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. 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.

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. 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.

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.

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.

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. 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).

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?

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.

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.

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.

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.

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

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.

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

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.

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

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

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

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

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

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.

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.

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.

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

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.

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. 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”?

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.

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.

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

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.

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.
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.

<u>Discussion:</u> 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.