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Interactive comment on "Decomposing uncertainties in the future terrestrial carbon budget associated with emission scenario, climate projection, and ecosystem simulation using the ISI-MIP result" *by* K. Nishina et al.

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In this manuscript, Nishina et al. report results of an ensemble of 120 projections of the terrestrial carbon cycle from six GVMs driven by climate projections from five GCMs under atmospheric four RCPs scenarios up to the year 2100. The focus of the study is to determine the relative contribution of the ensemble components (i.e. GVM, GCM and RCP) to the total uncertainty in projections of 21st century NPP, and changes in vegetation and soil carbon stocks.

The relative contribution of different sources of uncertainty to the total uncertainty is not a particularly new topic (e.g. Ahlström et al., 2012). However, the strength of this manuscript is the large number of projections, and the explicit ANOVA-based quantification of the relative contribution of each component to the total uncertainty as represented in Figure 2 and 4.

Main comments:

The manuscript is relatively short and the language needs to be clarified in places, especially the abstract. This study would also benefit from some more details in the methods. For example, an equation for the cophenetic correlation coefficient is missing. Perhaps the authors could consider including a table with each GVM's main features that can be related to the results presented here: e.g. is Ra a fixed fraction of GPP? How is phenology handled? What are the reference NPP, VegC and SoilC to which projections are compared? etc. . . I would also like the authors to discuss their results in the context of their findings in Nishina et al. (2014) that use the same ensemble.

My main suggestion is that, although I agree with the main findings, I believe that some more analyses would strengthen the paper, such as reporting the changes in VegC and SoilC turnover times. The parameterization of turnover time is highly model-dependent and controls pool sizes at equilibrium (e.g. Exbrayat et al., 2014) because VegC transfer to litter / soil pools, and SoilC decomposition are represented using first-order kinetics. Therefore, (initial) pool sizes control the absolute response of C release following a relative change in turnover time in response to changes in environmental conditions. Therefore, part of the uncertainty in VegC and SoilC shown in Figure 1 may be partly attributed to differences in historical VegC and SoilC pools reported in Nishina et al. (2014). Studying changes in turnover times would enable to compare GVMs with a new approach.

Specific comments:

p. 1200 l. 6: "Potsdam"

p. 1201 l. 14: Could the authors detail why they only use 70 simulations? From 6 GVMs, 5 GCMs and 4 RCPs, there should be 120 simulations available.

From p. 1201 l.26 to p. 1202 l. 7: Isn't SDGVM a DGVM? Could the authors also indicate which fixed land cover data they used in the GVMs? How comparable are DGVMs and GVMs with fixed land cover?

p. 1205 ll.3-5: please rephrase this long sentence.

p. 1206 ll. 2-3: historical simulations are not reported here.

p. 1206 ll. 8-9: "in previous inter-comparison of models"

I struggle to read Figure 3. It is a very complicated figure that deserves a more detailed description than the short paragraph 3.3. In a dendrogram, the definition of a cluster relies on the choice of a threshold in the similarity value, a subjective choice that has to be indicated and justified here. I agree that the four GVM-based clusters for SoilC are fairly obvious, but the number of clusters for NPP and VegC can vary several folds following slight variations in the threshold and it is hard to relate the description in paragraph 3.3 to Figure 3. Figure 4 would benefit from using another colour scheme with more contrast better extreme values (classical RGB?).

References

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