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Interactive comment

Interactive comment on "Effective radiative forcing and adjustments in CMIP6 models" *by* Christopher J. Smith et al.

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Anonymous Referee #1:

General Comments:

The paper presents a comprehensive analysis of the diagnosed values of effective radiative forcing (ERF) for the CMIP6 models, and breaks down the contributions of this forcing from greenhouse gases, aerosols, and land-use. The use of ERF has continued to grow and it is now at least as widely-used, if not more so, than traditional metrics of forcing such as instantaneous forcing or stratospheric-adjusted forcing. It is an important paper for benchmarking the performance of CMIP6 models, and its findings will hopefully be used in upcoming assessment reports. That being said, there

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are many different findings in this paper and it would likely be more digestible if the findings were explored in more detail in separate papers. This is something of an omnibus paper. However, despite its size, the paper has important findings and only requires minor revisions before being acceptable for publication.

» Thank you for your positive comments on the paper. We agree that it is a long paper and we appreciate your time, energy and effort given to the thorough review.

» It is always a consideraton whether to split papers such as this into more than one volume. We were of the belief that just reporting the headline fixed-SST ERF results from the CMIP6 ensemble "as-is" did not constitute enough analysis to merit a paper on its own, although these figures (table 2) will possibly be the most widely-used. I felt that Smith et al. (2018b) would have benefitted from the regional analysis, so I was keen to include it in this submission. It sounds like this motivated interesting questions from both reviewers, so was a useful addition, at the expense of a longer paper.

Specific Comments:

The last point made by the authors in the abstract, which appears to be supported in Figure 8, namely that they see no evidence that aerosols are contributing to the spread in ECS, is striking and bears more discussing. The range and change in ECS for CMIP6 is and should be of great concern to the large numbers of individuals involved in CMIP6 and to the scientific community as a whole, since an explanation is required. I hope that a body of literature will emerge (and quickly) to develop this explanation, and to the extent that this paper can contribute to that body, it is important that the lack of correlation between present-day aerosol forcing and ECS is promulgated. Is it fair to say, then, that the mystery of CMIP6 ECS persists or, perhaps, deepens?

» Thank you for picking out this key point which we could highlight more. The last sentence of the abstract has been extended:

» "Therefore, there is no evidence to suggest that the increasing spread in climate

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sensitivity in CMIP6 models, particularly related to high-sensitivity models, is a consequence of a stronger negative present-day aerosol forcing, and little evidence that modelling groups are systematically tuning climate sensitivity or aerosol forcing to recreate observed historical warming."

» From the model description papers cited in table 1, with two exceptions (MPI-ESM1-2 and EC-Earth3), there is either an explicit mention that historical temperatures were not used as a model performance indicator for their CMIP6 configuration, or the model paper was silent on this. Indeed it is evident from a number of models' historical temperature evolutions that they were not a target of model tuning:

- CanESM5 (high climate sensitivity, moderately low present-day aerosol forcing) shows more warming than observations over the historical period (Swart et al., 2019);

- NorESM2-LM and NorESM2-MM (lower Gregory sensitivity, stronger aerosol forcing) shows less warming than observed (Seland et al., 2020). (The NorESM2 models actually have high equilibrium sensitivity but low sensitivity as measured from a 150-year Gregory regression due to strongly increasing feedbacks over time).

- UKESM1-0-LL (high sensitivity, about average aerosol forcing) has approximately the correct level of present-day warming but is too cool in the 1960-2000 period (Sellar et al., 2019).

» A footnote was added in section 5.3.2:

» MPI-ESM1-2 (Mauritsen et al., 2019) and EC-Earth3 (Wyser et al., 2019) are the only documented exceptions. MIROC6 (Tatebe et al., 2019) did tune the aerosol forcing to better correspond to the AR5 best estimate but explicitly did not tune for surface temperature.

» While the magnitude of year-2014 aerosol ERF may not constrain climate sensitivity, potentially the evolution of aerosol forcing since 1970 may provide some constraint on transient climate response. As different models include different aerosol processes, the time history of aerosol forcing can be quite different in different models even when driven with the same emissions. 6 models so far have performed the RFMIP Tier 2 aerosol forcing transient experiment and future work will investigate this.

The paper notes that the spread in ERF between models is narrowed relative to CMIP5. This is a most welcome finding, given the poor specification of forcings in CMIP5. I recommend that the paper indicate that the result is consistent with a high-level recommendations from the Stouffer et al, 2017 paper (doi: 10.1175/BAMS-D-15-00013.1).

» Thank you for the suggestion. In the Conclusion (line 491) we add a sentence:

» "This has helped to address a concern from CMIP5: that forcing was poorly characterised in CMIP5 models and inconsistently determined (Stouffer et al., 2017)."

» We should caveat, and we discuss later in the paragraph, that there are more models in CMIP6 that did not submit the RFMIP Tier 1 experiments in which aerosol forcing is probably stronger than the lower bound from the 17 models for which we have data:

» "Although 17 models is a reasonable sample size of the CMIP6 population, more models may submit forcing results to CMIP6 that would widen this range (and indeed, we would encourage modelling groups to do so). One example is E3SM which did not perform the RFMIP aerosol forcing experiment but where it would be likely that the 1850–2014 aerosol forcing would be more negative than -1.37 W m-2 (fig. 25 in Golaz et al., 2019)."

» This may also have been true in CMIP5 in which the sstClimAerosol experiment was from a subset of models.

The narrowing of the range in aerosol forcing is particularly notable and welcome. That being said, the authors should point out in the abstract the importance of the aerosol forcing adjustments and large range in model results, especially with respect to clouds. The finding is included in the paper already and is notable in that it highlights challenges for the scientific community that studies aerosol-cloud interactions.

» In combination with one of your comments further below, this sentence has been added to the abstract:

» "In most cases, the largest contributors to the spread in ERF is from the instantaneous radiative forcing (IRF) and from cloud responses, particularly aerosol-cloud interactions to aerosol forcing."

» Along with the previous comment, in order to more clearly illustrate comparisons with CMIP5 which were discussed in the text but not graphically compared in the first submission, a new Figure 5 has been added which detail these comparisons, clearly showing the increased 4xCO_2 forcing in CMIP6 and slightly reduced range of aerosol forcing.

The limited importance of land use for forcing is surprising, and the spatial patterns there appears to be strong, with some overlap with aerosol forcing. Is there cancellation or reinforcement for these effects?

» To attempt to explain this further, the change in aerosol optical depth in the nine models that output this diagnostic in the land-use experiment is shown in fig. S4. Aerosol-induced changes in some models are notable but not large. For example CanESM5 has the greatest increase in aerosol optical depth over the Northern Hemisphere land regions but is relatively weak in terms of land use forcing. The exception to this is for the models that include ice nucleation effects from biogenic aerosol which is coupled to the land surface scheme. This is clearly the case for NorESM2-LM, where the cloud adjustment dominates the forcing and results in a positive land-use change ERF, and is already documented in fig. S3. All of the other 13 models have a negative land use forcing, which are in line with an observationally-constrained estimate from CMIP5 models (Lejeune et al., 2020).

» As you assert, regionally land-use change is important. In regions experiencing lots of deforestation (North America, Western Eurasia and South America) albedo is increased causing a negative ERF (fig. 12). In these regions land-use forcing determines the multi-model mean ERF, however, while models agree on the negative land-use ERF, there is no model consensus on the net forcing (fig. 13). Compared to land-use change, aerosols cause a slightly weaker negative forcing in the deforested regions, which in the northern hemisphere is caused by an increase in SW reflectance (fig. 8). The aerosol effect will reduce downward SW at the surface and so reduce the effect of surface albedo changes caused by land-use change. The cause of the negative aerosol ERF in the South American deforested region is more complex and here there is no obvious non-linearity in the combined land-use and aerosol ERF.

» It should also be noted that land-use change does not only impact climate via radiative forcing and that the temperature impacts of other mechanisms are often larger and of opposite sign (e.g. Bright et al., 2017).

The final point made by the authors in the conclusion, which is that there is a need to constrain cloud responses to forcing since they contribute to the largest uncertainty in forcing, is well-taken but disturbing. Clouds appear to be not just a problem for feedbacks, as is widely accepted by the community and has motivated a sustained focus on constraining cloud feedbacks, but they are a problem for forcing as well. This point should also be in the abstract and discussed in the abstract.

» From tables 3, 4, 5, 7 and 8 it is apparent the largest contributions to the spread in ERF are from IRF and cloud adjustments. In addition to improving cloud processes, there is still some way to go in radiative transfer modelling. Work is in progress under RFMIP to do this, and we show in Figure 5 for 4xCO_2 that the inter-model spread is much reduced compared to CMIP5 which may be indicative of an improvement in model radiative transfer.

» In conclusion, added:

» "The instantaneous radiative forcing and cloud adjustments are generally the largest sources of inter-model spread in the forcing component in climate models. Since IRF is not directly calculated in this study, some of this spread may be from residuals in the kernel decomposition and the true spread in IRF may be smaller than reported here. One strand of RFMIP will include benchmarking of GCM radiative transfer against line-by-line codes. Radiative transfer is a well-grounded theoretical problem where the diversity in line-by-line codes is small (Pincus et al., 2015), so this component of inter-model diversity has a measurable yardstick for improvement."

However, the recommendation of the authors is vague and it is highly unclear to me how on how cloud responses can be constrained. Through process studies? Developing observational constraints? There are strikingly strong spatial patterns of ERF. Can some type of fingerprinting be used? The authors should indicate in the paper whether or not there even is a path forward for actually constraining these cloud responses or if the community needs to develop one before even being able to go down it to actually develop those constraints.

» We agree that the last sentence of the conclusion was vague in the first submission. We have added some suggestions at the end of the conclusion (following on from the passage above) and a final summary sentence that links back to the introduction.

» "Cloud responses are more difficult to constrain and exhibit a wide range of behaviour to both greenhouse gas and aerosol forcing. However, progress is beginning to be made. For greenhouse gas forcing, techniques from the climate feedback literature that have observational parallels, such as analysing cloud-controlling factors (Klein et al., 2017), can be applied to adjustments. Use of the ISCCP simulator diagnostics with the ISCCP cloud kernel, another method conceptualised by climate feedback investigations (Zelinka et al., 2012), allows cloud adjustments to be calculated directly facilitating better inter-model comparison. For aerosol forcing, observational methods exist to determine RFari and RFaci using satellite and reanalysis data (Bellouin et al., 2013; Bellouin et al., 2020a). Ultimately, reducing uncertainty in effective radiative forcing will reduce uncertainty in climate projections due to the central role of forcing in driving the Earth's global mean temperature response."

Minor points:

The x-axes on Figure 4 need fixing.

» Rotated x-axis labels to make clearer.

Figure 5 has lots of information but is confusing in there is concurrence between models in the spatial patterns of ERF but there appears to be little concurrence in some of the spatial patterns of adjustments and cloud contributions, even though when summed up, they are significant across models. This is even more the case for Figures 7 and 11, and some explanation of how this is achieved is needed for readers.

» You make a good point here. Forcing adjustments are in many cases robust in sign in the global mean change but less so spatially between models. This highlights the point that forcing and adjustments are best considered globally averaged quantities. At the end of section 5 (line 481) we have added the following:

» "For all forcings, but particularly for land-use, aerosol and total anthropogenic, many of the forcing and adjustment terms do not show robust signals regionally. This indicates that adjustments are best considered as global-mean quantities that affect the globally-resolved forcing-feedback framework (Eq (1))."

Line 386: Should be “equivalent” not “equalivent”

» Typo corrected - thank you.

» References used in response to reviewer #1 not in manuscript: Bright, R. M., Davin, E., O’Halloran, T., Pongratz, J., Zhao, K. and A. Cescatti, 2017: Local temperature response to land cover and management change driven by non-radiative processes. Nat. Climate Change, 7, 296-302, https://doi.org/10.1038/nclimate3250.

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