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Climate of the Past Discussions

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Interactive comment

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

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We would like to thank the anonymous Referee #1 for her/his constructive comments which helped to significantly improve the manuscript.

R: 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

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Discussion paper

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.

Our response: The major aim of our study is twofold: a) introduction of a new PFT-based biomisation method for comparing different Earth System Model simulations and b) application of the method to two cases, mid-Holocene and LGM. Perhaps our method is also applicable to simulations of future climate, but we do not have tested it. Therefore, we have chosen submission of our paper to Climate of the Past. Referee 3 explicitly agrees with our choice. However, we leave it to the decision of the editor whether the revised manuscript would be better published in GMD.

R: My next major issue with this study is that authors appear to have overlooked some key literature and methodology on comparison of modelled 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.

Our response: We fully agree with the referee that the Delta-V method is a powerful statistic to quantify the difference between simulated biome distributions. Though, Delta-V was designed to quantify the strength of change between biome maps by considering and weighting changes in attributes and plant forms and considering the dominance of the plants in the biomes. Biome models such as BIOME4 or BIOME1 include many biomes that differ only slightly in the PFT composition. In our study, we use mega-biomes that (mostly) differ strongly from each other. Changes in attributes such as the phenology of the plants do not play a role for this biomisation method, reducing the advantages of the Delta-V method. In addition, we do not focus on the biome changes between the time-slices, but on the differences between the simulated biome maps at a certain time. We assume that the maps do not deviate strongly from each other and from the references. As Sykes et al. (1999) state, the "kappa statistic is well suited to assessing the degree of similarity between two maps that are expected a priori to agree“. The further advantage of the kappa statistic is that it is a very widespread, well-known method. Thus, we think the kappa statistic is an appropriate method for this study, though, we are aware of the weaknesses of this statistic (e.g. treating all biome differences equivalently). The other methods (FSS and BNS) are very easy to understand and apply and are able to cope with slight pattern displacements. We will consider including the Delta-V statistic in the biomisation tool for upcoming research.

Specific comments:

R: Line 28 Change to “ . . . these thresholds represent ...“ Our response: done.

R: 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 grid cell-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.

Our response: Even if the FPC could be calculated for all models, the vegetation height is not a standard output in the CMIP5 database. So a comparison of the method based on all models is not possible. We decided to move the comparison between the PFT-method and the method by Prentice et al. to the Appendix E as the comparison is only based on one model and as the inclusion of temperate savanna in the biome distribution provided by Zhu et al. hampers the comparability with the PFT-biomisations.

R: 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.

Our response: We carefully looked through the manuscript again and corrected all capitalized biome names and change ’savannah’ to ’savanna’.

R: 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.

Our response: We agree, this is misleading. ’Wood’ includes all shrubs and forest PFTs. We add this information and change the names from forest to trees and from wood to woody PFTs. We now write: "In detail, the PFTs calculated by the respective dynamic vegetation model are aggregated into the groups ’trees’, ’woody PFTs’ (i.e. shrubs and all tree PFTs), ’grass’ and ’desert’ which is calculated by one minus the total vegetation“ and changed the PFT group names in the following text consistently.

R: 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.

Our response: The bioclimatic limits (with few exceptions) follow the limitations defined in the biome model BIOME4, we mentioned this in the lines 89-91 in the originally manuscript, but we agree that we have not emphasized this enough. We deleted Tab.2 and include detailed information on the references in the caption of Tab. 3 (now Tab.2). We add to the caption of Tab.3 (now Tab.2): .... "Bioclimatic limits and assumptions on minimum PFT coverage needed for the assignment of PFTs into the PFT-groups and into the 9 Mega-Biomes. The separation of boreal, temperate and tropical tree-PFTs is based on the same bioclimatic limits as the respective forest mega-biomes. Only temperature-based limitations are used, i.e. the growing degree days on a basis of 5°C

(GDD5) or on a basis of 0°C (GDD0), the monthly mean temperature of the coldest month (Tc), and the annual mean temperature (Tann). Bioclimatic limits are mainly taken from the BIOME4 model (Kaplan et al., 2003, marked with *). The limit for tropical forest is taken from BIOME1 (Prentice et al., 1992), but is also commonly used in DGVMs (e.g. JSBACH). The limit for the differentiation of deserts has been empirically determined in this study and is close to the value chosen by Handiani et al. (2013) and within the range of the Koeppen-Geiger climate classification for polar climate and the Holdridge alpine life zone classification. The Tc limit for warm savannas is taken from JSBACH (C4-grass criteria) to exclude temperate savannas. The assumptions on minimum coverage have partly been taken, partly been empirically adapted from Handiani et al. (2013)...“

R: 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.

Our response: We agree that the information on the biomisation criteria were imprecise. We tested several criteria for the differentiation of warm to cold desert (e.g. the BIOME1 criteria of 22°C for the temperature of the warmest month), but all criteria fail to separate the polar from the temperate and hot desert. Even the criteria chosen in Handiani et al. (2013) of a mean annual temperature above 0°C for the (warm) desert lead to grid cells with polar deserts in Asia (Taklamakan and Gobi) in some models. We decided to choose 2°C, because it was the lowest temperature (closed to the limit of Handiani et al.) that lead to a reasonable distribution of the polar desert. In addition, the limit is well in the range of the Koeppen-Geiger climate classification of polar climate (tundra climate: temperature of the warmest month between 0-10°C, ice cap climate: temperature during all months below 0°C) and the limit (Tann < 3°C) of the Holdridge life zone classification). In Tab.3 (now Tab.2) we wrote 'cold desert' which was misleading. We changed it to 'polar desert'. Furthermore, we add information on the biomisation criteria in the table caption (see last comment).

R: Line 113 Change "residuum" to "residual" Our response: done.

R: Line 151 It appears some text is missing after the word "following.." Our response: We corrected it.

R: 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.

Our response: We further explained the difference between SEIB and the other models in the revised manuscript: "SEIB deviates from the other DGVMs in this study as it does not use the tiling approach of calculating PFT fractional coverage for each grid-cell. It is a so called forest gap-model, simulating the interactions among individual trees that compete for light and space in arising gaps (e.g. due to disturbances) within a spatially explicit virtual forest. The model was built for capturing the vegetation dynamics on local scale. The application of the model for larger (e.g. global) scales is possible, but global simulations partly disagreed with observations (Sato et al., 2007). The PFT distribution used in this study has been calculated in the post-processing for CMIP5 via the relative net primary productivity of the vegetation categories, it was not explicitly calculated by the model, which may lead to additional biases in the vegetation distribution."

R: 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.

Our response: We agree that the evaluation of the biomisation method is long and we slightly shorten it in the revised manuscript. We furthermore move the comparison with the FPC method to the Appendix. Our intention for the study was the implementation of a tool that works for present day but also palaeo-timeslices and can be applied for all DGVMs. Therefore, we decided to include not only the mid-Holocene, but also the LGM time-slice, enlarging the study.

R: 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.

Our response: This is indeed a very helpful suggestion, we included such a flowchart in the Appendix for the VECODE model, i.e. the model with the fewest PFTs. Unfortunately this flowchart looks different for the other models, because all models use different PFTs. We will include detailed information on the biomisation for all models (e.g. flowchart) in the README file of the biomisation Tool, which will be provided when this manuscript will be finally published.

R: 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.

Our response: We revised this sentence: "RF99 additionally includes the biome ’Evergreen/Deciduous Mixed Forest/Woodland’. Here, this biome is assigned to the mega-biomes ’temperate forest’ in warm regions and ’boreal forest’ in colder regions via the modern growing degree days distribution (GDD5$\geq$900°C for warm region, GDD5<900°C for cold region), derived from observations (University of East Anglia Climatic Research Unit Time Series 3.1, University of East Anglia, 2008, Harris et al., 2012).“

R: 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).

Our response: Thank you for this information, we adopt this limit from the bioclimate criteria for C4 grass in the JSBACH model and it worked well. The limit MTCM of 17°C taken in other biomisation approaches (e.g. Roche et al., 2007 or Handiani et al. 2013) was too high.

R: 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)?

Our response: We calculate the dominant mega-biome in the model grid-cell by counting the number of grid-cells covered by this biome in the RF99 original grid. We explained it in the text: "Within each of this model grid cells, the dominant mega-biome type in the 5-minute-resolved RF99 data was taken for covering the RF99 grid-box in T31 or T63 or in the 10°grid. In more details, each grid-box on a T31 Gaussian grid contains 45*45 grid-cells of the 5-minute-resolved RF99 data. Within these 45*45 grid-boxes the fractional coverage of all mega-biomes is calculated and the biome with the highest fraction is chosen for covering the T31 grid-box. ....“ If there is a tie among types, this is not considered. In lots of grid-cell three or even more biomes are present, but in all grid-cells only one biome is dominant, which we take as mega-biome for this cell.

R: 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.

Our response: Please see the comment above, for this study, we decided not to use the Delta-V statistic.

R: 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?

Our response: This is indeed problematic, but so far, the BIOME6000 database is the best data available. As mentioned in the original text, the size of the neighbourhoods depends on the grid of the model data. For T31 we choose three grid-boxes in each direction, i.e. 11.25° on a Gaussian grid in each direction, for T63 we choose six grid-cells in each direction (i.e. 11.25° on a Gaussian grid) and for the 10°grid, we choose only one grid-cell in each direction. These regions seems to be very large in 'reality', but not in the model world. Due to the weighting by a distance decay function, the furthest off grid-cells of the neigborhood have only a very minor influence on the score. We included some examples on the meaning of the BNS, translated into distances: "For instance, a BNS of approx. 0.82 or 0.46 means that (in the mean) the best neighbour grid-cell is among the grid-cell 'circle' next to the site-locating grid-cell in T63 or T31, respectively. Accordingly, a BNS of 0.04 indicates a distance between the best neighbour and the site-locating grid-cell of of 7.5° on a Gaussian grid.“

R: 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.

Our response: Indeed, the main aim of these global figures is to capture the main differences in the broad pattern (just by eye). A detailed validation of the simulated biome pattern is given by the metrics. As South America is an 'outstanding' region, because the tropical forest biome is strongly underestimate in the climate based biomisations (for most models) and well captured in most of the PFT-based biomisations, we include a Table in the Appendix to verify this statement.

R: 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.

Our response: Thank you very much for carefully reading the manuscript. Alaska is indeed imprecise. We mean Alaska and the north-western parts of Canada. We corrected this in the revised version.

R: 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.

Our response: The gridding of the biomised pollen sites is much effort and is not the subject of this study. We are no experts for pollen-based reconstructions. We just used the best global synthesis available for us to evaluate our model results.

R: 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?

Our response: We refer to the BIOME6000 reconstructions, shown in Fig.7. The records are not shown in Fig.8. We shift the '(Fig.7)' -reference to the end of this sentence and inserted 'BIOME6000'. Again, the aim of the figure is giving a broad (not detailed) overview of the biome distributions in the different simulations. We did not enlarge the figures to make possible that they still can be plotted on one page.

R: 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.

Our response: We agree with the referee that this is not the best example and deleted this sentence in the manuscript.

R: 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). and R: 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.

Our response: We apologize for misinterpreting the literature and the discussion with palynologists. We discussed it again and corrected this sentence to: "The mega-biome 'warm-temperate forest' (e.g. subtropical forest) includes PFTs that can be assigned to several biomes and is rather defined by a co-existence of certain PFTs. For instance, in the BIOME4 model it is not only defined by the dominance of temperate evergreen broadleaved trees, but can also be defined by a dominance of cool conifers (with sub-PFT of temperate evergreen broadleaved trees). The cool conifers – in turn - are also part of temperate forest biomes. Given the limited number of PFTs in the DGVMs , the confinement of biomes via PFT-mixtures is not possible. As biome models such as BIOME4 generally manage to simulate warm-temperate forest at the correct locations, we adopt the bioclimatic limits from BIOME4 (limit for temperate evergreen broadleaved trees) for defining this mega-biome. Nevertheless, the calculated warm-temperate forest distribution strongly disagree with the reference datasets. The warm-temperate forest shares most subtropical tree species with the tropical evergreen forest (Ni et al, 2010). These biomes tend to overlap in some regions and are sometimes mixed up in reconstructions (Chen et al, 2010). In addition, this mega-biome includes the 'warm temperate rainforest' and the 'wet sclerophyll forest and woodland' in the BIOME6000 reconstructions (cf. Harrison, 2017), which may not be able to be identified with our biomisation method. Regarding the modern reference of RF99, we decided to assigned the biome 'temperate needle-leaf evergreen forest and woodland' of the RF99 dataset to the mega-biome 'temperate forest', although, this biome is also located e.g. in the southern USA which should be assigned to the warm-temperate forest. Therefore, the evaluation of this method with respect to warm-temperate forest might be ambiguous. The correct reproduction of the warm-temperate forest distributions further hampered by the rather regional distribution of the warm-temperate forest covering only few grid-cells." We added the reference of Chen et al to the references: Chen, Y., Ni, J., Herzschuh, U., 2010.: Quantifying modern biomes based on surface pollen data in China. Glob. Planet. Chang. 74, 114-131. R: 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 distinguishes it from, e.g., tropical forests. Even the BIOME1 paper recognizes this. The sentence should be clarified.

Our response: We changed this sentence to (see above): "As biome models such as BIOME4 generally manage to simulate warm-temperate forest at the correct locations, we adopt the bioclimatic limits from BIOME4 (limit for temperate evergreen broadleaved trees) for defining this mega-biome.“

R: 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.

Our response: We add the word ’tropical’ : “While tropical savannas require the co-existence of trees and C4-grass, they can only be distinguished from forests by their unique functional ecology, fire tolerance and shade intolerance (Ratman et al., 2011)”

R: 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).

Our response: Thank you very much for bringing this problem to our mind. We included it in the revised manuscript: “Modern pollen rain analysis reveal that woody plant taxa typically characterizing savannas are under-represented or even absent in the pollen/vegetation ratios. Given the lack of savanna indicators, this biome may be overlooked in fossil pollen records (Jones et al. 2011).”

R: 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. Our response: We agree that is indeed not precisely described. We now write: " ...we compare the pre-industrial climate-based biomisations with the biomisation of the CRU-TS4 dataset. . ..“

R: 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?

Our response: This table has nothing to do with the aggregation of the PFTs into the PFT groups used in the biomisation. We just divided the PFTs in different PFT classes to structure this table. We added the header ’PFT class’ to the first column.

R: Table 2 Provide a reference or empirical rationale for choosing these limits. and R: Table 3 Provide references or empirical rationale for the choice of parameters and thresholds in this table.

Our response: We provided detailed references on the bioclimatic limits and the thresholds in the table caption (see comment above). Please notice that we deleted Tab.2.

R: 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.

Our response: This is indeed misleading. We changed the caption to: ". . .The plant functional type (PFT) fractions simulated by the individual dynamic global vegetation models (DGVM) are assigned to the PFT groups ’desert’ (i.e. 1-total vegetation), ’grass’ (containing all grass PFT-types) ’woody PFT’ (containing all trees and shrub types) and ’trees’ (containing all tree types). The ’trees’ and ’woody PFTs’ are further differentiated into ’tropical trees’, ’temperate trees’, ’temperate woody PFTs’ and ’boreal woody PFTs’ via bioclimatic limitations (Tab.2). For DGVMs explicitly distinguishing tropical, temperate or boreal tree types, the original classification of the DGVMs is used. Afterwards the PFT-groups are assigned to nine mega-biomes by assumptions on the minimum coverage of certain PFT-groups needed in a grid-cell and additional bioclimatic limitations (Tab.2)." We furthermore changed the column header and the terminology across the text etc. accordingly.

R: 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.

Our response: Thank you very much for these suggestions to improve the figures. We enlarge the figures by deleting the latitude and longitude labels and also change the colour legend. We decided to keep the model labels and not to use letter codes. We think, the reader can then find a certain model more quickly.

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