

Interactive comment on “Assessment of pre-industrial to present-day anthropogenic climate forcing in UKESM1” by Fiona M. O’Connor et al.

Anonymous Referee #3

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To the editor,

I think this work serves two main purposes, i.e., (1) documenting (and disentangling) the ERF from short-lived emissions, long-lived GHGs, land-use, and ODSs in UKESM1, and (2) indicating the relevance of ESM-interactions for estimating the anthropogenic forcing.

I think the work is solid, the subject is well explained, comparisons with earlier studies are included, and the information shared in figures and tables is appropriate.

Presenting estimates of ERF is important to understand the evolution of climate (modeled or observed). Models participating in CMIP6 were motivated to perform specific

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experiments in AerChemMIP and RFMIP, which would allow a better characterization of the forcings felt by ESMs, e.g., over the historical period. It is important that those ERF estimates are well-documented, as they facilitate the interpretation of the fully-coupled behaviour of models (and climate). I think this study achieves presenting ERF estimates for the UKESM1 model in a nice, coherent, and attractive way. In general, the study is well written and attractive to read.

In addition, as UKESM1 contains a relatively extensive description of atmospheric chemistry, aerosols, and aerosol-cloud processes, the interactions between different species and processes are included. This makes this model an interesting tool to estimate the ERF of various short-lived emissions or longer-lived GHGs.

I think this work is valuable and of sufficient quality to be published in ACP. However, I think the manuscript could be considerably improved. I have listed several points on which I think the manuscript should be changed.

I have grouped my comments in three categories, i.e., main remarks, technical remarks, and additional detailed remarks.

Main remarks

1. TABLE 3

Table 3 is very interesting and plays to my opinion a rather central role in this study. A lot of the values in the table are being referred to in the text. However, some parts of Table 3 are not discussed. This is the case for two large and two small groups of experiments : SO₂-BC-OC-Aer, NO_x-VOC-O₃, LU and anthro. For these groups, only the NET ERF is discussed, but not the individual contributions. I assume this is related to the fact that for those 4 groups another split than the one in Eqs. (6)-(8) is followed, which is probably assumed to be more appropriate.

To make the study more consistent, I would at least mention that the non-discussed values exist in the Table 3, and are given for comparison/completeness/consistency.

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Explicitly presenting the numbers from the other approach followed for some experiments in a table would be interesting. Now these numbers are only mentioned in the text.

2. EXPLANATION OF RADIATIVE FORCING

Although radiative forcing and effective radiative forcings are very useful concepts, it is not always so easy to explain them. I think the authors have done a nice effort in trying to explain it. However, an extra effort should be done to make it more precise, fully coherent, and more illustrative. The final link why it is a useful concept should be elaborated more. I list here below also some specific places where I think the description should be improved.

- page 2, line 61-63 : Including As a result ... : I do not think that "As a result" is appropriate, as I do not think there is a causal relationship.

- page 2, line 63-64 : RF is a framework : this is a rather vague explanation.

- page 4, line 115-117 : I think that the sentence on line 115-117 illustrates that ERF can be very different from IRF, however, it does not illustrate that it is better (as suggested in the former sentence). It might be an option to reverse the order of these sentences.

- page 4, line 113-115 : although it is correct what is written here, I think it should be explained better.

3. FEEDBACKS, INTERACTIONS, NON-LINEARITY, INDIRECT EFFECTS

I think that feedbacks, interactions and indirect effects are an important part of this paper. In the abstract, it is stated that "... by quantifying ..., it enables the role of various climate-chemistry-aerosol-cloud feedbacks to be quantified." However, despite this sentence in the abstract, not all feedbacks have been quantified (it would be a very large task to quantify them all). The fact that they are active in UKESM1, and impact therefore ERF estimates, is already an important step. I think the text will benefit from

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more precisely describing what is quantified, and what one is not able to quantify.

I do not ask the authors to do additional analysis, or perform more comparisons with a model which contains less interactions (e.g., HadGEM-GC3.1). For quite some of the forcings, the authors did a very nice job in unraveling several contributions. However, it would be nice if the authors could be more careful and precise in some of the general expressions about feedbacks and interactions.

I have in addition the following comments.

1) I think the authors should explain what according to them is the difference between a feedback and an interaction.

2) It is a pity that the studies of Morgenstern et al. [2019, in preparation] on ozone and O'Connor et al. [2019, submitted] on methane have not been published yet.

3) Some of the formulations, referring to feedbacks and interactions, are often rather vague (less firm than the abstract suggests). Examples of these are :

- page 15, line 354 : ... "likely" ... the effect of adjustments ... including O3 depletion and fast cloud adjustments

- page 15, line 359 : which "might" be due to an aerosol effect

4) the abstract stresses well the non-additivity (or non-linearity) of several ERF estimates. However, the study sometimes remains vague in reasons for it, and just uses expressions like "the non-linearity between the GHG", or "the internal mixing of aerosols". Other examples of not so clear usage of the concepts :

- page 2, line 36 : due to the inclusion of non-linear feedbacks and ES interactions : vague

- page 2, line 38 : By including feedbacks between GHGs (isn't it interactions?)

- page 2, line 38-39 : some of which act non-linearly

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4. UNCERTAINTIES

The ERF estimates in the manuscript are accompanied by an uncertainty range in large parts of the main text and in Table 3, but not in the abstract, the conclusions, or some parts of the main text. This should be made consistent.

Also, one should maybe say something about the real estimate of the uncertainty. There is something on aerosol ERF uncertainty on page 20, line 453-455, and maybe this can be expanded. In general, UKESM1 shows uncertainties of around 0.02 to 0.04 W/m² on global 30-year mean averages of ERF – it is the uncertainty on the ERF in the model. However, if one wants to see these numbers as best guesses for the the ERF as experienced by the Earth, then the uncertainty is probably much larger. Emission uncertainty, lack of process understanding, too low spatial resolution, biases in the mean state of UKESM1 and others factors might contribute to the uncertainty. It would be interesting if the authors could also shed some light on this.

5. VALIDATION OF THE MODEL

There is very little information on the validation of the model, except via references to Mulcahy et al. [2018, 2019] and Archibald et al. [2019]. There are a few paragraphs (e.g., page 20, line 454-457 : aerosol comparison; page 21, line 491-492 : AOD comparison), mentioning some comparisons with observations. We do not ask for a detailed comparison, but some qualitative main findings on the performance of the model might be mentioned, both related to distributions of forcing agents (aerosols, ozone, ...) as to the general behaviour of the model (mean model state).

Technical remarks

1. POSITIVE NUMBERS

Some positive numbers have a "+", some not. This should be made consistent.

2. REFERENCES TO THE PHYSICAL MODEL

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It might be relevant to explain or highlight the relevant differences between UKESM1 and HadGEM3-GC3.1 better (both models are very close and have both participated in CMIP6 I assume). In the text quite often comparisons with HadGEM3-GC3.1 are made, and it would illustrate the role of having more/less interactions in a coupled model. I would think that possible differences are related to fixed ozone, fixed oxidants for secondary aerosol formation, fixed methane profile, fixed CO₂ profile, ...

3. REFERENCE TO FORSTER ET AL. [2016] FOR STANDARD ERROR

The way to calculate the standard error is mentioned three times. It should be mentioned when the uncertainty is met for the first time (both in the text and in the tables). The standard error and its reference are now mentioned in :

- page 12, line 272 (Table 3).
- page 18, line 402 (related to CH₄) : where the 0.04 W m⁻² is the standard error following Forster et al. (2016) : should be mentioned once (the first time).
- page 31, line 760-761.

4. MULTI-ANNUAL MEAN / ANNUAL MEAN / MEAN

It is mentioned in the beginning of the manuscript that almost all values and maps will be 30-year averages. Some sentences and figure captions in the text treating those values or maps once more mention explicitly that the averages are "multi-annual", whereas other figures captions or sentences do not. Please describe it in a consistent ways. An option might be to describe it clearly at some point in the, and say that it is valid for all the remaining text. Examples of differences in the description :

- page 18, line 395 : multi-annual mean (while page 14, line 323 : global mean)
- page 24, line 562 : The multi-annual mean ...
- page 25, line 599 : on a global annual mean basis

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- page 29, line 699 : Global distributions of the multi-annual distributions

- page 33, line 795 : multi-annual global mean

5. WRITING OF LAND USE

Both "land-use" and "land use" are used in the text.

6. CONSISTENCY

- page 10, line 261-265 : why is a difference of 0.2 W/m² for GHG called "consistent", but for the aerosol mentioned "less then the estimate from HadGEM3-GC3.1"?

- page 22, line 502 : -0.12 W m⁻² (-0.4 to +0.1), whereas on page 10, line 267 : 2.3 (+1.7 to +3.0) [5-95%] W m⁻² : the position of Wm⁻² is different.

- page 22, line 515-519 : only here "(i)" and "(ii)" are used. I would not use it just for this single occasion

- page 23, line 536 : only here "&" is used. I would not use it just for this single occasion.

7. NAMING of SWcs, Lwcs, SWcre and LWcre COMPONENT OF ERF

There are different abbreviations used to express the same physical quantity. One should make these coherent. An example of this is the LW clear-sky component of ERF, which appears in different ways in the text, tables, and equations :

- page 11, Table 3 : LWcs

- page 10, Eq. 8 : ERFcs

- in the text : a CS LW component

- page 16, Fig. 4 : (a) clear sky SW, (c) clear sky LW, and (f) net (SW+LW) CRE : it sounds as something is missing after SW and LW

- page 31, line 762-770 : NETcs, LWcs, SWcre

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- page 32, Fig. 12, caption : SWcs, LWcs, SWcre, and LWcre

8. STATISTICAL SIGNIFICANCE OF DIFFERENCE BETWEEN IDENTICAL SIMULATIONS

Several experiments have been performed on different machines. Related to this I have the following remarks :

- Table 4 : I assume there are three piClim-SO₂ experiments, so one could show the difference R1-R2 but also R1-R3.

- Table 4 : Some differences are a bit large compared to their uncertainty for SO₂ : NET ERF, SW CRE, NET CRE. Is there an explanation for this?

- Table 4 : Why is everything 0 for the NTCF experiment?

9. NO_x EMISSIONS AND SO₂

Figure 11 : is the impact of NO_x emissions on AOD not mainly in regions with SO₂ emissions from volcanoes? Indonesia, west coast of South America, Etna region, existing ship-lanes in 1850 over North Atlantic? Might probably NO_x have then also a large effect if it is co-emitted with anthropogenic SO₂?

10. FORMULAS

The "." or "," at the end of formulas can be closer to the actual equations (currently there is a 1.5 cm large gap). There should not always be a ":" at the end of the text just before a formula. This depends on the context. One can also have a ",", or nothing.

11. REFERENCES TO SECTIONS

The references to specific sections are not always correct. Please check and correct them. A list of incorrect section references :

- page 8, line 192 : Sect 3.1 -> 4.1

- page 12, line 287 : Sect. 3.2 -> Sect 4.2

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- page 13, line 291 : Sect. 3.3 -> Sect 4.3
- page 13, line 296 : Sect. 3.4 -> Sect4.4
- page 14 ,line 307 : Sect. 2 -> Sect 3.
- page 25, line 602 : Sect. 3.2.3 -> Sect 4.2.3
- page 29, line 705 : Sect. 3.4.2 -> Sect 4.4.2

12. USE OF e.g.

There should be a "," before and after "e.g."

13. REFERENCE TO EQUATIONS

The way one refers to equations is not homogeneous in the text (sometimes capital letters, small letters, abbreviations, or no abbreviations). Please make it coherent. A list of the differences found in the text :

- page 4, line 111 : Eqn. 1
- page 10, line 251-252 : in equation (5)
- page 15, line 348 : following the equations (6)-(8) described above
- page 19, line 425 : using Equations (6)-(8)
- page 27, line 643 : From Eqns. (10) and (11)

14. USE OF THE WORD "FORCINGS"

I have the impression that the word "forcing" (outside the context of radiative forcing and effective radiative forcing) is possibly not used in a completely coherent way. I have listed below a few locations where it is used, and it seems not to always have the same meaning. I think the text should be more careful and precise in how and where it is used.

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- page 1, line 16 : climate forcings
- page 1, line 31 : a positive forcing due to ozone
- page 3, line 65 : various mechanisms, both anthropogenic and natural. Here I would have used the word forcing and not "mechanism".
- page 3, line 65 : ... use of RF ... it is often used inconsistently ... : is RF meant in the first part of the sentence, because that is well-defined (page 2, line 54-56)?
- page 3, line 67-69 : This sentence mentions three things, and it is not clear whether the reader should see a causal relationship between "the inconsistent calculation of forcing between drivers", and "the large differences in forcing between CMIP5 models". For the last part of the sentence, it is not clear whether the authors mean that models are very different or that the methods of estimating forcing can be very different.
- page 3, line 82 : the resulting RF : is not the same as meant by the definition on page 2 line 54-56.
- page 7, line 171 : in which SSTs and SI and all forcings
- page 33, line 782 : between the aerosol and O3 forcings
- page 35, line 840-845 : "forcing" used four times. Also "anthropogenic" forcings and "natural" forcings, whereas it is not so clear what to understand here under natural forcing. In the text there is a reference "As summarized above", but I think there was not a large focus on "natural forcings".

15. THE USE OF ABBREVIATIONS

The definition of an abbreviation is often given several times, whereas that should be limited to :

- defined once in the abstract,
- defined once in the conclusions,

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- defined once in the figure caption, and
- defined once in the main text (but not more).

Once a definition is given in the main text, it should not be defined again.

16. PD AND PI ARE ADJECTIVES

- page 1, line 21 : at the PD, or relative to the pre-industrial (PI) : PD and PI are both adjectives, and not substantives. So they cannot be used on their own. E.g., on page 1, line 15 it is used correctly as an adjective : "... a wide range of present-day (PD) anthropogenic climate forcings ...".

17. PD-PI DIFFERENCE

The way the ERF is described as a difference between TOA fluxes in PD and PI times, is not coherent in the text. Sometimes one finds a "PD forcing" type of expression, sometimes a "PD to PI forcing" type of expression, and sometimes neither PD nor PI are mentioned. Some examples of the varying usage are given below. It would be nice if the description could be more coherent. An option would be to write in the text that all values shown in text from that point onwards are always PD-PI differences, and then PD and PI should not be repeated every time.

- page 3, line 71 : between the pre-industrial (PI) and the present day (PD)
- page 3, line 76 : at the PD relative to the PI
- page 5, line 130 : the PI to PD effective radiative forcings (ERFs)
- page 5, line 132 and 133 : PD forcings will be quantified relative to the PI [in addition is "the" strange before PI which is just an adjective]
- page 5, line 139 : PD anthropogenic forcings relative to PI
- page 6, line 157 : To calculate the pre-industrial to present-day effective radiative forcings ... due to a PI-to-PD perturbation

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- page 10, line 260 : the global mean PD ERF [here without any reference to PI]
- page 12, line 271 : PD effective radiative forcings (ERFs) of climate relative to PI
- page 12, Fig. 2 : y axis PI-to-PD ERF
- page 12, line 280-281 : PD ERFs from changes in ... since PI
- page 12, line 281 : PD ERF from GHG [but no reference to PI]
- page 13, line 303-304 : effective radiative forcing (ERF) relative to the pre-industrial
- page 14, line 319 : cloud radiative effect (CRE) at the present day [no reference to PI]
- page 15, line 338 : this forcing is small for the PD [no reference to PI]
- page 15, line 347 : due to changes in N₂O from PI to PD
- page 16, line 370 : for the present relative to the pre-industrial
- page 18, line 400 : resulting in a PD ERF [no reference to PI]
- page 19, line 423 : of the PD methane ERF relative to PI
- page 19, line 430 : This PD GHG ERF
- page 20, line 462-463 : of PI to PD aerosol ERF
- page 21, line 472 (Fig. 7) : Aerosol ERF at TOA ... [no PD or PI mentioned]
- page 22, line 501 : The PD OC ERF relative to PI
- page 23, line 551 : The global mean ERF from ... [no PD or PI mentioned]
- page 25, line 591 : the PI-to-PD change in tropospheric O₃
- page 27, line 651-652 : the tropospheric O₃ RF between PI and PD
- page 28, line 679 : and the PD aerosol ERF
- page 30, line 723 : ERF from PI-to-PD changes

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- page 31, line 744 : the PD ERF from piClim-O3

- page 33, line 795 : of changes (PD-PI) in

18. TIMESLICE EXPERIMENTS

Maybe it would be nice to better define what a timeslice experiment is. I would possibly describe it as an experiment with fixed boundary conditions (possibly having seasonal cycles), which one runs for several years to reduce the noise-to-signal ratio (the noise is caused by inter-annual variability). The longer one runs, the better estimate for the mean one can obtain. The locations where it is used are :

- page 6, line 158 : maybe explain timeslice here (forcings are kept constant)

- page 7, line 171 : time slice

- page 15, line 604 : timeslices : in ACCMIP they were different from here ...

19. FIGURE 4

Fig. 4, panel (a) : Why is there only some spatial variability north of 30N?

20. CONSISTENCY OF THE FIGURES

The figures and their captions should be more coherent throughout the manuscript. Below I list some places where improvements should be made.

- page 16, Fig. 4 : at the top of the atmosphere (TOA) : this is however not mentioned in Fig. 3.

- page 17, Fig. 5 : why mentioning "annual mean" or "multi-annual mean", whereas it is not mentioned in Fig. 4.

- page 19, Fig. 6 : it would be nice to have the global mean values given in the figure heading.

- page 19, Fig. 6 : mentions "according to Ghan (2013)". Why not in Fig. 4?

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- units in figures : Fig 7 uses "(W/m²)", whereas other figures use "/ W m⁻²".

- page 24, Fig. 9 : of the "multi-annual" distributions, whereas other figures do not mention "mult-annual".

- page 26, Fig. 10 : it would be nice to add the global mean RF.

- page 32, Fig. 12 : it would be nice to add the global mean values

Additional detailed remarks

ABSTRACT :

- page 1, line 23 and 28 : "larger than the sum of the individual GHG ERFs" and "less than the sum of the individual speciated aerosol ERFs" : I don't know whether these aspects should be mentioned in the abstract.

- page 1, line 15 : "In this paper" : I would not mention "In this paper" in the abstract.

- page 1, line 18-19 : by quantifying ..., it enables ... : this sentence seems not completely coherent. There is also twice "quantify" in the same sentence (in addition there was already "quantify" on line 15).

- page 1, line 21-22 : I would put the numbers at the end of the sentence, and "carbon dioxide, nitrous oxide, ..." at the beginning of the sentence. Now, one first reads numbers, but one does not know what their meaning is.

- page 1, line 19 : by this sentence, one suggests that climate feedbacks can be quantified by fixed-SST simulations. However, some of them are strongly suppressed in fixed-SST simulations.

- page 1, line 25-26 : is the "BC absorption" not part of the "instantaneous forcing from aerosol-radiation interactions"?

- page 1, line 27 : mean -> "imply" or "cause".

INTRODUCTION

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- page 2, line 42-45 : necessary ... detailed ... all aspects : this is probably exaggerated. I suggest to formulate it differently.
- page 2, line 42 : attribute it and its impacts : it (refers to climate change) and its impacts. It is not clear whether the authors mean something different with "climate change" and "impacts".
- page 2, line 44 : climate response and its impacts : (same comment as above).
- page 2, line 44 : a key mechanism : I would not call CMIP6 a "mechanism". Also "key" is possibly exaggerated - other initiatives (if CMIP would not have existed) might have also had good outcome.
- page 2, line 45-46 : which designs and distributes data : "designs data" sounds strange. Possibly one could say that "experiments are designed".
- page 2, line 47 : "these important climate science questions" : it is not clear which questions one refers to.
- page 2, line 50 : quantifying changes to the Earth's radiation budget, often termed radiative forcing : maybe radiative forcing needs a bit more explanation.
- page 2, line 64 : It's been -> It has been.
- page 3, line 65 : the strength of the various mechanisms : of various mechanisms
- page 3, line 65 : mechanisms, both anthropogenic and natural. I do not think that "mechanism" is the most appropriate word to be used here, especially in the context of "anthropogenic" and "natural".
- page 3, line 70-71 : is typically based on .. and using -> uses.
- page 3, line 72 : I suggest to put "e.g." before "based on Myhre et al. (1998) and Ramaswamy et al. (2001)", as there might exist other expressions.
- page 3, line 75 : Skeie et al. [2011] : I think that study is not so much about

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observational-based estimates of forcing. Is this paper very relevant in the discussion of forcing strength of GHGs?

- page 3, line 81 : including -> therefore allowing.
- page 3, line 82 : meaning -> implying.
- page 3, line 82 : doesn't -> does not.
- page 3, line 84-85 : of a robust ... constraint -> of robust ... constraints.
- page 3, line 84 and 89 : twice "additional uncertainties".
- page 3, line 85-86 : across multi-model ensemble : is that really what the authors want to stress? Might "across models" be sufficient?
- page 3, line 87 : chemistry models : does one mean CTMs or CCMs?
- page 3, line 91-93 : three times the word "uncertainty" in one sentence. Maybe it can be reduced to two.
- page 3, line 91-93 : it looks like aerosols get only very limited text attributed.
- page 4, line 96 : is "schematic" the correct wording?
- page 4, line 101 : Although -> Because/As.
- page 4, line 102 : maybe "also" can be skipped. I don't know if it really reflects well the meaning of the sentence.
- page 4, line 109 : andAi -> and Ai (blanco needed).
- page 4, line 110 : or over the land : vague.
- page 4, line 112-113 : but global mean surface temperatures or global ocean conditions remain unchanged. However, in reality with fixed-SST simulations, land surface temperature (and thus global mean surface temperatures) can still change a bit.

C16

- page 4, line 113 : error -> uncertainty (I assume the authors mean "uncertainty").
- page 5, line 129 : I would skip "including" because the list mentioned seems rather complete.
- page 5, line 130 : forcings from anthropogenic drivers -> forcing from anthropogenic drivers.
- page 5, line 131 : with a fully coupled : the "with" gives the impression that one uses the model here in its "fully-coupled" configuration. But here it is not used in its fully-coupled configuration.

SECTION 2

- page 5, line 142 : "is" the atmospheric and land components -> consists of.
- page 5, line 150 : "is determined by prescribed oxidant fields" : maybe describe differently, as oxidants are not the only determining factor.

SECTION 3

- page 7, table 1 : piClim-VOC : add that also CO is perturbed.
- page 7, line 166-167 : twice ERF : I think mentioning "fixed-SST" is enough to describe the experiments. Obtaining the ERF is the result of such an experiment.
- page 7, line 171 : Effectively, this involves ... : it is a bit a strange way to mention that also a reference simulation is needed.
- page 7, line 171 : SSTs and SI and all forcings : one should not have two "and"s in a row.
- page 7, line 171 : the abbreviations SST and SI have not been defined.
- page 7, line 173-176 : One uses two different expressions, i.e., "monthly time-varying climatologies derived from 30 years of output" and "30-year monthly mean climatologies", to describe the same thing.

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- page 7, line 177-184 : is it not in agreement with RFMIP and AerChemMIP?
- page 8, line 194 : emissions and/or GHG concentrations : I think this can just be "and".
- page 8, line 199 : fixed SST ERF experiments : I think fixed-SST is enough to describe the experiments.
- page 8, line 200 : ammonium nitrate : but other forms of nitrate are probably also not present (e.g., nitrate on dust and seasalt).
- page 8, line 208 : fixed SST timeslice ERF experiments : I think fixed-SST is enough to characterize the experiments.
- page 8, line 210 : This makes ... to changes in platform that cannot guarantee bit-reproducible results : this can probably be expressed more precisely.
- page 8, line 211-213 : was scientifically consistent with each other : one should be more clear in what is meant by "scientifically consistent".
- page 8, line 220 : Further to this -> in addition to this.

SECTION 4

- page 9, line 234 : Cloud-Radiative Effect (CRE): in the rest of the text, no capital letters are used when defining an abbreviation.
- page 10, line 240 : either ... and/or : I do not think that it is common to combine "either" with "and/or".
- page 10, line 241 : what is meant by "dynamical feedbacks" : changing meteorology which changes lifetime and thus burden of aerosols?
- page 10, line 251-252 : and any non-aerosol changes in CS flux : is, e.g., the deposition on snow of BC included in this term?
- page 10, line 252 : "The effective radiative forcing (ERF), clear-sky CS), and cloud

C18

radiative (CRE) contributions" : as I assume that "contributions" also relates to "clear-sky", I would write : "The effective radiative forcing (ERF), and its clear-sky CS) and cloud radiative (CRE) contributions".

- page 10, line 257-258 : following equations (6) to (8) : maybe only (8)? (As that is the final split which is presented in Table 3).

- page 10, line 262 : HadGEM3 GC3.1 -> HadGEM3-GC3.1

- page 12, line 271 : effective radiative forcings (ERFs) of climate : I do not think "of climate" is needed here.

- page 12, line 273 : use Realisations 2. -> use realisation 2.

- page 12, Fig. 2, caption : "diagnosed from paired fixed SST timeslice simulations with an atmosphere-only configuration of UKESM1". It is not clear why the fact that paired simulations are needed to estimate ERFs is mentioned here. It is, e.g., not mentioned in Table 3 (although also paired simulations are the bases for the results of Table 3).

- page 12, line 283-284 : ERF from the piClim-HC perturbation experiment ... positive forcing from the other LLGHGs : in the first part of the sentence one talks about the ERF from experiments, and in the second part about the ERF of physical things (in this case LLGHGs). The sentence should be improved.

- page 12, line 287 : The aerosol forcing is ... due to their ... : "aerosol" is singular, but "their" refers to something plural. So it sounds a bit strange.

- page 13, line 294 : "weakly positive in comparison with other forcings" : this sounds a bit strange.

- page 13, line 298 : their combined : sounds strange. I would suggest "the combined".

- page 13, Fig. 3 : maybe add the global mean values in the figure headings.

- page 14, line 308-309 : Despite ..., ... produce slightly different results. Isn't it what

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one should expect? As it is expected, I would not use "despite".

- page 14, line 324 : 1.83 Wm⁻² (but 1.82 Wm⁻² in Table 3).

- page 14, line 325-328 : It is informative to stress the absolute difference in CO₂ ppm. However, maybe one could add that the relative change in CO₂ concentration is more relevant for the forcing.

- page 15, line 337 : low-clouds -> low clouds.

- page 15, line 338 : piClim-CO₂phys : the "2" should not be an index.

- page 15, line 340-341 : low-level cloud -> low-level clouds.

- page 15, line 337-341 : The first sentence gives the impression that the balance comes from two terms. However, the second sentence adds that the balance (or closure) comes from other terms.

- page 15, line 342 : found a much larger effect : this looks like a dramatic message, but it is just because the forcing is stronger (it is a 4xCO₂ experiment). Therefore the reader is a bit in doubt whether he captures what the authors want to say.

- page 15, line 354 : weren't -> were not.

- page 16, line 366 : correlated to -> correlated with.

- page 16, line 378 : SAM : this abbreviation has not been defined.

- page 17, line 380-381 : "and a reduction of associated ... " : this sentence is slightly confusing, as through the reduction in high clouds, the outgoing LW radiation can be stronger again.

- page 17, line 386 : near surface wind : maybe one can specify the altitude. Is it at 10 m?

- page 18, line 408 : are of the order -> are in the order.

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- page 18, line 417 : The major driver ... is greenhouse gases (GHGs) -> are [although I am not sure].
- page 18, line 417 : which is offset by aerosol : This is a slightly unlogical construction : I would think that forcings can be offset, but not GHGs.
- page 18, line 418 : key metrics -> key values.
- page 19, line 424 : "in e)" -> I would advance that slightly.
- page 19, line 433-437 : is the value 2.82 Wm⁻² representing the 1850-2011 estimate? (does it already include the correction for going from 1750 to 1850?)
- page 19, line 436 : e.g. CH₄ : I assume this is also valid for CO₂. Why not mentioning CO₂?
- page 19, line 440 : However, there is a discrepancy in ERF of 0.35 W m⁻² ... which cannot ... -> However, the discrepancy of 0.35 Wm⁻² ... cannot ...
- page 20, line 443 : is this non-linearity similar to (or larger/smaller than) the one which one sees in the RF formulas of AR3 for N₂O and CH₄?
- page 20, line 450 : The rapid adjustments (RA) ... includes -> The rapid adjustments (RAs) ... include.
- page 20, line 453-454 : are there no other sources of uncertainty : the lifetime of aerosols? Their vertical profile?
- page 20, line 454-455 : twice "sources" in this sentence.
- page 21, line 480 : AEROCOM II -> AEROCOM Phase II.
- page 21, line 481 : The BC ERF was +0.32 W m⁻² -> The BC ERF is +0.32 W/m².
- page 21, line 481 : and small negative offset -> and a small negative offset.
- page 21, line 484 : in upper-level cloud -> in upper-level clouds.

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- page 21, Fig 7b : should the orange bar represent -0.14 Wm⁻²? It looks larger.
- page 23, line 525-526 : Are there two experiments with prescribed CDNC : piClim-control-fixedCDNC and piClim-aer-fixedCDNC? "By comparison with the main piClim-aer" : shouldn't be added "and piClim-control"?
- page 23, line 532-533 : To complete the breakdown, ... : I assume this was on the main piClim-control and piClim-aer simulations, and not on the ones with fixed CDNC. This is maybe not so clear from the text.
- page 24, line 566 : "over the South Pacific" : looking at the figure, it is not so clear that the South Pacific stands out more than other regions.
- page 24, line 573 : is increased -> are increased.
- page 25, line 596 : tropics -> subtropics.
- page 25, line 602-603 : due to experimental setup -> due to the experimental setup.
- page 25, line 609 : 15 % -> 15% (there is a blanco space between "15" and "%" in the text).
- page 26, Eq. 9, and line 630 : CH should not be written in italic.
- page 26, line 630 : where ... is ... the concentrations -> concentration.
- page 26, line 631 : there is apparently no blanco space after "piClim-control,".
- page 28, line 669-670 : I suggest to write "hydroxyl radical" and "nitrate radical".
- page 28, line 674: it doesn't -> it does not.
- page 28, line 680-681 : maybe add at which altitude. It is mentioned in the caption of the figure, but it is maybe informative to mention it also in the text.
- page 29, line 699 : Global distributions of the multi-annual distributions : twice "distributions".

C22

- page 29, line 707 : is changes to OH -> are changes to OH.
- page 30, line 710-714 : how is the ari from NO_x calculated? How in general are the ari/aci from NO_x and VOC calculated?
- page 30, line 731 : as was the case with NO_x : is meant here that the same mechanisms are active related to OH? As the change of OH is however opposite, the forcing is also opposite.
- page 30, line 735 : and CO₂ -> and CO₂ response.
- page 30, line 735-739 : maybe one can mention explicitly that part of this message was already given earlier (on page 30, line 718-720).
- page 31, line 753-754 : Is this true : are these the two main reasons (the fact that there are interactions, and the fact that there is non-linearity)?
- page 31, line 764 : closely correlated : the correlation seems not that high (-0.44). In addition, from Figs. 12a and 12b it is not so easy to see that there is an anti-correlation. So I would not write "closely" correlated.
- page 31, line 767 : "may be" : can this not be said with more certainty? It seems like a sound explanation.
- page 31, line 770 : with good correlation with -> correlating well with.
- page 31, line 771-772 : cloud -> clouds (twice).
- page 32, line 778 : ", (h)" -> ", and (h)".
- page 33, line 782 : This study also attempts ... : it seems to be a bit a sudden introduction. Maybe one can first introduce the topic in general, and then say that it is also a focus of this study.
- page 33, line 782 : interaction between the aerosol and O₃ forcings : is it really an interaction between the forcings which causes this?

C23

- page 33, line 784 : particularly in the net CS components : it is a bit strange to focus on the the net CS component, as the difference in SE CRE is even bigger.
- page 33, line 784 : Firstly we calculate the aerosol IRFs. How are the IRFs calculated?
- page 33, line 792 (not shown) and Fig. 13: it might be interesting to add a figure panel with the differences in O₃ profiles.
- page 34, line 805-807 : when reading this sentence, is seems that more attention is given to the +0.07 Wm⁻² effect (i.e., the change from -0.39 to -0.32 Wm⁻²), than on the value of -0.32 Wm⁻² itself.
- page 34, line 817 : and its cloud-free, aerosol-free, component -> and its cloud-free and aerosol-free component.
- page 35, line 822 : The seasonality of ... cause -> causes.
- page 35, line 830 : hist -> historical (the official name of this CMIP6 experiment).
- page 35, line 840 : As summarized above ... : maybe this is not a very good introduction - I don't think that "summarized" is the best way to refer to earlier text.
- page 35, line 848-850 : What is the motivation for this sentence? Why is it mentioned that ozone is prescribed, but not, e.g., that oxidants like OH, NO₃, and H₂O₂ are sometimes prescribed in models?
- page 36, line 857 : AF19 : this abbreviation is used only three times - I would think that it does not make so much sense to define it.
- page 36, line 866 : what is GC3.1? Should probably be HadGEM-GC3.1.
- page 36, line 866 : (well within the uncertainty range) : maybe one can add which uncertainty range is meant.

SECTION 5

C24

- page 37, line 887 : paper -> study.
- page 37, line 900 : may result : cannot it be expressed more firmly?
- page 37, line 900 : coming from cloud top -> coming from cloud tops.
- page 38, line 942 : reproduces -> reproduces well.
- page 38, line 949 : we consider -> we suggest.

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