Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2019-282-RC1, 2020

Geoscientific Model Development Discussions

Interactive comment on "On the increased climate sensitivity in the EC-Earth model from CMIP5 to CMIP6" *by* Klaus Wyser et al.

Anonymous Referee #1

Received and published: 29 January 2020

In the paper the authors discuss the estimated Equilibrium Climate Sensitivity (ECS) from 11 or so simulations with different set ups of the EC-Earth model. They conclude that the 1K increase of ECS from CMIP5 to CMIP6 stems from the implementation of the first and second indirect aerosol effect. Increasing the vertical and horizontal resolution, including interactive vegetation, and new model tuning play a minor or no role.

The content of the paper is very relevant in the current discussion on reasons of higher ECS in many CMIP6 models and the simulations are well justified and documented in the tables. However, the paper seems pretty hastily written, references to the literature are missing, and the analysis is very superficial (global mean only with no discussion on actual reasons and local expression of differences), compared to any scientific paper

and also to papers documenting high sensitivity of other models (e.g. Gettleman et al. 2020 and Zelinka et al. 2020)

Major comments

1) The paper could be easily improved by describing the changes to the model code in terms which can be understood by non-aerosol specialists, e.g. somebody who might be interested model weighting and query your paper for understanding why EC-Earth model version X has a higher sensitivity that EC-Earth model version Y; which of the two to include in a certain assessment and how much weight to give each model version. Such a person should be able to take more away from your paper than "... [the change] is mainly caused by the change in the representation of aerosols and their impact on clouds and radiation" (line 179), isn't it? It would help to flesh out line 65-88 (not necessarily in length but in readability for non-experts).

2) I my eyes, a main conclusion of the paper is "This series of sensitivity experiments suggests that the increase of the ECS from CMIP5 to CMIP6 is mainly caused by the change in the representation of aerosol and their impacts on clouds and radiation. The implementation of MACv2-SP as it is suggested for CMIP6 models without explicit aerosol scheme has fundamentally changed the way how aerosols are prescribed in the model, yet this change has little effect on the ECS as long as cloud effective radius and autoconversion are independent of the aerosol concentration. The ECS increases when the more advanced treatment of the first and second indirect effect is introduced, with the largest contribution coming from the latter" (line 177-184 or so)

The paper would profit from putting actual numbers behind these claims, which are now only expressed in Table 1 and 2 (the only actual results of the paper). Simply showing the relative contributions locally, some spatial fields of how the second indirect effect is expressed, and the different contributions (short and long wave, clear and full sky) to Delta Q net (as in Fig. 1), which is standard in climate sensitivity papers, e.g. Andrews et al. 2012 or 2015, or Zelinka et al. 2020 "Causes of higher climate

sensitivity in CMIP6 models" (which goes much deeper in the discussion but does not replace papers like this one). However, there should be more depth than "it's likely the second indirect effect".

Why? Where? How trustworthy is it (which model versions should I use for which purpose)? Why did you include it into the model? What do we learn from it about climate sensitivity and projections of warming (and precipitation) in the next century? With the men power of six co-authors you should be able to go into a little bit more depth (e.g. compare Gettleman 2019 "High Climate Sensitivity in the Community Earth System Model Version 2 (CESM2)".

3) The paper discusses changes in ECS of 1K and smaller changes between the model versions. There should be uncertainty estimates for all ECS estimate and a discussion in how far differences are even statistically detectable (I'm guessing the difference in line 155 between 4.3 and 4.2K is not even significant (as you also argue, but you should show it)).

Minor comments

line 21: delete "easily" (of course this can't be generalized to any other model (?)) line 27: what's "the climate change context" (?) line 30: ... for the warming. I guess in the next century? Are there more recent papers on this discussion? line 34: "was found" and "has been found" sounds as if these models fell from the sky onto your desk. The models were made more sensitivity (by people like you, changing the model), this is active human action and doesn't passively happen. line 40: "An important question is if we understand the reason for this increase" This statement is very sadly showing the level of climate science these days. I hope you are able to understand the reason for this increase and both the user and the wider public want to know it. If you don't understand the reason, why should I trust you, your model, the IPCC assessment using the model, ... ? line 43: "Unfortunately the complex nature of the model development process makes it impossible to turn back the development in a system-

atic and continuous approach." What about starting systematically in the first place (!?) *You* developed the model and made these decisions only a few years or even months ago. I know this is a hard process, but the necessity for careful documentation has been openly discussed now for several years (e.g. Hourdin et al., 2016, "The art and science of climate model tuning.", but also already Mauritsen et al. 2012 "Tuning the climate of a global model"). You at least have to blame yourself (or colleagues) for following this approach and not take it as passively given. line 56: This is the last time I'm making that comment: "The EC-Earth global climate model has evolved from ..." You did evolve the model (actively) and should be able to confidently explain me why things change, isn't it? line 71: Explain how (what is in Martin 1994). line 71-73: More explanation and depth is necessary for a reader not familiar with the model or aerosols in general. line 75: Again more explanation necessary. How are these plumes prescribed? Do they change in time in the historical but not in the step forcing (the same way as in the control simulation)? A non-aerosol expert would profit here from some plots visualizing the changes in the model. line 79: the *direct* aerosol effect? What's the background aerosol mass concentration? line 81: shortly explain effective radius, auto conversion-efficiency, activation scheme in laymen's terms line 85: ... are accounted for by multiplying the resulting cloud droplet number ... I couldn't figure out to what "resulting" refers to. Resulting from what? line 91: What's the baseline CO2 concentration? Is it the same in all models in CMIP6? I think that wasn't the case for CMIP5? How did you deal with this in your model development? line 126: No that's not a basic assumption of the Gregory method. It is very common to detrend models, e.g. Proistosescu and Huybers 2017 or Andrews et al. 2015. (rest of the section is fine though) line 139: In how far does discarding the first five years change the results? See comments on uncertainty above. line 169: Does the lower value refer to the fixed vegetation version? line 181: as long as cloud *droplet(?)* effective radius ... line 184: What's the reason for the correlation in the Kiel 2007 paper? Are there any updates of the discussion in the literature? line 183: This is great that the tuning is discussed! line 189: add maps of these quantities or short wave cloud radiative effects or other feed-

backs here (?) line 205: delete “strictly speaking” (or explain how “loosely speaking” your results are valid for any other model (of course they are not (?))) line 221: “All co-authors have participated in the analysis of the results” I see that the model development and running is hard work but I don’t see how the analysis takes six people as only single ECS numbers are produced which is a straightforward and standard task. There are no actual analyses done in the paper. line 298: give panels names a, b, c line 310: Express resolution also in approximate km units

References to statements missing throughout the text (e.g. line 62, whole section 2.3, especially line 110, line 113, line 124, and other places)

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2019-282, 2019.