Growth Strategies (PGSs) share an identical set of morpho-ecophysiological functions—such as photosynthesis, carbon allocation, and soil-water accessibility—and are defined by the same set of trait parameters. However, the specific values assigned to these parameters determine the capability of a strategy to perform these functions as explained in lines 199-200. Accordingly, we use the term 'functional capability' to clarify the relationship between trait parameter values and the resulting performance levels of each strategy. But this usage of the word “capability” is unfortunately introduced in lines 199-200 after its first usage in line 196. So, in our revised manuscript we replaced the formulation “... based on their inherent functional capabilities ...” (line 196) by “... based on their inherent functional capabilities defined by the values of the trait parameters ...”.

10. l. 186-189: The statement that harsher or more variable environments necessarily result in lower species richness may be too general. While this pattern can hold in extreme environments where physiological tolerance is the main filter (e.g., cold deserts, saline systems), many ecosystems with strong climatic variability (e.g., savannas, seasonally dry tropical forests, Cerrado) sustain high functional and taxonomic diversity(. Richness patterns are influenced not only by environmental filtering but also by competition, dispersal, disturbance regimes and historical factors. It may be useful to soften or qualify this sentence.

    Thank you for pointing this out. Upon re-reading, we find that this paragraph does not add to our explanation of environmental filtering; therefore, we have removed it from the resubmitted manuscript.

11. l. 256: What do you mean with “in the long run”?

    We notice that the wording is not precise. So for the revised manuscript we rephrased Ln 256 from “A PGS is alive only if it can maintain positive carbon storage in the long run” to “A PGS is alive as long as it maintains a positive carbon storage”.

12. l. 292: The term “allometry” may be slightly misleading on this section. Here, the model uses fixed carbon allocation rules among tissues, whereas in ecology allometry usually refers to empirically derived scaling relationships among organ sizes or biomass compartments (e.g., mass-based scaling laws). It may help to clarify that what is implemented is an allocation scheme rather than allometric scaling in the classical sense.

    We agree that the title of the section (“Plant allometry”) is not really up to the point. Following the suggestion of the reviewer we have modified the title; in the revised manuscript it now reads “Carbon allocation”. Otherwise, we think that throughout the manuscript we use the term “allometry” correctly in the sense of its primary definition given in Webster’s Encyclopedic Unabridged Dictionary (Gramercy Books, 1996): “Biol. 1. Growth of a part of an organism in relation to the growth of the whole organism or some parts of it”. This is exactly how the JeDi allocation scheme is constructed: carbon allocation is made such that the organs grow in a fixed relationship to the whole “plant”. Accordingly, we only changed the section title.

13. l. 806: I suggest also stating the resolution in degrees.

    We have speicfied the approximate horizontal resolution (∼3° ≈320 km) in the revised manuscript.

14. l. 839: The “cold start” needs more clarification.

    The “cold start” has been shortly explained a few lines before (lines 834-35) by noting that “... for a "cold start" the initial state is chosen largely ad hoc by using an initial state that is technically easily realizable, e.g., by using a worldwide equally filled soil water reservoir.”. Following the reviewer’s suggestion, we expanded this a bit by writing instead: “... for a "cold start" the initial state is chosen largely ad hoc by using an initial state that is technically easily realizable, in our case we use an equally filled soil water reservoir and an unvegetated Earth (no biomass carbon).”

15. l. 869: I suggest changing "not known” by “no well explored and understood”

    We will rephrase Ln 869 to “That biodiversity affects climate is expected (see e.g. (Begon et al., 1999, p. 917), (Díaz et al., 2007)) but not well explored. ”

16. l. 878: "..trees survive environmental filtering less often than grasses.”. Why does it happen?

    Compared to tree PGSs, grass PGSs do not have woody tissues so that more carbon is available for leaves and roots. They are likely more productive during the early growth

phase and refill their storage pool. Consequently, we suspect that they have a higher likelihood of surviving during the development phase compared to tree PGSs.

17. l. 880: I suggest improving the description on how Latin-hypercupe works and how the traits are sampled from their original range of values

    Unfortunately, we referred here to the wrong section. The generation of random trait values is explained in detail in section 2.1.3. We corrected the reference in the revised manuscript.

18. Figure 5: The pattern in Figure 5 suggests that, when potential diversity is high (N = 600), most of the surviving strategies occur in warm and wet climates. This indicates that the model tends to favor water-acquisitive, high-transpiration strategies under interactive land–atmosphere coupling. The resulting feedback (higher ET → cooler and wetter climate → persistence of acquisitive strategies) may reinforce wet tropical states. It would be helpful to acknowledge that this convergence emerges from the way plant water-use and climate coupling are represented in the model, and does not necessarily imply that high diversity generally promotes humid climates in real ecosystems.

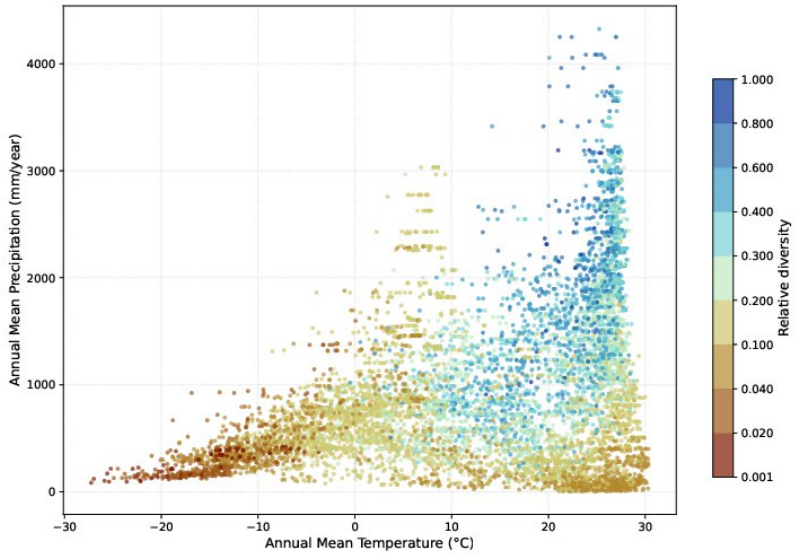

Figure 1 Global plant diversity patterns in climate domain. Shown is relative species richness normalized by the maximum richness in the dataset published by (Cai et al., 2023), who used a machine-learning algorithm to predict species richness from observation data. Climate domain is represented by annual mean precipitation from the GPCP dataset (Adler et al. 2018) and annual mean temperature from CRU dataset (Harris et al. 2014).

We unfortunetly cannot follow the reviewer here: In Fig. 5, we do not see that in the simulation with high potential diversity (N=600) the relative diversity is higher in warm and wet climates than in the simulation at low potential diversity (N=10). Actually, in the wet and warm climates, bluish dots dominate in both figures, but for N=10 the blue color is darker than for N=600, so that obviously relative diversity is in these regions higher than in the high diversity simulation. Accordingly, the model behaviour is just opposite of what the reviewer remarks. That in both simulations relative diversity is higher in wet and

warm climates is, in fact, a rather good sign that the model behaves as it should, because also in reality diversity is larger in wet and warm climates — for comparison, we include here a figure of the global diversity pattern projected on the climate domain (Figure 1) from a machine-learning estimate based on observational data of biodiversity (Cai et al., 2023). Hence, we do not think that Fig. 5 shows artificial behaviour emerging from the 'particular way plant water-use and climate coupling are represented in the model'; instead, it is a rather strong indication that the model behaves correctly.

Nevertheless, we want to add two further remarks: First, the number of surviving PGSs is rather low in the N=10 simulation so that we cannot be sure that statistical differences between the two cases N=10 and N=600 in Fig. 5 are significant. Hence, we do not try to interpret differences between the two figures, the only conclusion that we draw in the text from these figures is that in climate space the diversity distribution is in both cases somewhat realistic. Second, we would like to clarify a potential misunderstanding regarding the direction of causality. Our model does not suggest that biodiversity 'promotes' warm and wet climates. Rather, while the feedback effect of biodiversity on climate is relatively small, the impact of climate on biodiversity is substantial. This relationship—where climate acts as the primary driver of diversity patterns—is clearly reflected in our results (see Fig. 5) Accordingly, our model has no tendency to "reinforce wet tropical states" as the reviewer suggests. In fact, the mechanism leading in our experiment to wetter and cooler conditions at high diversity is operating in all regions worldwide (and thus also in hot and dry regions) as demonstrated by our analysis in Figs. 9c and 9d. ¶

l. 985: The sentence "Accordingly, one can expect that regional climates converge with increasing potential diversity – and thus also global climate" sounds like a general principle. I understand that this convergence happens within the model, given your assumptions. However, saying "one can expect" gives the impression that this should also happen in nature. In reality, species/strategies are not determined only by environmental filtering; competition, dispersal limitations, history and disturbance also play important roles in shaping community composition. Also, climate in many regions is strongly influenced by ocean–atmosphere circulation and topography, not only vegetation. So I suggest rephrasing to make clear that the climate convergence shown here is specific to the model framework, rather than a general ecological rule.

We are afraid that this could be a misunderstanding: The whole paragraph in which this sentence appears is not meant to make general claims, but to function as a bridge passage between the first topic in that section (convergence in ecological function), and the second topic (convergence in regional and global climate). And because "vegetation operates in close interaction with the surrounding physical environment" (line 985) one can expect from the convergence in ecological function also a convergence in regional and global climate, as expressed in the incriminated sentence (lines 986-87). We did not explicitly say here that this is an expectation for what to be found in simulations (instead of in general), but this should be obvious from the next sentence (lines 986-87) by which the next paragraph starts: "And indeed, this is what we see in the simulations" (line 988). Nevertheless, to definitely prevent such a misunderstanding we have extended that

sentence, so that in the revised manuscript it now reads "Accordingly, one can expect that in our simulations not only regional climates converge with increasing potential diversity, but also global climate".

19. l. 988-994: This result needs to be discussed more carefully. For example, in drier regions, strategies with lower transpiration are not "suboptimal" — they are adaptive and necessary to avoid hydraulic failure. In these systems, higher diversity often means more variation in conservative water-use strategies, not an increase in ET. So in such cases we would not expect vegetation to shift the climate toward cooler and wetter conditions. This makes me think that the convergence seen here is more related to how the model links diversity to transpiration, and not something that would generally be expected in real ecosystems. I suggest adding a short discussion on this point, because otherwise the reader may interpret the convergence as a broader ecological rule.

    Our paper is a description of our newly implemented JeDi-BACH model and how it behaves as part of the ICON-ESM. Throughout, we do not intend to make any general claims. Nevertheless, the model shows interesting results that justify — as hoped — its tedious development as a future tool to investigate the relation between plant functional diversity and climate, and more specifically how diversity may affect climate. The results are interesting in the sense of suggesting hypothetical relations between the two — and more then suggesting hypothesis cannot be expected from a model like ours, where, as in global vegetation models in general, the representation of processes is largely under-complex. Accordingly, the whole paragraph to which the reviewer is referring here, is exclusively describing model behavior, and it is not meant as a claim on general behavior of ecosystems in relation to climate. But indeed, the model behavior seen in these exploratory simulations is interesting insofar as it may be understood as an interesting hypothesis on such behavior in nature — but going that far it would need a need a different paper. So, the only thing that these simulation results say at the moment, is that the model may be an interesting tool to study such processes. Overall we are a bit surprised that the simulations presented in this model description are taken as a study of the phenomena they show — if so one would not present them in a model description paper.
    To prevent such a misinterpretation, we added in the revised document the following sentences to that paragraph:
    "But one should be careful in order not to over-interpret this result: We have not fully analyzed this, but this tendency towards a wetter an colder climate at high diversity is a consequence of the biomass scaling. PGSs that are more successful in the accumulation of biomass are typically also more productive and have a higher rate of transpiration. By the biomass scaling these PGSs contribute relatively more to the grid-cell wide evapotranspiration, so that there is a tendency to maximize evapotranspiration. While this is not an unplausible behaviour, such behavior may not apply in general everywhere, as survival strategies that minimize transpiration may be more successful (e.g., in dry climates). So at the moment one should take our simulation results only as a demonstration that the model might be an interesting tool to study how biodiversity may affect climate."

20. l. 995-997: There two " : ” in the sentence

    We have updated the text accordingly.

21. l. 1035-1037: The text is bt confuse here. The first sentence says the focus is not on vegetation characteristics, but then the second says you varied vegetation parameters to test sensitivity. I assume what you mean is that the goal is not to tune vegetation to match observations, but to see how much changing vegetation parameters affects the climate. If this is the case, maybe rephrasing this part could avoid confusion.

    We have modified line 1035 to “In this chapter, we focus on how sensitive is terrestrial climate to vegetation parameters.”

22. l. 1059-1061: What is the possible implication of the use of a common spin-up and the likelihood of the death of the “... strategies that died in this spin-up could be able to survive in the sensitivity simulations if a separate spin-up would have been performed with the changed parameter”?

    With this remark we wanted to point out that by a cold start the surviving strategies are adapted to the set of parameters used for this cold start simulation and that this may have consequences for the obtained sensitivity: Performing the cold start simulation with a different set of parameters other PGSs may survive that are adapted to this different set of parameters. Hence, when performing cold start simulations separately for each parameter set considered for testing the sensitivity, this sensitivity could be slightly different from that we find by changing the parameters after having already performed the spinup with a certain set of parameters. But our experience with the model is that spinups performed from a cold start using slightly different parameters at higher potential diversity have only a negligible effect on the final diversity patterns (relative diversity). So instead of these lengthy explanation of a possible minor effect on sensitivity, we omit that remark in the revised manuscript.

23. l. 1065: I was not completely sure why only the last 15 years were used for the analysis. Since climate averages are typically based on ~30 years to smooth variability, it would help to explain why 15 years was chosen here.

    Because every single simulation takes so much time, we could only simulate 30 years. And since we expect because of the abrupt parameter change large perturbation to ecosystems during the first, say, 15 years of the simulation, we had only the remaining 15 years left for analysis.

24. Section 5.2: I'm a bit unsure about the use of the Mann-Whitney test for comparing the control and experiment simulations. In some regions the mean precipitation differs by ~200–300 mm/year, and temperature by more than 1°C (Fig. 12, 13 and 14), which are not small differences from a climatological point of view. However, the test indicates that these are “not significant”. So I wasn’t sure if this reflects a limitation of the test (and the short 15-year window, with high interannual variability reducing statistical power) or if the

intention is to say that these differences are not considered meaningful in the context of the model. At least qualitatively, a change of this magnitude seems important.

Even if the absolute difference is large, it may be small compared to the variations, in which case the difference is not significant- this is what the Whitney-Mann test —which assesses whether two distributions are stochastically different— is essentially testing for. The "signal" emerged due to the parameter change does not emerge clearly from the "noise" of interannual variability over this period. To verify the robustness of our results, we did a similar analysis using Student t-test for precipitation (Figure 2). The resulting patterns of significance were spatially consistent with those produced by the Mann-

Whitney test.

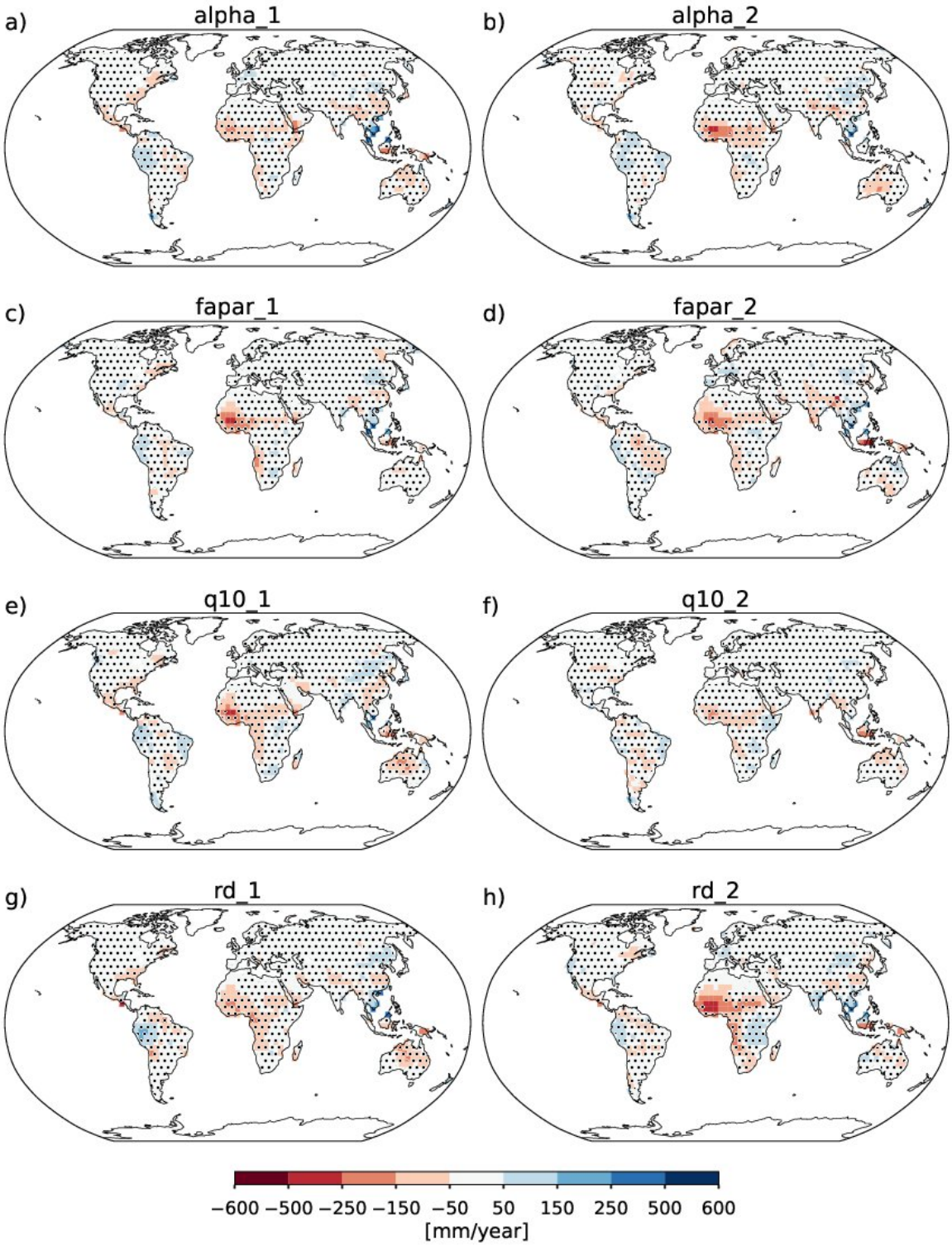

Figure 2 Difference of annual mean precipitation (mm/year) between each sensitivity simulation and CTRL simulation. In grid cells marked with a dot the difference to the control simulation is statistically not significant according to a 5% level in a Student t-test. Ocean surfaces and Antarctica are excluded from the analysis as they were prescribed with the same forcing data in all simulations.

25. l. 1164-1169: I see the conceptual link being made with Walker's redundancy hypothesis, but I am not sure the extension to climate stabilization is justified. Walker's idea refers to the maintenance of ecosystem functioning through functional redundancy. Here, the stabilization of climate appears to come from the model's fast internal reorganization of plant strategies, while large-scale physical drivers of climate (ocean–atmosphere circulation, teleconnections, etc.) are held constant. So the stabilization seems to be a property of the model structure rather than a general ecological mechanism. I suggest phrasing this more cautiously to avoid implying that functional redundancy stabilizes climate in real systems.

Upon re-reading the section, we agree that the paragraph should be removed, though for a slightly different reason: Based on our simulation results, we suggested this extension for the model behavior upon parameter changes. But it is hard to defend that the ecosystem disturbances addressed by Walker's hypothesis may be viewed as parameter changes. Accordingly, we now think that we inappropriately mixed model world and real world. To keep the manuscript focused and accurate, we have completely removed this paragraph from the revised version.

26. l. 1188-1189: I assume the mechanism to be that higher potential diversity increases the chance that strategies with high water-use efficiency dominate, which increases ET, leading to cooler and wetter conditions in the coupled model. However, this explanation is not stated very explicitly in the manuscript. Since this is a central result, it might help to describe this mechanism more directly in one place, so the reader does not need to reconstruct it from different parts of the text.

Thank you for the remark. We now realize that we have indeed described only part of the mechanism that leads to a wetter and cooler climate; in particular, we left out how the enhanced evapotranspiration occurs—the mechanism is likely somewhat different from what the reviewer interpreted from our text. Therefore, we included in the revised manuscript some remarks on the possible mechanism causing the enhanced evapotranspiration that leads to a wetter and cooler climate. These remarks can be found as part of our revision in response to point 19 of the reviewer's comments (see above).

27. I also wanted to comment on the mechanism proposed to explain why higher diversity leads to cooler and wetter climates. In the model, "better adapted" strategies seem to be associated with higher stomatal openness and higher ET. However, in many real climates (especially dry or seasonally dry systems), the strategies that are best adapted are actually the more conservative ones (reduced stomatal conductance, tighter hydraulic control). So higher adaptation does not necessarily imply higher ET. I am not completely sure whether this affects the interpretation here, but maybe clarifying that this mechanism is specific to the way plant water-use is represented in the model would avoid giving the impression that this relationship holds universally in nature.

Thank you for this thoughtful comment, it is indeed our fault not to have addressed the possible mechanisms behind the increased evapotranspiration. We don't think that it has much to do with stomatal conductance or enhanced water use efficiency, instead we think, as explained in our previous reply, that it is a result of biomass scaling in combination with positive correlations between NPP (i.e., carbon allocation), biomass (more NPP, so more biomass), and transpiration (more NPP, so more transpiration). As noted in the reply above, we have added that explanation to the revised manuscript.

28. Discussion: it would be important to address the implications of the fact that community composition and diversity in the model emerge exclusively from environmental filtering. In real ecosystems, other processes such as dispersal, competition, biotic interactions, disturbance regimes, and evolutionary history also play key roles in shaping plant communities. Clarifying this would help avoid interpreting the modeled patterns as general ecological outcomes.

The reviewer's comment regarding the omission of important ecological processes was also noted in the general comments on the manuscript. As stated in our response there, we have—as suggested—added a discussion of these points to the revised manuscript.

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