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Interactive comment on "Historical greenhouse gas concentrations" *by* Malte Meinshausen et al.

Anonymous Referee #2

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The manuscript presents a data synthesis and assimilation study which aims at producing historical greenhouse gas concentration fields to be used in CMIP6 historical simulations. While I am overall confident that the results are of sufficient quality, I found the manuscript difficult to read and confusing in several aspects. It is not enough focussed on the most relevant time period (1850-2014) and species (in terms of radiative forcing). The task of precisely documenting and illustrating the data for all considered periods and 43 species may have been too ambitious, and results in incomplete documentation of the data even for the most important period and species: incomplete and hard to read figure captions, match of the data and scenarios hard to appreciate on figures in 1850-1950, missing references etc. (see below), however it should not be too difficult to improve the results presentation and discussion for the most important species and CMIP6 simulations relevant period. The scientific aspects of the study would be better highlighted if figures and tables that are little or not commented in the text were placed in a supplement more focussed on the technical

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documentation of the data. I noted several potential circular arguments that should be clarified. In a worldwide IPCC context study, I was sad to read that the data used are nearly exclusively American network data for the atmospheric measurements (whereas for example WDCGG conveniently provides a large dataset in consistent format) and Australian data firn and ice core data (whereas considering all existing firn/ice datasets for the CMIP6 historical simulations period and most important species should not require a tremendous bibliographic effort). In a CMIP7 perspective, I think that more efforts could be made to relate the building of model inputs to an IPCC worldwide data synthesis, and include uncertainty estimates.

Detailed comments:

CMIP6 could be mentioned in the abstract.

The lists and number of species at lines 33-36 and 138-142 are not consistent.

The introduction or Section 4 could mention how other important greenhouse gases (e.g. O_3), greenhouse gas producers (e.g. CO, organics), aerosol source species (e.g. organics and sulfur compounds) and/or aerosols should be handled in historical simulations.

lines 52-56: this sentence is misleading, other contributions could be emphasized such as Buizert et al. (2012), WDCGG etc.

lines 76-78 and Tables 3 and 10: it would be useful to provide a radiative forcing ranking of the 43 species considered.

lines 83-85: only a few references are provided here, as well as in Sections 3.4 and 3.5 and the Supplement.

lines 101-112: the role of the ocean could be mentioned in the discussion of past latitudinal CO_2 gradients.

line 121: more recent references could be provided for CH_4.

lines 176-180: references are provided for a subset of AGAGE data (not CH_2Cl_2 discussed at lines 167-168) but not NOAA data. More generally, it is not clear to me if the datasets for all species are published and/or publicly available in AGAGE or NOAA databases.

lines 181-196 (calibration scales): for the major halocarbons in terms of radiative forcing, calibration scale intercomparison studies (e.g. Hall et al., 2014; Rhoderick et al., 2015) could be used at least to evaluate uncertainties. Scale names for the seven species mentioned at lines 186-188 suggest that measurements were not made by AGAGE or NOAA. Is it the case? If yes, could the data source / reference be provided?

lines 252-262: I'm not at ease with the principle of scaling CO_2 variations with temperature variations while producing inputs for models aiming at evaluating the impact of CO_2 on temperature. On Figure 2 a.3 the seasonality change is provided only after about 1950. I think that the earlier CO_2 seasonality change should be illustrated and discussed.

lines 278-281, 292-294 and Figures 1, 2, 4, 5 and 9. The ad hoc smoothing of Law Dome data and very high sampling resolution of non Law Dome recent ice core data (e.g. Bauska et al., 2015, Mitchell et al., 2013, Rhodes et al., 2013) makes the choice of using Law Dome only data less obvious than some years ago. The choices of time scales in the Figures make it very hard to appreciate the match of the scenarios with available firn and ice data especially for the beginning of the CMIP6 historical runs (1850-1950). One specific issue is that in view of the N_2O data dispersion, I'm not convinced that the dip in the N_2O scenario around 1850 is really reliable.

lines 305-308: in this very technical description, I did not understand the main message. Why does the Gosh et al. (2015) data need to be updated? Why excluding North GRIP? Does that induce significant changes?

lines 312-315: the methodology is unclear to me here. Mixing ratio data are always local. Global or hemispheric means should already be the result of an assimilation procedure. Is there some circularity in constraining an assimilation procedure with assimilated data?

lines 314-321: the list of key studies should be focused on key species in terms of radiative forcing. It would be useful to provide the references of all data used in the supplementary tables.

lines 323-325: what are the data used to constrain the major halocarbon trends (e.g. CFC-11, CFC-12, HCFC-22) before about 1978?

lines 373-377: I don't understand the motivation for grouping the species as ozone depleting versus non ozone depleting. Splitting the species between those destroyed in the troposphere or not seems more obvious to me as they have very different vertical structures. Could the ozone depleting choice be commented?

lines 380-390: are the inputs used for CMIP5 simulations (CO_2 fluxes?) and the CMIP6 input scenarios discussed here fully independent?

lines 399-409 and Figure 1: the consistency of the different datasets for the CMIP6 simulation period (after 1850) should be made more visible on the figure and should be commented in the manuscript.

lines 448-449: Section 3.1 starts with discussing discrepancies of several ppb between ice core datasets and large uncertainties on meridional gradients. Providing estimates of the uncertainties on the global mean CO_2 at the dates mentioned would be useful.

lines 497-498 and Figure 1f: in view of the large discrepancies between ice core records and large dispersion of the N_2O data in the 1850-1970 period, more firn and ice datasets could be used to evaluate the trend used in CMIP6 historical runs (e.g. Machida et al., 1995; Battle et al., 1996; Ishijima et al., 2007)

Sections 3.4 and 3.5 lack focus on the most important species in terms of radiative

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forcing and bibliographic references.

lines 536-545: bibliographic references should be provided for the nineteenth century mixing ratio estimates.

lines 717-721 and Figure 9: the reason why the early part of the CMIP6 CO_2 trend is smoother than the CMIP5 trend whereas the early part of the CMIP6 N_2O trend is less smooth than the CMIP5 trend is unclear to me. Could this choice be commented?

Section 5.2: Figures 10, 11, 13 and 14 are not directly comparable to Figure 2 and could be placed in a Supplement, whereas Figure 12 could include a representation of the CMIP6 scenarios in similar format as the CMIP5 mixing ratio outputs.

Section 5.5: the comparison with other literature studies lacks priorities in terms of radiatively most important species and a check of the independence of the data used for evaluation with respect to those used to generate the assimilated fields. For example, the CO_2 and CH_4 high Northern latitude trends in Buizert et al., 2012 were provided by V. Petrenko (see file SCENARIO_NEEM2008_....xls) and are mostly based on NOAA ESRL and Law Dome data (see Section 2.4.2 in file Supplement Buizert ....pdf). On the other hand, the comparison with early CO_2 atmospheric data (Keeling et al., 1976) is not commented. I'm surprized that the Cape Grim air archive data are not commented for N_2O (Park et al., 2012) and other species (e.g. Newland et al., 2013). It could be mentioned that the Martinerie et al. (2009) trends for halocarbons are based on industrial emission histories and are used in Buizert et al. (2012) for the time period preceding atmospheric measurements.

Lines 860-870: I could not see the Buizert et al. (2012) trends on the Figures. Are the commented differences within uncertainties provided in Buizert et al. (2012)?

lines 871-877: would the pioneer study by Butler et al. (1999) be more consistent with other trends before 1950 if the South Pole firn air age spread was taken into account?

Section 6: major additional uncertainties for the early part of CMIP6 historical simulations such as the lack of constraints on nineteenth century CO_2 meridional gradients could be mentioned.

Tables 2, 4, 5, 6, 7, 8, 9, 11 (technical documentation) could be placed in a Supplement

Technical corrections:

lines 60, 708, 709 etc. and references: Meinshausen et al., 2011, 2011a and 2011b seem to be the same article

lines 158-159: the first figure quoted in the manuscript should be Figure 1 rather than Figure 22

lines 189-193 and 281-282: It would be clearer to describe the scale change of the firn and ice data together with the scale description of the atmospheric data.

line 230: define EOF notation at first use

line 595: is it really needed to quote Eyring et al., GMDD, 2015 instead of Eyring et al., GMD, 2016?

lines 799-800: check the writing. This sentence seems contradictory with lines 167-171, 801-802 and 824-829.

lines 904-905: Trudinger et al. (2004) is not more recent than WMO (2014) and Velders et al. (2014)

lines 1408-1410: incorrect list of authors

Figure 1: horizontal scale issue for panel c. A complete reference should be provided for each dataset. I saw only CO_2 data (no CH_4 and N_2O data) in Rubino et al. (2013), and Table 1 mentions different references for Law Dome CH_4 and N_2O data.

Figure 6: panels g, i, k, m, o, q would be much easier to read if the horizontal scale started in 1850 or 1900

Figure 12: I can't see the 12 five lines mentioned in the caption, and the shaded areas

are not described in the caption

Figure 15: wrong reference for the CH_4 "NEEM" scenario (see Supplement of Buizert et al. 2012, Section 2.4.2 in file Supplement Buizert ....pdf, and file SCENARIO_NEEM2008_....xls, CO_2 and CH_4 scenarios were made by Vas Petrenko). The NOAA global mean and WDCGG global mean results should be made easier to distinguish.

References not provided in the manuscript:

Battle et al., Nature, 383(6597), 231-235, 1996

Hall et al., Atmos. Meas. Tech., 7, 469-490, 2014

Ishijima et al., J. Geophys. Res., 112, D03305, 2007, doi:10.1029/2006JD007208

Machida et al., Geophys. Res. Lett., 22(21), 2921-2924, 1995

Newland et al., Atmos. Chem. Phys., 13, 5551–5565, 2013.

Park et al., Nature Geoscience, 2012, DOI: 10.1038/NGEO1421.

Rhoderick et al., Elementa Sci Anth 3: 000075, 2015, doi:10.12952/journal.elementa.000075

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