

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

Anonymous Referee #1

Received and published: 22 September 2016

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. 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? 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. 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

C1

implemented, how would it change the analyses and ensembles?

Specific Comments:

OSCAR = "Earth system change" model." Excellent.

p.3 "but all other sectors provided by the inventories are accounted for" – does this include aviation and shipping. OK, 2.3.3 explains part of this.

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?

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.

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

p.18 "it is parameterized by three relative chemical sensitivities (-) and the preindustrial natural emissions of the three ozone precursors (ENOnat , EConat , EVOnat)" 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. _OH CH4, _OH O3s, _OH TA , _OH QA , _OH NOx, _OH CO, _OH VOC, _OH NOx, _OH CO and _OH VOC) 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.)

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'??

C2

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

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 (infinity is hard with a computer. . .)

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.

“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!

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?

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 $1 / (1/\tau + \text{min.loss.freq})$

p.35 “The differential system is solved with the forward Euler method (Euler, 1768) with a time-step (Δt) 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 $\Delta t = 1/6 \text{ yr}$ ”. Since these equations do not appear to be very stiff (you can get away with $1/6 \text{ yr}$) 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.

C3

“more than 1043 potential combinations of parameters” – how about $2^{**128} = 10^{**43}$
Are there really 128 independent parameters in Table 2?

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.

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.

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.

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.

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.

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 en-

C4

sembles with climate variability on interannual to decadal scales. 2) Do an eigenvalue analysis of your linearized matrix to identify time scales and the structure of the major coupled modes. 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. 4) Describe how to go about updating the parameters when we have new results from the 'big' models.

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

Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2016-149, 2016.