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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.

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Author comments to referees #1 and #2

We'd like to thank the two anonymous reviewers for many helpful comments and suggestions that have helped us to improve the manuscript. Both reviewers acknowledge that the climate sensitivity of CMIP6 models is an important topic, and that it is important to understanding the differences to the models from CMIP5. The reviewers agree in their criticism that the submitted manuscript was incomplete and lacked much of the essential analysis. We have tried to address the reviewers' point by substantially extending the analysis and not only look at the ECS but also at changes in clouds and the cloud radiative forcing, and discuss the impact of these changes to explain the change in the ECS in the CMIP5 and CMIP6 version of the EC-Earth model. The analysis is now also covering regional aspects and not only global means. We hope the extension of the analysis pleases the reviewers and makes the message of the study more clear. Both reviewers also mention the lack of references to recent papers that discuss climate sensitivity in other CMIP6 models. We agree on that point and have added references to other studies where suitable. However, we'd also like to make a point that both reviewers have given examples of missing references and cite papers that have been published several months after our manuscript was submitted (end of September 2019), and we therefore were not aware of these references (e.g. the excellent paper by Zelinka et al.) We would like to emphasize that our submitted manuscript isn't intended to replace the full documentation of the EC-Earth3 model. The details of the changes when going from the CMIP5 to the CMIP6 version of the model, the model tuning, and changes in the model climate will be documented in a model reference paper. Thus, we have decided to not dive deeper into explicit details of individual steps in the model development, but keep the discussion at a general level and focus on the impact of the developments on the ECS instead.

Detailed replies to the comments from reviewer 1:

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 ECEarth 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). We have reworked the model description (2.1) and describe the differences between ECE2 and ECE3 clearer. As stated above, it's not the intention that this study replaces a detailed model description, we try to keep the description at a general level.

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 C2 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)”. We have extended the analysis and now include also discuss the differences in ERF and feedback parameter between between ECE2 and ECE3. We also look at the differences in the sensitivity of clouds and the cloud forcing in the two model versions, and show that the largest difference are found over the Northern Hemisphere Atlantic and Pacific Ocean.

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)). We have added uncertainty estimates to all numbers in the table with estimates given as standard deviation of the parameter estimates in the linear regression of the Gregory method.

Minor comments line 21: delete "easily" (of course this can't be generalized to any other model (?)) Agree.

Line 27: what's "the climate change context" (?) Replaced with "...widely used metric in climate modeling..."

line 30: . . . for the warming. I guess in the next century? Are there more recent papers on this discussion? No, the warming in this context is not bound to a fixed point in time (e.g. end of the century). The idea is that the equilibrium temperature at any point in time is determined by the cumulative emission up to this point. To our knowledge, Matthews et al were the first to describe this and we therefore think it makes sense to cite their work. Since then many papers have appeared in the literature that look at the relationship between cumulative emissions and temperature change (e.g. Frölicher 2016, Seshadri 2017, Miller and Friedlingstein 2018, Matthews et al 2018), but we consider the discussion of this topic to be outside the scope of our work and therefore don't add more references.

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. We haven't tuned the ECS in the EC-Earth model and get its value first after having done some of the CMIP6 simulations. So yes, for us it "fell from the sky". The same is probably true for other modeling groups too, otherwise there wouldn't have been so much discussion about higher ECS in the new climate models about a year ago at the CMIP6 analysis workshop in Barcelona (which among other things resulted in the Carbonbrief guest post: https://www.carbonbrief.org/guest-post-why-results-from-the-next-generation-of-climate-models-matter)

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, . . . ? This sentence has been removed.

line 43: “Unfortunately the complex nature of the model development process makes it impossible to turn back the development in a system- C3 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. The EC-Earth model is developed by a broad and heterogeneous consortium. Some developments can be easily reverted, others not. Sometimes it’s just not possible (or very difficult) to maintain different versions of process descriptions in the same model. This has nothing to do with model tuning.

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? This sentence was there to document the legacy of the EC-Earth model, not to explain any of its characteristics. Nevertheless we have removed this sentence as it’s not necessary for the ECS.

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 We have added a short sentence about the Martin et al parameterization of the effective radius. It would be beyond the scope of this study to go into the technical details of this – or other – parameterisations, the interested reader can easily find the formulae in the cited references.

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. The aerosol concentrations do not change in time, neither in the pre-industrial control nor in the 4xCO2 simulations. We have added a statement that the aerosol concentrations don't change over time (l.89f in the revised manuscript)

line 79: the *direct* aerosol effect? What's the background aerosol mass concentration? The background aerosol concentration is from the off-line run with TM5, this is described in l.85ff

line 81: shortly explain effective radius, auto conversion-efficiency, activation scheme in laymen's terms We don't think it is necessary to explain these basic concepts of coud microphysic and radiative transfer in a paper about climate model sensitivity.

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? Yes, the prescribed CO2 concentration is the same in all concentration driven models, this has been the case for CMIP5 and it still is for CMIP6.

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) Agree, the detrending is widely used and we have removed this statement.

line 139: In how far does discarding the first five years change the results? See comments on uncertainty above. The ECS estimates are changing a lot when the first few years are included in the regression, but we don't find any difference if we exclude 5 or 10 years. We have added a sentence about that (l.148)

line 169: Does the lower value refer to the fixed vegetation version? The paragraph has been reworded.

line 181: as long as cloud *droplet(?)* effective radius . . . Added droplet.

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 Kiehl et al argue that tuning change the ECS of a model, and we are not opposing their finding. However, our sensitivity experiments show that the ECS can also change without changing the tuning of the model, and this is the point we like to make here because we heard the argument in the discussion that higher ECS is just the result from the tuning.

line 189: add maps of these quantities or short wave cloud radiative effects or other feed- C4 backs here (?) Figs 2+3 have been added

line 205: delete "strictly speaking" (or explain how "loosely speaking" your results are valid for any other model (of course they are not (?))) This sentence has been changed according to the reviewer's suggestion.

line 310: Express resolution also in approximate km units Done.

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) Sorry, but we don't understand what the reviewer is asking here? For example l.110 reads "concentration until it reaches equilibrium. However, the brute force approach to run the model until equilibrium is not very", what statement on that line would require a reference?

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