

Interactive comment on “Harmonizing plant functional type distributions for evaluating Earth System Models” by Anne Dallmeyer et al.

Anonymous Referee #2

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General comments

Dallmeyer et al present a new method to convert plant functional type (PFT) distributions simulated by earth system models into biomes (“biomisation”) that can be more readily compared with other earth system and biome models (of varying complexity and number of PFTs) and with paleo-reconstructions of past vegetation distributions that are typically classified at the biome level. The authors highlight the difficulties of classifying plant species into wider groups, and the challenges posed when comparing different types of classifications (such as PFTs and biomes). However, I have two main concerns with the study that could potentially be alleviated with greater clarity in the text and/or with some restructuring and reframing of the paper. These are detailed more below and in the specific comments section.

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The introduction explains clearly why a model-generic PFT-based biomisation is needed, as opposed to other model-specific methods that have been used in the past. However, it is not obvious to me from the text why this new method that the authors are proposing is needed beyond the “classical approach” they compare to, and therefore how much this study is a novel contribution to the literature. All methods rely on subjective decisions as to how to classify vegetation into biomes, therefore adding another may only contribute to uncertainty in this area. This is particularly the case given that both the PFT-based and classical methods compare reasonably well; therefore, I am left wondering why the classical approach cannot be used and why a new method is needed? As the authors state in lines 338-340 “In general, the skill in representing the individual mega-biomes is similar for the PFT- and the climate-based method. Both approaches have the same strengths and weaknesses, but the spread between the models is larger for the PFT-based biomisations.” And in lines 346-347 “Overall, the metrics indicate that the PFT-based method works similarly well as the classical approach of biomising climate states.” What then is the specific value of this new PFT-based biomisation method? I perhaps have misunderstood (but therefore likely some other readers will too) – so more clarity is needed on this in the introduction. Biomisation itself should be defined in the introduction, and the classical approach needs to be defined there, and not at the end of Section 2.1, and a detailed explanation should be provided as to why we cannot use this method and instead need the new PFT-based method.

It is clear the authors have thought in depth about performing a rigorous analysis and the caveats of their analysis are well detailed in the discussion. The analysis of the differences between models and comparison of different mega-biomes (as shown in Fig. 6 for example) is interesting and useful for the modeling community, particularly given the authors are testing whether the number of PFTs, model complexity, grid cell resolution, or simulation land use contribute to these differences. However, this is not a stated objective or important outcome of the paper. I understand the authors do not want to focus on a model comparison because the same climate data were not used to

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drive the models, and therefore the authors have chosen to focus on the introduction of the new PFT-based biomisation method. However, I am not sure this is a strong enough focus to sell the paper given the PFT-based and classical biomization methods produce similar results, as I discussed above. Furthermore, much of the text reads as a model comparison, thus causing confusion as to the objectives of the paper. And the authors claim this is a new powerful method that is needed in order to more readily compare models (as well as models with reconstructions), but then say this study cannot be a model comparison because they were not forced with the same climate states. It seems perhaps redundant to introduce a new method for a particular purpose and then not use it for that purpose. Clearly there is a lot of useful analysis and discussion in this paper, but I think it would be more useful if it were to be re-framed and re-structured with different objectives.

Finally, the manuscript is somewhat dense in places, with many figures to digest, likely owing to the difficulty of describing complex and detailed analyses. The reader would likely benefit if the authors could spend some time editing the text to make it more concise.

Specific comments

Introduction Lines 45-50: It might be useful if the authors detail the differences between diagnostic biome models forced with GCM-derived climate data vs DGVMs coupled to GCMs? Similarly, it might be useful to detail the difference between PFT and biome classifications. Examples could help with this. And finally, it might be useful in line 50 to clarify again that simulations from the DGVMs have been disregarded because PFT classifications are different to biome classifications.

Methods

Section 2.1 Tables 2 and 3 and for all bioclimatic thresholds (lines 99-111): Is there a reference for the bioclimatic limits used here? Why have these bioclimatic limits been used? Is there evidence to suggest these are better descriptors of bioclimatic limits

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than used in other studies?

Lines 115-119: Please could the authors give more detail here? Perhaps they could provide more detail as to what they mean by “biomise the underlying climate”. An illustrative example would be helpful.

Section 2.2 It would be interesting to denote which simulations in Table 4 are run with dynamic vs static vegetation. Lines 154-156: Please could the authors briefly explain what is the difference between a gap model and a tiling approach. This will aid a reader who is less familiar with these models. Further, please could they describe how the PFT distribution has been determined via the NPP of the vegetation categories for SEIB and why the vegetation categories do not already correspond to the PFT distribution?

Section 2.3 Line 175: Please could the authors briefly explain how Haxeltine and Prentice (1996) derived their vegetation compilation and what it includes?

Line 180: I do not understand this “on a basis of 5°C being higher than 900°C derived from modern observations”. Please could the authors explain what they mean here? Does this correspond with Table 3? If so, it might be useful to say that.

Line 203: “biomisation of simulated climate states (i.e. the classical method)”. This is the first time the classical method has been referenced. It might be useful to the reader to describe it earlier and to detail how the authors’ method differs from this classical method – particularly as it is used to compare the new method with throughout the results section. Otherwise, it appears somewhat out of the blue here.

Results

Section 3.1 It might be more constructive to compare the PFT- and classical climate-based biomisation methods for each model side by side in one figure, and not the former in Figure 3 and latter in Figure 4, given the authors compare the two. It is hard to compare each model given they are on separate pages.

Line 287: It would be instructive to reference that “better represented” means in com-

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parison to observations that are provided in Figure 2.

Discussion

Section 4.1 is well described and an honest and comprehensive view of the caveats of the methods.

Technical corrections Ni et al. (2010) missing most of the reference Dallmeyer et al (2017) reference missing → check all references in the bibliography Line 97: additionally → additional Line 147: has → have Line 148: what is the importance of knowing that the simulations have been re-done on a new computer? Will they be better as a result? Line 159: extra → specifically Line 168: “than the other simulations...” It would be helpful to add “that were run with dynamic/interactive vegetation”. Line 187: details → detail Line 399: works → work Line 420: “very rudimentary” → “in a very rudimentary manner” Line 521: coverage → cover

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2018-41>, 2018.