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Interactive comment on "The SSP greenhouse gas concentrations and their extensions to 2500" *by* Malte Meinshausen et al.

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REPLY TO

Interactive comment on "The SSP greenhouse gas concentrations and their extensions to 2500" by Malte Meinshausen et al. Anonymous Referee #2 Received and published: 5 December 2019 This paper describes the distribution of greenhouse gases (and some of their impacts) as needed for the CMIP6 experiments, especially ScenarioMIP and AerChemMIP. I find the paper very thorough in its documentation, and is clearly a very useful addition to the CMIP6 papers. I have minor comments listed below, and the authors can decided whether to integrate them in the next version:

REPLY: Thank you for your overall positive review. END REPY.

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Line 50-52: I am not convinced that the SSP are more evenly spaced than 2.6-4.5-6.0-8.5. The addition of 1.9 is useful for the lower end, but 6.0 or 7.0 is basically equivalent in terms of distance.

REPLY: Thank you. The issue is that RCP4.5 and RCP6.0 were very similar up to the middle the of century (also evident from our Figure 9 of mid-century CO2 and CH4 concentrations. As shown in Figure 9, the high-priority SSPs are generally more evenly spaced. END REPY.

Line 63-64: “it is a collective choice. . .” seems like a policy statement that I don’t feel belong to the paper.

REPLY: Thank you. As wider relevance for the general public is often pursued as a last sentence in an abstract or the end of the conclusions, we feel it is appropriate to put the framing of a “choice” to these future scenarios. The scenarios give decision makers a set of tools to weigh various possible future options against each other. As the primary scenarios in the scientific literature, it is hence important that these SSPs are generally understood as reflecting the collective choice of society, not only as an abstract future uncertainty. In order to put the language a bit more neutral though, we deleted the term “hothouse”, and adapted the language from “collective choice” to “societal choice” so that the last sentence of the abstract reads now:

“The SSP concentration time series derived in this study provide a harmonized set of input assumptions for long-term climate science analysis; they also provide an indication of the wide set of futures that societal developments and policy implementations can lead to - ranging from multiple degrees of future warming on the one side or approximately 1.5C warming on the other.” END REPY.

Line 72: ESMs are driven by many more emissions than CO2

REPLY: Thank you. We changed this section in order to provide a clearer separation between ESMs and AOGCMs. For GHGs, ESMs are normally not driven with CH4, N2O or any other non-CO2 GHG emissions – at least not in the main CMIP6 experiments to our knowledge.

The expanded section now reads (also due to other review comments): "The atmosphere-ocean general circulation models (AOGCMs) are physical climate models that may include biogeochemical model components, such as vegetation or some atmospheric chemistry, but they are not able to project CO2 concentrations from emissions due to an incomplete, imbalanced or non-existent carbon cycle. The climate models that have this ability to project CO2 concentrations from emissions, are often referred to as Earth System Models (ESMs) (Lawrence et al., 2016;Jones et al., 2016). These ESMs are also often run in 'CO2-concentration driven mode' for computational ease and to allow for an easier separation between carbon cycle feedbacks and climate responses. As of today in phase 6 of the Coupled Model Intercomparison Project (CMIP6) (Eyring et al., 2016), both AOGCMs and ESMs use concentrations from all non-CO2 greenhouse gases to perform multi-gas experiment (such as the future scenario projections) due to either missing non-CO2 gas cycles or prohibitive computational costs of including such cycles. END REPY.

Line 86: the correct reference to the description of the experiments is the GMD papers, not the es-doc site.

REPLY: Thank you. We provide the GMD paper references and rephrased the reference to es-doc.org site now to read: "... (see search.es-doc.org for a tabular overview of the experiments)." This hopefully avoids the misunderstanding that the es-doc.org site is the primary reference. END REPY.

Line 111: while aerosol abundances are important in present-day and early 21st century, this becomes much less of an issue further in the 21st century

REPLY: We agree. Nevertheless, remnant aerosol emissions, including remainder NH3 and biomass-burning aerosols, will still cause some radiative forcing differences around 2100, if the current scenarios are somewhat representative of the range. Thus, for com-

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pleteness, we mention here not only GHG concentrations (the topic of this paper) but also aerosols and expand to other forcers for completeness. The sentence now reads: " "Those labels are merely indicative, given that actual radiative forcing uncertainties (and differences across ESMs that implement the same concentrations, aerosol abundances, ozone fields and landuse patterns) are substantial." END REPY.

Line 133: it might be beyond the scope of this paper but it would be useful to know how much of the difference in concentrations comes from the updated model. Could the old model be run with the current emissions/harmonization?

REPLY: Thank you. As the list of the considered GHGs expanded, we rather ran the inverse: We ran the current model also with the RCPs. The results are shown in Figure 11. It is apparent that the new model calibration leads to increased CO2, CH4 and N2O concentrations at least for the upper scenarios. We now added the following text to the end of section 4.4:

"When projecting future concentrations under the old RCP emission scenarios, the new calibration choice for the gas cycles of MAGICC (section 2.4) produce increased CO2, CH4 and N2O concentrations compared to the original RCP concentration timeseries, at least for the upper scenarios (Figure 11)." END REPY.

Line 188: it might be worth explaining in more details the meaning of "harmonization" and "categorization"

REPLY: We point the reader now more explicitly to Gidden et al. 2019, where these steps are explained. END REPY.

Line 225/line 238/line 553-554: the fact that the paper is from 2015 (WMO 2014) highlights one issue that keeps coming back, that is that the emissions/concentrations of ODSs are out of phase with the WMO recommendation. This is rather unfortunate, but also points to the fact that the system needed to create seems rather complicated/obscure and therefore limits the possibility to easily generate concentrations from other scenarios.

REPLY: We agree. At the time when we had to pull together these scenarios and provide them to the CMIP6 community, the WMO 2018 scenarios were not available yet. Admittedly, our documentation paper (this study) is delayed. END REPY.

Line 256: “AerChemMIP”

REPLY: Corrected. END REPY.

Line 261 (also lines 268-269): what is the justification for bringing negative emissions to 0? Don’t we have the technology assumption to keep them negative? This seems arbitrary without a justification.

REPLY: Thank you. In response to this valid point, we now inserted a justification in the text that reads: “We did not assume permanent net-negative CO2 emissions to maintain proximity to the original scenario design and in the light of biophysical and economic limits of negative emissions, as well as potential side-effects (Fuss et al., 2018;Smith et al., 2016)”. END REPY.

Line 305: there has been a lot more work on OH concentrations since 2001 and 2011. REPLY: We now clarified how these two references are meant to be understood in this sentence, i.e. simply as a description of the underlying modelling skeleton, which has been calibrated (as in section 2.4.1) to the Holmes and Prather et al. studies. The new sentence now reads: “ “On top of this, increased CH4 emissions are modelled to affect (alongside several other reactive gas emissions like CO, NMVOC and NOx) tropospheric OH concentrations (as described for our modelling framework in Meinshausen et al., 2011a; based on Ehhalt et al., 2001)” END REPY.

Line 327: problem with reference REPLY: Apologies. Fixed. END REPY.

Line 376: “while we do not entertain. . .” seems very much a lost opportunity. Even if it is only partial, adding knowledge on uncertainty, especially on feedback, would be quite important to discuss and include.

REPLY: We agree that a fully probabilistic setup is warranted in the future. However, given the main focus of this study, i.e. to provide standardized inputs (without uncertainty) for a large multi-model intercomparison exercise, it was beyond the scope of this study to entertain a probabilistic setup – for the permafrost module and for the other modules. END REPY.

Section 2.7: this section seems to be out of place since the discussion focuses on the concentrations

REPLY: We agree that this section is not directly on concentrations. However, given that the Etminan results substantially shifted the radiative forcing of CH4, we ought to make sure that the underlying modelling framework represents this update. Without it, the projected temperature-dependent concentration projections could not have been undertaken on the basis of the latest findings. Also, we needed to develop new parameterisations for the Etminan Oslo line by line results, as the originally published parameterisations were not valid for the full range of projected concentrations (as our long-term concentrations for SSP5-8.5 exceeded 2000ppm). END REPY.

Line 584: It seems rather unfortunate that the research community only has access to a handful of those 475 scenarios. I strongly encourage the authors to identify a path towards a better integration between the two communities.

REPLY: The IPCC Special Report 1.5C database is publicly available (with registration, see https://data.ene.iiasa.ac.at/iamc-1.5c-explorer/) and can be used for research. Also, under the leadership of Zebedee Nicholls, we are developing a close integration of MAGICC into the IIASA database so that scenarios can be amended by GHG concentration projections and also probabilistic temperature projections. Thus, the reviewer's suggestion is much appreciated, and we are working on it (with our limited resources). END REPY.

Section 4.4: it would be amazingly useful (and most likely powerful) if we had on the same graph all those scenarios, including IS92 and SRES!

REPLY: In terms of emissions, some of us prepared such a graph for the forthcoming IPCC AR6 report. Please register as an Expert reviewer. See section 1.6 in Chapter 1. We however take the reviewer's comment as encouragement to provide more dedicated comparisons also in the concentration and temperature space in future studies. END REPY.

Line 691-692: Why is SSP5-8.5 much higher than RCP8.5? REPLY: The Integrated Assessment modelling teams intended to approximately match again 2100 forcing levels of 8.5 W/m2. With the specific modelling team behind the chosen illustrative marker SSP5-8.5 scenario (i.e. the REMIND group at the Potsdam Institute for Climate Impact Research) projecting comparatively lower CH4 concentrations and greater abundance and use of fossil fuels, the CO2 concentrations increased more. See also Figure 9 on this aspect. More detail to be found in the REMIND SSP5 papers, such as Kriegler et al, 2018 (https://doi.org/10.1016/j.gloenvcha.2016.05.015). END REPY.

Section 4.5: I am not sure I fully see the value of this section. It seems that it will be much more useful to do an evaluation of MAGICC against the CMIP6 models.

REPLY: Section deleted. Such an evaluation is being prepared by us (many things, little time. . .) and there are some preliminary comparisons available in Nicholls et al., GMDD 2020 (https://www.geosci-model-dev-discuss.net/gmd-2019-375/). END REPY.

Line 799-801: based on this, it seems that the whole discussion on latitudinal and seasonal variations could be significantly reduced.

REPLY: We would argue that while the effect is not beyond the min-max "variability range", there is nevertheless a strong reason to get the latitudinal and seasonal variations correct. After all, there are lot of process amended and introduced in ESMs that would not pass the test of causing a global or zonally averaged temperature signal beyond natural variability min-max ranges. ESMs are not performing well when it comes to estimating high polar warming. The inclusion of latitudinally and seasonally resolved GHG concentrations can therefore possibly help to address this bias. END REPY.

Line 955: “AGAGE”

REPLY: Apologies. Corrected. END REPY.

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