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Interactive comment on "The compact Earth system model OSCAR v2.2: description and first results" *by* Thomas Gasser et al.

Anonymous Referee #2

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This paper is an extremely well written and thorough description of the OSCAR 2.2 model. The model carries a significant number of innovative approaches that will prove very useful in investigating the range of possible forcings, feedbacks and interactions within the full Earth system that will ultimately determine the global and regional response of this system to anthropogenic emissions of greenhouse gases, aerosol precursors and human-induced land use change.

From this perspective I recommend that the paper is published with only minor revisions for the sake of clarity and brevity. These are indicated below under requested revisions. In addition to this I have a number of general suggestions and comments/questions, which might further improve an already very good paper. The authors might like to consider some of these.

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Discussion paper

General points. 1. The paper is very long. I realize this is necessary to provide the level of detail required for a reader to properly understand the formulation of the model. The sections describing the model components are generally very good and very clear. That said, the section where model simulation results are presented for the historical period is actually quite thin and the weakest part of the paper. In particular, a number of areas where OSCAR deviates significantly from observations, more complex models or IPCC best estimates are not always fully explained or discussed. Also the model is designed, primarily, to allow a probabilistic investigation of future Earth system change. No examples of the application of the model to possible future conditions are included in the paper.

Hence my main suggestion is that the authors consider a high-level restructuring of the paper and instead submit 2 (linked, Part I, Part II) articles, with Part I essentially being the model description part of the submitted article and Part II being (i) an extended version of the present section on the historical period simulation (with some more discussion and explanation of deviations from observations/other models/IPCC estimates and (ii) include an initial example of how the model can/will be applied in the context of investigating future Earth system change. I realize that point (ii) is no doubt intended by the authors in subsequent papers, nevertheless, some brief examples of how the model is to be applied in a future projection sense would be illustrative in the context of the model description paper (my suggested Part I). Furthermore, in the Part I article I would recommend an initial section that gives a very brief overview of the model structure (aimed at non modeling scientists that may still be interested in the model application, e.g. results presented in Part II) along with a note to the effect that readers interested in the model formulation should read all of Part I, while those mainly interested in the model application and results can just read the short description and then jump to Part II.

This suggestion, which is just that (a suggestion), would in my opinion make the combined papers significantly more interesting to a wider audience than the present paper.

If the authors prefer to keep the present single article then I suggest they more carefully discuss/explain some of the key deficiencies in the historical simulation as this section was a little thin in places.

Requested revisions 2. In a general sense I found the approach of emulating/constraining the OSCAR components and parameterizations using more complex model results (e.g. CMIP5/HTAP) or IPCC results, assumed that the reader was already well informed of this type of approach. , i.e. it was not always clear if emulation/constraint was being applied and used across the ensemble of CMIP5 models or rather to a "best fit model vs observations" or an ensemble mean was being used to constrain OSCAR parameters for the historical period. Request revision: A brief and basic description of the emulation approach might help more general readers

3. Given the importance of marine carbon uptake and potential changes in the efficiency of this carbon sink/source in the future, I felt the level of detail describing the marine C cycle compared to the terrestrial C cycle was somewhat unbalanced. Equally, I was surprised that marine C cycle did not include any parameterization of the marine biological pump. While solubility processes may dominate historical and future marine C uptake, there is evidence that changes in the biological pump are likely to play a non-negligible role in future marine carbon uptake. On this note, does the relative accuracy of the ocean carbon uptake for the historical period (shown in figure 5) indicate that the biological pump was largely unimportant in this increased uptake? Or does it suggest the way the model has been constrained implicitly includes a biological component? Requested revision: Some comments on the importance or not of the marine biological pump seems warranted, particularly if the primary application of the model is to investigate future uncertainties in coupled climate-carbon cycle processes.

4. Is ocean acidification and its potential impact on marine carbon uptake included in the model? This was not obvious to me. Requested revision: Please make it clear if yes and if not, as with point 2 above, what are the possible consequences for application to future projections.

5. For certain model parameters: e.g. environmental controls on fire ignition, as an example, it is not clear how many more complex ESMs with interactive fire models were used to determine these parameters. The number of CMIP5 models with interactive fire models was pretty small. Requested revision: Particularly where only a small number of models were available for constraining parameters this should be made clear.

6. With respect to the terrestrial carbon cycle and atmospheric CH4, it seems that permafrost is not included in OSCAR? Is this correct. If so why was this decision taken and, like my comments in marine biology, there is evidence that permafrost melt may be an important future feedback in the Earth system. Requested revision: Omission of this feedback seems like it needs a motivation and acknowledgement of potential projection limitations due to this decision.

Questions (not particularly requiring modifications in the paper unless the authors feel it will help)

7. With respect to surface temperature and precipitation changes (pages 33-34) the global climate sensitivity (λ) plays an important role. This is derived from CMIP5 abrupt 4xCO2 and pre-industrial control simulations. λ includes all "fast" climate feedbacks such as water vapour and cloud feedbacks. My question relates to the definition of cloud-aerosol effects in OSCAR, these seem to be potentially decoupled from (future) cloud changes, with the latter defined through λ. As future cloud aerosol effects will be mediated by any future changes in the distribution of fractional cloud and cloud microphysical properties, is there some risk that future cloud aerosol impacts may be inaccurate due to this decoupling?

8. A similar question arises with respect to the calculation of precipitation and in particular, the regional weights for precipitation. How are cloud-aerosol changes on regional precipitation included, if at all, in OSCAR?

9. It is stated that the model is primarily used for annual mean or longer analysis. This is understandable given the time step and basic aims of the paper. My question

is whether, in an approach analogous to statistical downscaling which brings an increased spatial dimension to coarse spatial resolution data can something similar be done in an effort to infer higher time frequency changes based on the annual mean timescale changes?

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