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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.

Klaus Wyser et al.

klaus.wyser@smhi.se

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"On the increased climate sensitivity in the EC-Earth model from CMIP5 to CMIP6" by Klaus Wyser et al. 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 2:

Major comments: The authors present climate sensitivity estimates from a series of simulations including all or subsets of the aerosol related changes between the CMIP5 and CMIP6 versions. Simulation results indicate that as well direct aerosol effects from the new climatology as the inclusion of different indirect aerosol effects contribute to the higher climate sensitivity. However, no attempt is made to understand why and how these effects come about, which I think would be crucial to make this paper useful for readers beyond the notion that differences in climate sensitivities simulated by different EC-Earth version may be related to the treatment of aerosols. There should be an attempt to attribute the changes to different types of climate feedbacks, as well as to identify possible regional patterns and mechanisms that affect the feedbacks. One

could e.g. imagine that the modifications affect ECS rather indirectly, e.g. by modifying the climatological cloud distribution which then reacts differently to an increased GHG concentration. It would be important to figure these things out. We address the concerns with an extended analysis of the differences between the CMIP5 and CMIP6 version of the model by looking at the response of clouds and the cloud forcing, see new Section 3.1.

There have been other publications on increased or tuned climate sensitivities in CMIP6 models (at least Andrews et al., JAMES, 2019, Gettelman et al., GRL, 2019; Mauritsen et al., JAMES, 2019; Zelinka et al., GRL, 2020, but there may be more I'm not aware of). I find it surprising that the authors ignore all of these publications. Their own work needs to be put into the context of these earlier studies. Indeed, there are many publications about changes in the climate sensitivity of CMIP6 models. We made an attempt to update references in the paper and put our results in a wider context.

To estimate the climate sensitivities in different model configurations the authors deviate from the common approach of branching a simulation with instantaneously increased CO2 concentration from a control run with a climate in equilibrium. Instead, the authors start control runs with modified configurations at the same time as the runs with increased CO2 and use anomalies of the latter with respect to the former. The sentence where this is described (L134) cites Andrews et al. (2012) which is a bit misleading as this reference is not appropriate for the use of anomalies. My hypothesis is that likely the use of anomalies is unproblematic, but If there is no reference confirming this I think the authors should show that, e.g. by using other common approaches as slab ocean models or +4K experiments which also don't require long spin-up runs. I'm a bit concerned about the applied method because of the strong initial (over five years or so) adjustment due to the change of configuration and the change of sign in the temperature trend afterwards. We are sorry for having put the wrong reference for the detrending of the results before evaluating the ECS, the correct reference is Andrews at el (2015) and this has corrected. We are aware that we deviate from the

standard protocol by not letting the model run to equilibrium in its different configurations before starting the 4xCO2 experiment. We explain all this in the manuscript and motivate it with the expensive computational resources that would be needed to run the model to equilibrium for each configuration. Please not also that this deviation from the CMIP6 protocol is only used for the sensitivity experiments in Sec 3.4, the piControl and 4xCO2 experiments for CMIP6 have been started from a properly spun-up state.

Minor comments: L60: The table doesn't show "basic differences" between the different EC-Earth versions but version numbers (and resolutions) of the subcomponents. Agree, changed in the text and table caption. L89: I don't like the use of "tas" for near surface air temperature. I know it is a CMIP variable name, but it looks odd in a written text, isn't used for the global mean in CMIP, and is inconsistent with other names (Qnet). We prefer to use CMIP variable names when possible. There are no CMIP names for net radiation or cloud forcing and for these variables we use Qnet and CF.

L100 "most important updates are likely those related to the revised aerosols". I guess "most important" is meant in the sense of ECS. Why is that likely? Other authors have e.g. documented that also tuning of model physics may affect ECS strongly. Can this be excluded a priori. No, tuning may also play a role as suggested by Kiehl (2007), this is mentioned at the end of Sec 3.4. However, here we show that for the ECE3 model we can get back the ECS from ECE2 by switching of the updates in the aerosol-cloud interaction that were not present in ECE2. In all the sensitivity experiments we don't change the model tuning, it only is an effect of having the indirect aerosols effect switched on or off. We also try to be clear that this is only true for the EC-Earth model, we cannot say anything about if this would have the same effect in other models.

L117 "Since models may present a not perfectly closed energy balance : : :" Is that the case for EC-Earth? Many models can experience a small drift, or long-term climate variability in long runs such as piControl. In the CMIP6 version of the EC-Earth model the decadal or even multi-decadal variability is surprisingly strong for reasons yet unknown.

L123 “Therefore we divide : : :” This is common practice. Yes, but still it has to be mentioned here to explain how we get from the 4xCO2 experiment to a value that corresponds to a 2xCO2 experiment.

L126 Why is a “well-tuned” model a basic assumption of the Gregory method? And how can good tuning be characterized? Indeed, “well-tuned” is not necessary for the Gregory method. We have updated the text accordingly.

L134 The new control experiments are no piControl experiments, which are supposed to start after a spin-up. I’d suggest to name them differently. Agree, we now only use piControl where we refer to the proper CMIP6 (and CMIP5) piControl experiment.

L185 The authors speak of “subsequent tuning to match a realistic preindustrial equilibrium and present-day climate”. This sounds like the model’s climate sensitivity was tuned? Or is this just a misunderstanding? We tuned the model with the goal to get a stable pre-industrial climate and a present-day climate close to observations. We did not tune the climate sensitivity explicitely. However, when the new treatment of aerosols was introduced we had to re-tune the model to again come close to our tuning goals.

L193 I don’t find it easy to understand why a change in complexity would have the “potential to modify the sensitivity” beyond the fact that any model change has this potential. I would also like to see an explanation for the statement in the following sentence. Would the assumption be that the addition of an indirect effect would lead overall to a larger aerosol forcing and the attempt to compensate for that by tuning the model to a higher climate sensitivity to obtain a better fit to the historical temperature trend? The reviewer is correct, any model change can change the sensitivity and it’s not a priori given that the sensitivity increases in a more complex model. We therefore have removed this paragraph.

Code and Data availability: I don’t know the exact policy of GMD. But usually these days journals require the availability of primary data, which to my understanding is the model code and the scripts and input files needed to run the model, not only for editors and

reviewers. Unfortunately we cannot freely distribute the EC-Earth code but are bound by license agreements with the ECMWF (for the IFS code). GMD has accepted this restriction and allows the distribution of the code only to editor and reviewers. Hopefully this restrictive policy of ECMWF will change with the next version of EC-Earth that will be based on OpenIFS and can be distributed more freely.

Table 2: The “experiment” column should contain more information. E.g. it would be nice to be able to identify quickly which experiment in table 3 belong to which in table 2. Tables have been reorganized and split, and should be easier to read now.

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