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Climate of the Past Discussions

Interactive comment on "Climate–vegetation modelling and fossil plant data suggest low atmospheric CO_2 in the late Miocene" *by* M. Forrest et al.

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This paper presents a reconstruction of late Miocene vegetation using a dynamic vegetation model driven by the climatic outputs of climate model runs for two different partial pressures of CO2 in the atmosphere, 280 and 450 ppmv. These partial pressures reflect the range of atmospheric CO2 pressures that have been reconstructed from proxy data for the late Miocene. The authors compare the vegetation reconstructed with palaeovegetation data available for this time period. They also compare in detail their results with late Miocene vegetation model reconstructions published in the literature. For the comparison with the data, they build an agreement index (AI) which is an

interesting and relatively novel aspect of their work. Since the AI is significantly higher for the low CO2 (280 ppmv) case, they conclude that climate and vegetation modelling suggest low CO2 in the late Miocene and so would favour the lower values in the range exhibited by the proxies.

The paper is generally well written, scientifically sound and with some clearly novel aspects with respect to previous work on the subject. I am thus in favour of its publication in Climate of the Past. I just have a few comments or suggestions that the authors might want to address.

(1) Section 3.4 : your comparison at the PFT level and associated statistics is presented as a new method for model-data comparison. However, as mentioned by the authors, François et al. (2011) have also performed a similar comparison at the PFT level, and contrary to what is said here, they also used the PFT diversity from the data (see for instance their table 7 and the comparison with model NPPs in their figure 6), although only presence-absence is used in their kappa calculation. What is the advantage of your AI index compared to the more traditional kappa method ? Kappa can also be averaged over sites or over PFTs. The statistical study on kappa presented here for AI (which is really interesting and the most novel contribution of this paper) is also possible for kappa. You just define more classes (abundance classes) that may also be involved in the kappa method, but actually have not been involved because of the large uncertainties on model PFT abundances. Models are certainly more robust in evaluating presence/absence than abundance. Morever, as mentioned in your section 3.4, it is not obvious that PFT diversity from the data can directly be compared to model abundances. Even presence/absence in the data may be uncertain due to the PFT assignment scheme in the data (see again François et al., 2011). This may also critically depend on the number of PFTs in the classification used. This might be discussed somewhat more, because the associated uncertainty might have some impacts on the conclusions reached.

(2) Section 4.1, figure 2 : it might be interesting to add on figure 2 the AI that would be

obtained with present-day (control run) model vegetation (when comparing to palaeo-data). Is it significantly different from the AI for the 450 and 280 ppmv late Miocene configurations ? If it is close to the 280 ppmv late Miocene case, it might mean that your model is not fine enough to discriminate between the present-day vegetation and the late Miocene one.

(3) Section 4.3.1 : the characteristics of Miocene vegetation in Europe is indeed as discussed here the widespread presence of temperate deciduous trees, with some temperate evergreens in the south. Evergreens are however different from present-day Mediterranean (drought-tolerant) evergreen trees, since data show the presence (not dominance) of temperate evergreen perhumid trees. This is a very important climatic constraint from the point of view of the data, while your model does not separate between drought-tolerant and perhumid temperate evergreen trees. The impact of this simplification on the results should be discussed, or at least it should be mentioned. Also, your figure S2 indicates that the SI index strongly varies from one site to the next. This is an important result that shows that there are still some features that are not well captured by the model (or possibly it might be a problem in the interpretation of the data). It would be interesting to discuss figure S2 in the main text.

(4) Section 5 (Summary and conclusions): In view of the large uncertainties on climate models (including other boundary conditions than CO2), vegetation models and PFT classification, I am not sure that models can really provide a strong constraint on palaeo-CO2. It is interesting to learn that you model is more consistent with low CO2 in the late Miocene, but this is a very indirect constraint. I would suggest that you reformulate the last sentence of your conclusion to make the statement less direct (there are uncertainties and it may be model-dependent, so we may need to study the same problem with other climate/vegetation models).

(5) Some small typos:

âĂć P 2254, line 10: ‘possibly because’ âĂć P 2262, line 25: ‘Fig 1a and b’ does not

correspond to the present-day biome map, it should be figure S1 âĂć P 2263, line 7: ‘It also shows a band’ âĂć P 2263, line 12: ‘particularly shrubs’

Interactive comment on Clim. Past Discuss., 11, 2239, 2015.