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Geoscientific Model Development Discussions

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 1.0. My apologies for taking so long to get my comments in. This is a very well written paper that carefully and meticulously documents the new parameterized ESM called OSCAR. I have some minor issues outlined below, but overall, I would be happy to use this model in any of my applications where it is needed. OSCAR rests clearly on the analyses of the full chemistry-carbon-climate models and these sources are carefully documented here.

Response 1.0. We thank the referee for his/her review and support.

C1.1. Intro is OK, but (p.2) is OSCAR just another box model? Please say so, describing it as? Parameterized, multicomponent ESM without a model grid?

R1.1. Following this comment, some below, and comments from the other referee, we've decided to expand the introduction by with one paragraph, to take more time to present the model and the general idea of how its parameters are derived (what we call meta-modeling).

"*Here, we present an important update of a simple Earth system model that has already been used for some time. The model is named OSCAR, and this paper provides a comprehensive description of version 2.2. OSCAR can be described as a non-linear box model which number of boxes, however, is fairly large. It is not spatially resolved (i.e. it is not gridded) but key processes such as land-use change or aerosol physico-chemistry are regionalized to account for the disparity in such processes that is observed in the real world. OSCAR does not endogenously simulate intra- or inter-annual variability. Consequently, although the time-step of its inputs and outputs is one year, the main purpose of the model is to simulate trends in the Earth system change, and not year-to-year variations. 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.*"

C1.2. I like the idea of the regional boxes being used to account for the heterogeneity of the global forcing/response. Whatever, but get this upfront.

R1.2. The general approach for regionalizing is now also explained in this added paragraph in introduction.

C1.3. I admit that I did not proof all of the equations here, but rather looked at structural problems and inclusiveness. A key question (answered for me at the end) was is there a source of interannual variability and climate chaos? Of ENSO? in OSCAR. Apparently not – but if it could be implemented, how would it change the analyses and ensembles?

R1.3. The fact that there is no inter-annual (nor intra-annual) variability is now mentioned upfront in this additional paragraph about what OSCAR is.

Now, the issue of whether the inter-annual variability can be implemented in the model and how it would affect its results is not a small one! First, since OSCAR is not process-based it seems impossible that the variability be endogenous – except by adding specific (stochastic) equations to generate it. Second, since OSCAR has been thought and developed to focus on the trend of (anthropogenic) climate change, all equations would need to be modified to include the variability as an explicit driver. This would involve, for instance, to implement a stochastic model of the Southern Oscillation Index (or at least forced with observed values), with the value of the SOI then influencing e.g. the NPP or emissions from biomass burning, following laws calibrated on e.g. CMIP5 models.

One can easily see that implementing all this is no small work, especially if natural modes of variability other than ENSO are to be considered (e.g. the Atlantic Multi-decadal Oscillation or the QBO for the stratospheric chemistry). We agree with the referee, however, that this would be an extremely interesting direction in which the model could be developed. That said, there is little if any evidence that this would result in any important change in the trends simulated by the model, given how relatively 'smooth' the differential system of this version is. We believe that the work on natural variability would be more interesting if 'strong' non-linearities of the system were also added to the model (e.g. permafrost, ice-sheets, . . .).

C1.4. OSCAR = "*Earth system change model.*" Excellent.

R1.4. Thank you. We do believe this is an important distinction to make.

C1.5. p.3 "*but all other sectors provided by the inventories are accounted for*" – does this include aviation and shipping. OK, 2.2.3 explains part of this.

R1.5. Emissions from aviation and shipping are included in what we call the "*anthropogenic emissions*" drivers. What is detailed in 2.2.3 is that the effect of contrails is included in the model only as a direct RF.

We extended the sentence quoted by the referee to add shipping and aviation as an example of what is included in these drivers. This gives: "*Note that the emissions from biomass burning of natural vegetation are removed from these datasets, since those emissions are endogenous to the OSCAR model (see section 2.4.1), but all other sectors provided by the inventories are included, notably (but not only) agricultural waste burning, and shipping and aviation.*"

C1.6. p.7 "*nutrient deposition – albeit the latter not in this version of OSCAR.*" OK, I would have liked to see this included and am glad it is clearly stated. Perhaps a table of links and processes with a yes/no/partial would be useful to scan?

R1.6. We understand the referee's motivation in suggesting this table of included/excluded processes. And we gave it a try but weren't satisfied with the result. We find really difficult to decide where to stop in the details provided in this table. Let's take the ocean carbon-cycle as an example. If we want to speak generally, so as to keep the table short and easy to read, one can say that our model includes the physical pump but not the biological pump. But even for the physical pump, one can argue that it is only partially included, as e.g. no change in sea-ice cover or over-turning circulation is implemented in the model. And, because our model is formulated in perturbation, one can also argue that there is a biological pump that (implicitly) remains constant whatever the environmental/anthropogenic perturbations. So to be thorough we should provide a table of the links between the state variables of the model, e.g. the DIC of the surface ocean is a function of the carbon in the surface ocean, of the mixed layer depth, and of the sea surface temperature. But this would make the table rather long and complicated. And summarizing these causal links is the reason why we made figure 1 initially.

However, we can agree with the referee that the mentions of what is not included in the model were difficult to find in the main text. So we've added additional introductory paragraphs to the subsections dedicated to CO2, CH4, N2O, O3 and the aerosols, in which we very briefly list the main processes that are implemented in OSCAR and those that are lacking. We believe that these introductory paragraphs, combined to figure 1, are enough for the reader to understand what is or not implemented in OSCAR.

C1.7. p.12 "*the migration of natural biomes caused by changes in environmental conditions (e.g. Jones et al., 2009). This is however not included in this version of OSCAR.*" Another item for the table of processes.

R1.7. Yes. See above.

C1.8. p.13 "*disturbe*" >disturb – surprising, minor. Overall this paper is very well and clearly written.

R1.8. Corrected.

C1.9. p.18 "*it is parameterized by three relative chemical sensitivities (-) and the preindustrial natural emissions of the three ozone precursors (ENOxnat , ECOnat , EVOCnat)*" OK, but worrisome, would clearly like to have a latitudinally dependent impact of the NOx and VOC emissions at least (probably not CO). for example, "*All the chemical sensitivities of the OH sink (i.e. ...) are taken as one of the four sets of values from the study by Holmes et al. (2013, table 2)*" This is really fine, but again ignores the NOx vs Latitude reactivity (Wild et al, 2001, Indirect long-term global cooling from NOx emissions, Geophys. Res. Lett.)

R1.9. Yes, the referee is entirely right: the OH chemistry (affecting the methane lifetime) should be regionalized. However, we did not find relatively recent studies that were readily exploitable to do this regionalization (e.g. the HTAP simulations used for

O3 did not report OH-relative variables). Also, we feel this regionalization is somewhat less crucial than that of e.g. O3 because in the model it affects only CH4, and CH4 is treated as a well-mixed gas. However, we agree this should be tested and requires additional model development. We decided to leave this feature and discussion for a future version of the model for which a better (and more consistent) atmospheric chemistry would need to be developed, based on more detailed global atmospheric models.

Nevertheless, it appears our paper failed to acknowledge this overlooked effect. We have therefore added a sentence right after these equations: "*None of these two formulations shows regionalized chemical sensitivities of the OH sink, however, whereas in reality the sink is sensitive to where the ozone precursors are emitted – especially the NOx (e.g. Wild et al., 2001).*" We've also added a small specific reference to that aspect in the discussion/conclusion section.

C1.10. p.19 "*This implicitly assumes that all the natural sources of methane but natural wetlands remain unchanged since the preindustrial*" – having trouble with the 'but', do you mean 'and'??

R1.10. The emissions of methane by natural wetlands are estimated by the model, and therefore are not implicitly held constant. This appears clearly in equation (47) which is right above the sentence quoted by the referee. And this explains the use the word 'but'. We've changed the beginning of the sentence to: "*This equation implicitly assumes [...]*", pointing the reader to the equation above to make the sentence clearer.

C1.11. p.20 Nice job with the complex system that is N2O.

R1.11. Thank you.

C1.12. p.22 "*in this case the associated lifetime is set to a value of infinity*" This is a typical problem with using lifetimes instead of loss frequencies. I would have made all of the tau's into 1/tau = LF which can then be set to zero (inifinity is hard with a computer.)

R1.12. It is true that loss frequencies are a scientifically sounder way of writing these equations. But (unfortunately) using lifetimes is much more typical within the community; therefore we chose to stick to these. We've just slightly extended that sentence, adding: "*– or equivalently its loss frequency (ν^X = $1/\tau^X$) is set to zero.*"

Note however that the 'numpy' library of 'Python' language handles values of 'infinity'. And OSCAR is actually coded with such infinity values.

C1.13. p.23 "*at chemical equilibrium with*" – I think equilibrium is really abused by many of our colleagues, it is rather a steady-state that is reached. Equilibrium has deeper implications of detailed balance as in thermodynamics.

R1.13. Agreed! This is of course a steady-state. We've checked our usage of 'equilibrium' throughout the whole paper, and changed to steady-state when relevant. We kept the wording 'preindustrial equilibrium' however. Although it is debatable whether it was really an equilibrium, a steady-state, or none of the two, it is a trivial equilibrium of our model.

C1.14. "*with linear global sensitivities that are regionalized thanks to region-specific weights*" This is very nicely done since that are large regional differences in both the chemical and RF response to these short-lived emissions!

R1.14. Thank you. We are not entirely satisfied by the degree of detail of this regionalization: four regions is not much. But, just as we couldn't regionalize at all the OH sink, we were here limited by the availability of (exploitable) data.

C1.15. p.25 The param for O3s is reasonable, but I am not sure that I would agree with the N2O impact being reduced at high EESC (eqn 61). The N2O loss occurs in a very different region from either Cly losses (lower strat or 40+km for ClO+O) and should simply be just linear?

R1.15. Thanks to the referee, we have added a new option for in the model to have this

change linear. So we end up with three options: no N2O-O3 effect at all, the saturating effect following Daniel et al. (2010), and the new option of linear variation (with EESCx set to 'infinity'). Main text has been changed accordingly.

C1.16. p.26 I am worried about the formulation of eqns like 63 because if loss frequencies go to zero then Tau goes to infinity (and stated earlier) and this formula blows up. This should be written in the form like 1 / (1/tau + min.loss.freq)

R1.16. We understand the referee's concerns. However, we do not share them for two reasons. First, in this version of the model these lifetimes are constant. Second, if the loss frequencies go to zero, the lifetimes increase and the species are no longer short-lived. So our assumed steady-state would become wrong much sooner than the equation would 'blow up', and we would have to use a (time) differential equation instead.

C1.17. p.35 "*The differential system is solved with the forward Euler method (Euler, 1768) with a time-step (dt) that can be chosen before any simulation with OSCAR – although time-steps greater than a quarter of year systematically make the model diverge. This time-step is usually set to dt = 1/6 yr*". Since these equations do not appear to be very stiff (you can get away with 1/6 yr) you might want to use a Bulirsch-Stoer high accuracy integrator (not much cost) with a 1-yr overall time step (it divides the large step into a nested sequence of explicit steps and then extrapolates to give you an almost perfect answer. Note that B-S does not help with very stiff equations like integration O(1D) at sunrise.

R1.17. We thank the referee for this suggestion that we will keep in mind if in the future the model appears to need too many time-steps with the explicit method. But we are reluctant to change anything as to the numerical solving method we use: it is unclear what the benefits would be in the short term. On the precision side, it is very likely that the difference of result between two solving methods is much smaller than the difference between two random parameterizations; and so the uncertainty from the method is likely completely covered by that from the parameters. On the computation time side, although the Burlisch-Stoer method may be faster, we believe that in the current state of the model there are many ways to improve the speed of computation even more, starting with coding the core function of OSCAR in e.g. C or Fortran instead of Python. This work of optimizing the model is not a priority for us, however.

C1.17. "*more than 1043 potential combinations of parameters*" – how about 2**128 = 10**43 Are there really 128 independent parameters in Table 2?

R1.17. Yes, the 10**43 combinations are obtained by a simple product of the number of 'individual' parameters or parameter sets presented in table 3. And yes, there are that many independent choices possible for any simulation with OSCAR. Now, the question of whether these parameters are actually independent in the real world is a completely different one! We know for sure that it is not the case for some. In the discussion we give the example of the parameters related to the ocean dynamic which are different for the C-cycle and for the climate response (whereas they should be the same). However, there is no solution to that problem as it stems from the way OSCAR is built as an aggregate of small emulators!

C1.18. p.36 and elsewhere. "*The interannual variability of the land sink simulated by OSCAR in the offline case does not match that from*" – I am a bit confused as to how you implement interannual variability and climate modes in OSCAR. DO not volcanoes affect the C-cycle thru T? if not diffuse radiation.

R1.18. There is no variability in OSCAR (as now explained upfront in introduction). The variability in the offline simulations come only from the variability in the climatology (CRU data) used to force the model. This is explained in the introductory subsection of the results section, but we've now also added a sentence in the 'experimental setup' section to remind the reader of this: "*Note also that, because the climate data is based on observation, the offline simulation will show natural variability, albeit not as a feature of OSCAR but as one of the driving data.*"

C1.19. p.37ff – Nice discussion and example with CH4. p.38 – When comparing the 'online' to 'offline' CH4 simulations it might be useful to remind people that the difference between the two, because nominally these two designations describe computational differences rather than models not having feedbacks. Same for N2O in the following.

R1.19. Yes. We have also added a reminder of how the atmospheric growth rate in the offline setup is reconstructed: "*[the offline atmospheric growth rate] reconstructed as the balance between the concentration-driven sinks and the anthropogenic emissions normally driving OSCAR in online mode*".

C1.20. p.41 "*the lack of interannual variability in OSCAR*" OK, now it is clear, but a discussion of this could be upfront – maybe I missed it.

R1.20. Done. See **R1.3** and **R1.18**.

C1.21. p.43 "*These many degrees of freedom increase the odds of seeing a given simulation diverge, or at least depart unreasonably from the plausible range of results.*" I am not sure this is true. Unless the equations are chaotic (e.g., Lorenz N-cycle model, Liapunov, ...) then divergence would not seem possible in a linearized model.

R1.21. Yes, this is an abuse of the word 'diverge'. Removed.

C1.22. p.43 "*Also, coupling of the tropospheric and stratospheric chemistries would be an improvement, especially for ozone, as would a finer regionalization be. We note however that a tremendous amount of factorial simulations by complex chemistry transport models would be needed to make such an improvement.*" Since the goal is to accomplish the cross-coupling and feedbacks, the N2O-CH4 link (Prather & Hsu, 2010 Science) would seem to be important (+10 molec of N2O => -3.6 molec of CH4). Is this feedback actually included in the strat-trop chemistry of OSCAR – if so great, and note it.

R1.22. This coupling is indeed simulated by OSCAR, albeit not as a direct coupling.

The change in N2O does impact stratospheric O3 which in turn impacts the OH sink and then CH4. The coupling therefore appears as a succession of several arrows in figure 1.

C1.23. p. 43-44. Future or current improvements – I would vote for 2 as primary to simulating climate and to understanding the coupling across cycles. 1) Find a way to do ensembles with climate variability on interannual to decadal scales.

R1.23. As discussed in **R1.3**, we do believe it is a very interesting way to develop the model further. But it is likely a huge undertaking.

C1.24. 2) Do an eigenvalue analysis of your linearized matrix to identify time scales and the structure of the major coupled modes.

R1.24. This is a good suggestion. We are currently working on some theoretical analysis of the system/model, and an eigenvalue analysis is definitely have its place in such a study.

C1.25. 3) Add a table 4 that lists the processes and known couplings that are NOT included; this of course cannot be complete but at least some of the major areas that you chose not to include. Very useful as a reminder.

R1.25. Answered. See **R1.6**.

C1.26. 4) Describe how to go about updating the parameters when we have new results from the 'big' models.

R1.26. The creation of OSCAR took a lot of time in this respect, as we had to process a large amount of data from the complex models: more than 1 TB. And it was too often that the 'official' data format was not followed by a given modeling group, which makes the processing even longer. So we doubt there is an easy way to update OSCAR with the future new results from the complex models. Of course, we will follow the CMIP6 exercise, especially as more diverse data is supposed to be made available. For instance, if PFT-specific data is indeed provided for the land C-cycle, we will be

able to get rid of our fractional weighting and we will have a better estimate of land carbon densities and therefore of land-use change emissions.

C1.27. p.63 Great figure, thanks. Minor fix: 'edges' in the figure caption should read 'lines'

R1.27. Thanks. The word 'edges' was from the graph theory vocabulary, but that's probably not necessary.

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