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Climate of the Past Discussions

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

Anonymous Referee #1

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This manuscript describes and evaluates a new method for converting the output of earth system models (ESMs) into a description of vegetation cover that may be used to evaluate the performance of individual models, and to quantitatively compare a result among models that integrates several aspects of the climate system into a single variable. Vegetation model output has been used as a quick evaluation tool of climate and fully coupled earth system model output for decades, as the global distribution of vegetation is influenced by a range of climate parameters, e.g. temperature and precipitation, and the seasonal characteristics of climate. A global map of biomes produced with climate model output is readily qualitatively compared to observations, e.g., in the form of a map of potential natural vegetation, and may also be directly compared with paleoecological reconstructions, without the need, for example, of converting pollen as-

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semblages, which reflect vegetation cover, to an estimate of climate conditions, which requires a transfer function or inverse modeling approach. As the authors note, most modern earth system models contain a dynamic, i.e., fully coupled, representation of vegetation cover. This provides the opportunity of using the ESM output of vegetation cover directly rather than translating modeled climate into vegetation cover using an offline vegetation model (e.g., the widely applied BIOME4 equilibrium global vegetation model).

Generally, this manuscript is well written, if rather long and tedious to read, and it describes a thorough study that will make a valuable contribution to the field. Ultimately, the scheme applied in the manuscript may become a standard methodology for intercomparison among ESMs, and for evaluation in the light of observations and paleoecological reconstructions. However, given that the authors emphasize that the manuscript describes a method rather than focusing on results, arguably the manuscript is not really suitable for *Climate of the Past* and should be published in a methods journal such as *Geoscientific Model Development*. The method described here would certainly not be limited to paleo-environmental applications, and could be widely applied, e.g., to model simulations for future scenarios.

My next major issue with this study is that authors appear to have overlooked some key literature and methodology on comparison of modeled vegetation with reconstructions. In particular the authors should have acknowledged and used the Delta-V method (Sykes et al., 1999) for comparing vegetation maps rather than the Kappa statistic. Delta-V is a robust method specifically designed for evaluating biome maps produced by vegetation models, that accounts for similarity between biome types in a way that Kappa cannot. If the authors feel that Delta-V is not applicable to their study, they should explain this in the manuscript. Along with using Delta-V, I have a number of specific comments on the text, which I describe below. Overall, this manuscript should be acceptable for publication after moderate revision.

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Specific comments:

Line 28 Change to ". . .these thresholds represent. . ."

Line 67 In the mention of FPC here, and as discussed elsewhere in the manuscript, it is not clear here how this quantity differs from PFT cover fraction. Sitch et al. (2003) explicitly equate gridcell-level FPC with PFT cover fraction (pg. 165, eqn. 8 and following discussion). In my understanding of the dynamic vegetation schemes of the ESMs used in this study, all would take a similar approach of equating FPC with cover fraction. Some more explanation, either here or at another point in the manuscript would be helpful.

Line 68 Here and at many other locations in the manuscript, the biome name "savanna" should not be capitalized. While many researchers mistakenly equate the biome "savanna" with the person or place name "Savannah", purposes of this manuscript savanna is not a proper name, and neither it nor any other biome names should be capitalized.

Line 94-95 I understand the PFT group names to represent super-groupings of PFTs. In this case, I can accept PFT groups called desert, forest, and grass, but the name "wood" does not make sense – how is this supposed to be different from the forest group. Some more clarification and precision of the terminology would be helpful here.

Line 107-109 The bioclimatic limits listed here needs citations to justify the choices made, or further explanation of how these values and thresholds were determined.

Line 111 Again, where does the arbitrary choice of a 2°C threshold in mean annual temperature for warm to cold desert come from? The widely used Köppen-Geiger classification separates hot deserts from cold deserts at a mean annual temperature threshold of 18°C (e.g., Kottek et al., 2006, and references therein). Provide citations or empirical justification for your choice of threshold.

Line 113 Change “residuum” to “residual”

Line 151 It appears some text is missing after the word “following..”

Line 154-155 The sentence “SEIB is a gap model. . .” needs further explanation to describe how it is different from other dynamic vegetation schemes. LPJ-GUESS is also a gap model, and it is also used in ESMs. It is not clear to the reader what is meant by this sentence and how it affects the results of the study.

Line 169-170 Given the authors emphasis “that this study is an introduction of a new biomisation method and not an evaluation of the different vegetation models. . .” it would have been helpful to focus the manuscript more on the methods and less on the very long evaluation sections that follow. In addition to Figure 1, I would have liked to see a flowchart showing the detailed decisionmaking process for classifying PFTs into PFT groups (or macro-PFTs as they are termed in the figure but not in the text) and then into mega-biomes, where all of the thresholds and other classification parameters are also included.

Line 179 The sentences starting with “As temperature threshold. . .” is not sufficiently linked to the previous lines – It is not clear what temperature threshold you are talking about. Clarify.

Line 183-184 Upon what basis or citation is the decision to set the limit for existence of C4 grass at a MTCM of 10°C? C4 grasses grow in climates with much colder winters in North America and Eurasia (see e.g., Still et al., 2003).

Line 186 How is the “dominant mega-biome type” selected. What if there is a tie among types? What if no type constitutes a majority of the cover (e.g., if there were three or more biomes present)?

Line 210 Please repeat the exercise using Delta-V (Sykes et al., 1999), or explain why you opted to not use this statistic, which is arguably more appropriate than Kappa when comparing vegetation maps. The FSS method described in section 2.4.2 appears to

be designed to deal with spatial offset, but not similarity or difference of vegetation categories.

Line 255 While the biomised pollen data the authors used in this study (BIOME6000) does not contain metadata about pollen catchment size, so it is impossible to know over what surface the BIOME6000 points are integrating. Nevertheless, in this section the spatial implication of the best neighbor score should be quantified, i.e., the comparison between model and biomised pollen data could be influenced by points how many km distant from either the pollen site or the nearest gridcell center?

Line 279-280 It is not clear from the world map figures – these are reproduced at a size that is much to small to discern anything but the broadest patterns – how the the "area covered by tropical forests is... more in line with observations". To clarify this point, provide a detail map to (as in figure 8) and/or summary statistics, such as total area covered by the biome in the simulations vs. the natural vegetation map.

Line 288-289 At the spatial resolutions used in this study, it is extremely difficult to resolve anything at the scale of Alaska (and impossible to see given the small size of the figures). Both BIOME1 and the vegetation models do not faithfully reproduce the vegetation of Alaska, so the meaningfulness of this comparison and the assertion in these lines is questionable.

Line 311 Why didn't the authors aggregate the biomised pollen sites on to the same grid used by the ESMs, similar to the process used by Trondman et al. (2015). Some areas have relatively few point observations of pollen spectra, while others many sites. A modal gridding procedure could have provided a more fair comparison between the models and the pollen-based reconstructions.

Line 353 It seems that Figure 8 should be called out at this point in the manuscript but it is not. The maps in Figure 7 are not sufficiently large to appreciate what is written in the text. Also the phrase "According to the records,..." needs a citation. What records are these?

Line 377 The phrase "e.g. Europe was mostly covered by grassy biomes. . ." is incorrect. It appears from the figure that, and in reality, Europe was mostly covered by ice. The assertion that Europe was largely treeless during the LGM is controversial and most previous modeling work has been inconclusive (Prentice et al., 2011). The most recent pollen-based reconstructions leave open the possibility of substantial woodland cover in parts of LGM Europe (Kaplan et al., 2016). A way to solve this for the current manuscript would be to delete this example from the text and choose another, less controversial example.

Line 401 The assertion that the "biome 'warm-mixed forest' (subtropical forest) . . . shares most tree species with tropical evergreen broadleaf forests" cannot be true, the citation to Ni et al., 2010 notwithstanding. First of all, the very high floristic diversity in the tropics and subtropics means that almost certainly different species grow in different habitats along a temperature gradient from the equator to the mid-latitudes. Perhaps some genera are commonly found in both biomes, but even then, most taxa are different, and perhaps the only thing these two biomes have in common is the presence of broadleaf evergreen trees, although even then, warm temperate broadleaf evergreen species are physiologically very different from true tropical species (e.g., Walter et al., 1971). Warm mixed forests are further distinguished from tropical forests by the presence of gymnosperms, which are rare to nonexistent in warm tropical forests. These characteristics distinguishing the biomes are acknowledged in the original BIOME1 paper (Prentice et al., 1992).

Line 403 The citation to Chen et al., 2010 is not included in the bibliography, but I am anyway skeptical about this statement, because as noted above, the presence of conifer taxa in an assemblage of otherwise (sub-)tropical pollen spectrum are indicative of warm mixed forest conditions, which is precisely how this biome can be identified (Prentice et al., 2000). More explanation/clarification are necessary here.

Line 408 The statement that ". . .warm-mixed forest is based on temperate broadleaved trees. . ." is incorrect, as this biome also, critically, includes needlelaf trees, which dis-

tinguishes it from, e.g., tropical forests. Even the BIOME1 paper recognizes this. The sentence should be clarified.

Line 415 While it is described later in the manuscript that this study only considers tropical savannas, the statement "Savannas [sic] require the coexistence of trees and C4-grass" is incorrect and must be clarified at this point in the text.

Line 423 Indeed, it is difficult to reconstruct tropical savanna on the basis of pollen data because tropical trees produce relatively little windborne pollen. A citation here would also be helpful, (e.g., Jones et al., 2011).

Line 467-468 It is not clear how biomised model output (biomes) is compared to the CRU-TS4 dataset (climate). I am missing a step in here. What was compared precisely? Clarify.

Table 1 This table appears to contain some of the information concerning the aggregation of model PFTs into PFT groups, but this is not indicated in the first column of the table, nor is the terminology consistent with what is used in the manuscript text and in Figure 1. Where is the PFT group called "wood" in the manuscript text?

Table 2 Provide a reference or empirical rationale for choosing these limits.

Table 3 Provide references or empirical rationale for the choice of parameters and thresholds in this table.

Figure 1 Some further explanation is needed to explain how the "simulated PFTs" relates to the long list of PFTs listed in Table 1. Also It is not clear how trees get classified into either a "... trees" (or "forest" in the text) PFT group vs. a "Woody PFT" (or "wood" group in the text). Clarification and careful standardization of the terminology across the text, tables, and figures, is essential for any revision of this manuscript.

Figures 2-4, 7, 10, 15 The maps presented here are generally two small for the reader to make anything but the most superficial of comparisons. Especially Figure 2 is too small, but the maps should be enlarged in all of the figures as much as possible and

presented in a standard size, e.g., by providing letter codes for each map within the map frame and then maximizing the size of the maps by removing the labels and whitespace around each map. Also the latitude and longitude labels are not necessary in this type of presentation and can be removed. To the contrary, the color legend should not use abbreviated labels, but instead give the full name of each mega-biome, so that readers can quickly interpret the figure without having to return to the text.

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