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

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Comment 2.0. 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.

Response 2.0. We thank the referee for his/her review.

C2.1. 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.

R2.1. We perfectly understand the reasons motivating the referee's suggestion. And the referee is perfectly right to assume that we plan a paper equivalent to his/her suggested 'Part II' for the future.

However, the main reason why we did not submit two companion papers is the time needed to write that second paper. During this time the first paper would not be available to the scientific community. OSCAR is a model that has been used already in various studies, and that is currently being used for others. So we felt (as many reviewers of said studies did) that a clear and precise description of the model was long overdue. And for the sake of time, we decided to submit a first paper, with a second one clearly in mind (simulations are ongoing as these lines are being written). Note also that the specific policy of GMD consisting in linking papers about the same model will partly compensate for the absence of 'Part I' and 'Part II' mentions on the papers.

As for the issue of what simulations should appear in this description paper, it was a difficult choice to make. The historical simulations with comparison to observations seemed to be a pre-requisite. But there are several reasons why we opted for no projections are all:

* First, we could not find one scenario to follow for the 'example of use' of the model: why make simulations for the RCP8.5 and not the RCP2.6? But if you choose 2 of the 4 RCPs, why not the 4? Coupled to the fact that the RCPs can be run in an emission-driven or concentration-driven fashion, this leads to already a lot of additional simulations.

* Second, this description paper is already quite long. To discuss even one simulated scenario would make it even longer – especially as it would likely require to increase the number of figures shown – and this is something we wanted to avoid.

* Third, there is an important conceptual difference between historical simulations and

projections. In the former case, we can compare our results with observations.

In the latter case, we only have other models results to compare with. This complicates (and extends!) the discussion: in the former case, a departure from the observation is a bad performance of the model; in the latter case, it may be explained by a difference in the models' structure. For all these reasons, we decided to show only the historical simulations, as it is ultimately the only way to (in)validate a model such as OSCAR. And we decided to keep the (lengthy) discussion about how to use observations to constrain the parameterizations of OSCAR, and how the (constrained or unconstrained) model performs for projections compared to e.g. CMIP5 for 'Part II'. We only hint at those things in the first two paragraphs of our conclusion.

C2.2. 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

R2.2. We have added a paragraph in introduction that has 2 purposes: summarizing what OSCAR is, and explaining the main idea behind how it is made (i.e. the emulation/calibration and probabilistic approach). The text referring to the emulation approach in the paragraph is: "*OSCAR is also a parametric model which relatively large number of parameters are almost all calibrated on complex models. We call this approach meta-modelling: each module of OSCAR is designed to emulate the behavior of other more specialized models (e.g. global climate models, dynamical vegetation models, or chemistry-transport models). For most modules, we have access to several sets of parameters (one per complex model used to calibrate) and, rather than taking the average or arbitrarily choosing one, we adopt a probabilistic approach in which a* *given simulation with OSCAR is repeated many times with different sets of parameters picked at random.*"

C2.3. 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.

R2.3. Unfortunately, the level of detail used to describe the marine C-cycle compared to the terrestrial one simply reflects the complexity of the modeling approach for these two components of the Earth system. Therefore it is true that the module for ocean carbon is 'simpler' than the one for terrestrial carbon, especially because it lacks a representation of key processes such as those related to the biological pump and also does not take into account regional specificities. This is both because of the history of the model development (see Changelog in appendix A in paper) and because the land-use module makes the terrestrial C-cycle rather complex. That said, we do not know of any other 'compact' model that would have a more complex ocean C-cycle representation, since it very quickly requires to explicitly model the multi-dimensional (fluid) dynamic in the ocean.

Because OSCAR is formulated as a difference to a preindustrial equilibrium (in which zero anthropogenic forcing is assumed), we can say that there is an implicit biological

pump in the model, with the important limitation that this pump is implicitly assumed to remain unchanged throughout any simulation. Is this bad? Well, for the historical period, as the referee noticed, no change in the biological pump seems to be needed to model the ocean C sink. Back in the Third Assessment Report, IPCC stated that so far (i.e. 2001) the physical pump explains virtually 100% of the ocean sink, that is: "Despite the importance of biological processes for the ocean's natural cycle, current thinking maintains that the oceanic uptake of anthropogenic CO2 is primarily a physically and chemically controlled process surimposed on a biogically driven carbon cycle that is close to steady state". In other words, the anthropogenic perturbation of the ocean C-cycle can only be seen in the physical pump and not the biological one. Additionally, we did try to implement a simple biological pump model on top of the Joos et al. (1996) physical pump model, but those happen to be incompatible, and more work in this direction is clearly needed.

To sum this up: we chose to keep the biological pump constant, and we will see with the RCP projections (albeit in another paper) how the model performs in the future. We've made this clearer in the sentence that was initially dedicated to this in the 'ocean C-cycle' section: "*Note that this model of the ocean carbon-cycle implicitly assumes no change in the biological pump – change that could be induced e.g. by changes in temperature, ocean circulation, nutrient availability or surface acidity (e.g. Ciais et al., 2013). This is one of the several processes not implemented in this version of OSCAR.*"; and added a specific comment in the results section: "*This relative good performance of the ocean carbon-cycle module, given that no change in the biological pump is simulated by OSCAR, suggests that the physical pump is enough to satisfactorily simulate the (recent) past carbon uptake by the ocean, as noted by Prentice et al. (2001). Whether this would be enough to simulate future changes remain to be tested.*"

C2.4. 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.

R2.4. It is not entirely clear what the referee refers to, here. If the referee is mentioning the non-linearity of the carbonate chemistry (i.e. the 'effect' of acidification on carbonate chemistry and on the saturation of the carbon sink) then yes it is accounted for in the FpCO2 non-linear function. If instead the referee refers to the potential effect on the biological pump then it is not accounted for. Note that the acidification effect on biological production is not included as well in most comprehensive models of the ocean carbon cycle. The modifications made in **R2.3** make the latter point clearer.

C2.5. 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.

R2.5. In the first version of the paper, we do provide how many models were used to calibrate the parameters in each case. For instance, in the previous manuscript, on page 11 line 4: "*Six models with wildfire emissions are available to calibrate on TRENDY, and four models are to calibrate on CMIP5.*" All these values are also summarized in the last table of the paper.

C2.6. 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.

R2.6. It is correct that permafrost is not included. And it is true that we failed to ac-

knowledge that permafrost was missing. This is now corrected in the section dedicated to atmospheric CO2: "*In equation (33) this version of OSCAR notably ignores the permafrost carbon that may be emitted under a warming climate (e.g. Ciais et al., 2013).*"

We don't think, however, that this requires much explanation in the paper: many processes are missing, and some of these processes may have a greater impact than permafrost (e.g. N-limitation of the terrestrial C sink, or biophysical effect of land-use). The reasons why these processes are missing are various: unavailability of data exploitable to calibrate, unsuccessful attempts, decision to freeze the model at some arbitrary point. We do not believe these reasons should appear in the paper.

C2.7. 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 () plays an important role. This is derived from CMIP5 abrupt 4xCO2 and pre-industrial control simulations. 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 . 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?

R2.7. The referee is right: the aerosol-cloud effect is estimated independently from any actual change in cloud cover induced by the overall climate change. It is however impossible to couple both effects without developing an explicit energy/water model (with atmospheric transport!) in place of the response functions we use. We have added a sentence in the 'cloud effects' section to precise this non-coupling: "*Note that the cloud effects are estimated independently from any change in cloud cover that is happening implicitly in the climate system module.*"

C2.8. 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?

R2.8. Again the referee is right. But it is actually true for all forcings: they are not distinguished when it comes to their impact on regional precipitation (though they are for global precipitation). A sentence has also been added: "*As per surface temperature, the pattern scaling approach ignores the difference in effect the various climate forcers may have on regional precipitations.*" Note also that this was already acknowledged in the discussion/conclusion.

C2.9. 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?

R2.9. This is a very interesting point. In some aspects, it relates to a point regarding inter-annual variability made by the other referee. The answer to the question as to whether it is possible to implement intra-annual variability in OSCAR is: yes, in theory. This would likely be done ex-post, using outputs from a simulation to 'downscale' the timeseries. One would need to calibrate the intra-annual cycle of the preindustrial period, e.g. on 'piControl' experiments from CMIP5, but also to calibrate how that cycle is affected by other changes in annual/decadal variables (e.g. how regional mean temperature affects the monthly or daily temperature profile). There is little doubt that the latter point would require a lot of work!

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