

Interactive comment on “Historical greenhouse gas concentrations” by Malte Meinshausen et al.

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Received and published: 29 November 2016

Reply to comment on “Historical greenhouse gas concentrations” by Anonymous Referee #2

Comment 1: 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 focused 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

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should not be too difficult to improve the results presentation and discussion for the most important species and CMIP6 simulations relevant period.

Reply 1: First of all, we would like to thank the reviewer for the extensive and without doubt time-consuming review comments that helped to, we believe, enhance the quality of the manuscript substantially.

We share the reviewer’s observation that the overall project has turned out to be rather ambitious in terms of covering lots of gases, especially because the time series and concentration fields had been built up from individual station data. Unlike for CMIP5, when existing time series were simply “glued together”, the ambition to provide monthly and latitudinal optional data coverage for CMIP6 and extrapolation meant that a simple merging of existing data was not possible. This comprehensive effort however allowed to produce more consistent time series.

For example, the CMIP5 global-mean time series was taken from the Law Dome record data, which did not represent a best guess for the global-mean concentration (which was in particular an issue for methane). The absence of global-mean concentration time series in the literature back to 1850 meant anyway that latitudinally gradients had to be taken account of when aggregating station data – which meant that the step to providing the latitudinal consistent fields was comparatively small, given they had to be produced anyway in the background to produce a best estimate of global-mean surface concentrations back in time.

We do however acknowledge the that focus on 1850-2014 for the three big gases could be strengthened. We hence adapted our old overview figure and added three panels specifically for that 1850-2014 period for CO₂, CH₄ and N₂O – in conjunction with some comparison and input data for our study. See the new Figure 6.

The new Figure 6 is: [SEE NEW FIGURE 6 ATTACHED AT BOTTOM OF THIS COMMENT]

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Caption Fig. 6 - Atmospheric CO₂, CH₄ and N₂O mixing ratios over different time-scales, from 800 thousand years ago until today (panel a), over the last 2000 years (panel b) and over 1850 to 2014 (panel c, d, e). The shown data is for CO₂: Mauna Loa data by Keeling et al. (Keeling et al., 1976); the Law Dome ice record (Etheridge et al., 1998b; MacFarling Meure et al., 2006; Rubino et al., 2013); NOAA ESRL station data (NOAA, 2013; NOAA ESRL GMD, 2014a, b, c); the EPICA composite data (Ahn and Brook, 2014; Bereiter et al., 2015; Bereiter et al., 2012; Lüthi et al., 2008; MacFarling Meure et al., 2006; Marcott et al., 2014; Monnin et al., 2004; Petit et al., 1999; Schneider et al., 2013; Siegenthaler et al., 2005) and the WAIS data (Bauska et al., 2015). For CH₄, the shown data is the Law Dome data (Etheridge et al., 1998a; MacFarling Meure et al., 2006), the instrumental data from the NOAA and AGAGE networks (see Table 3), NEEM ice core measurements (Rhodes et al., 2013) the EPICA composite (Barbante et al., 2006a; Barbante et al., 2006b) the long record by Loulergue et al. (2008) as well as the GISP2D, WDC05A and WDC06A records by Mitchell et al. (2013). In case of N₂O, the shown data is the Law Dome record (MacFarling Meure et al., 2006), the Talos Dome record (Schilt et al., 2010b), the GISPII record (Sowers et al., 2003) and the EPICA record (Fluckiger et al., 2002; Schilt et al., 2010a; Spahni et al., 2005) in addition to the H15 ice core record from Antarctica (Machida et al., 1995), the South Pole firn record (Battle et al., 1996), the Law Dome firn record “Park” (Park et al., 2012) and a modelling synthesis by Ishijima (2007). For data sources behind “this study’s” composite product, see Table 2, Table 3 and Table 4.

Furthermore, we produced another new figure (Figure 7), which specifically looks at the 1950 to 1990 period of CO₂ and provides an additional discussion of the CO₂ time series over that time frame in comparison with Scripps station data.

We furthermore highlighted in the abstract the CMIP6 purpose of the data and point towards this specific section high up in the introduction – to ease the modeler’s digestion of the manuscript. Specifically, we added the pointer to section 4 in the Introduction, stating: “The description of the datasets geared towards CMIP6 modelling groups is

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provided in section 4, including a description of available data formats and CMIP6 minimum recommendations. “

[SEE NEW FIGURE 7 ATTACHED AT THE BOTTOM OF THIS COMMENT]

Caption Fig. 7 – Comparison of 1950 to 1990 CO₂ concentrations with early Scripps station data (Keeling et al., 2001) for each 15°-degree latitudinal band. Also, the Law Dome ice record data is shown (panel k) with our 3rd degree polynomial smoothing. This study’s monthly CO₂ zonal means were derived from station data from 1984 onwards. Before that, this study used Mauna Loa MLO annual average and smoothed Law Dome data (see Table 1 and section 2 “Methods”). The shown comparison with monthly Scripps station data before 1984 is a qualitative validation of the applied methodology to regress latitudinal gradient and seasonality changes to times before 1984. See text.

Comment 2: 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 documentation of the data. I noted several potential circular arguments that should be clarified.

Reply 2: We would welcome more detail on the “potential circular arguments” so that we can address them. We assume that they related to below comment “14” in regard to the scaling of CO₂ seasonality changes with a composite indicator that includes global-mean temperatures. On the supplement: We organized the manuscript such that all the so-called factsheets for all gases (except for CO₂, CH₄ and N₂O) are presented in Appendix A and two additional CMIP5 ESM analysis figures in Appendix B. Following Reviewer #1 comments, we placed all other figures and tables in chronological order with the text.

Comment 3: 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 for-

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mat) and Australian data firm and ice core data (whereas considering all existing firm/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.

Reply 3: We fully agree with the author that the WDCGG is a tremendously valuable initiative and indeed that it should be considered as the starting point for a CMIP7 product. While we do not think that the results would be much different (due to largely overlapping datasets) between our predominant use of AGAGE and NOAA network data and the WDCGG, we do acknowledge this to be a limitation of our study. We now added to the limitations section:

“For the recent instrumental period, our study is predominantly based the NOAA and the international AGAGE network data. Consistent quality control and consistent scales are advantages of that approach. Ideally however, our study should have started out from a yet more inclusive representation, e.g. including the multiple additional station datasets gathered and archived by the World Data Centre for Greenhouse Gases (WDCGG) that are neither part of AGAGE or NOAA networks. The WDCGG station raw data is available at: <http://ds.data.jma.go.jp/gmd/wdcgg/cgi-bin/wdcgg/catalogue.cgi>. While the methodology of our study could be maintained or built upon, we hence recommend for any future updates, that those additional datasets are considered – with the appropriate quality control and scale conversion efforts.”

However, we would like to point out that we provide and acknowledge a large set of comparison data products and studies. For example, the new Figure 1 also features the West Antarctic Ice record (Bauska et al.) for CO₂ and the factsheets for individual gases include several comparison products (mostly displayed in panels f, g and h of Figures 10, 12, 13 and Figures 24 to 63 in Appendix A (Figure numbers refer to the revised manuscript).

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Furthermore, AGAGE is an international cooperative network that relies on substantial contributions (measurements and more) from international partners (Australians, Brits, Swiss, Japanese, Chinese, Koreans, among others. NOAA also relies heavily on international partners to conduct their global network. The characterization as ‘American network data’ is hence not quite correct, we find. Regarding the missing uncertainty estimates. The reviewer is also correct to point out this limitation. We already had mentioned that in our limitations section and fully acknowledge that. Given that the primary purpose and starting point for our study has been the provision of a dataset for the CMIP6 historical model runs, uncertainty estimates were not fundamental. In fact, given that a multi-model intercomparison should be run with a single standard input to be able to compare differences in the model responses, an uncertainty estimate would not have been of particular use for CMIP6. Nevertheless, given that the dataset is likely being used outside CMIP6, we again acknowledge this limitation. Given the multiple distinct sources of uncertainty, some of them correlated in space and time, a proper uncertainty analysis would have multiplied the effort that went into this study. We hence consider this as beyond the scope of our study and encourage future synthesis efforts to include a statistically correct uncertainty analyses.

Detailed comments:

Comment 4: CMIP6 could be mentioned in the abstract.

Reply 4: Done.

Comment 5: The lists and number of species at lines 33-36 and 138-142 are not consistent.

Reply 5: Thanks for spotting that and our apologies. We made both sections consistent now. The abstract’s listing is: “We provide consolidated datasets in various spatiotemporal resolutions for carbon dioxide (CO₂), methane (CH₄) and nitrous oxide (N₂O), as well as 40 other GHGs, namely 17 ozone depleting substances, 11 hydrofluorocarbons (HFCs), 9 perfluorocarbons (PFCs), sulfur hexafluoride (SF₆), nitrogen trifluoride

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(NF3) and sulfuryl fluoride (SO2F2).”

And the more detailed listing in the methods section is: “We consider a total of 43 GHGs: CO2, CH4, N2O, a group of 17 ozone depleting substances made up of five CFCs (CFC-12, CFC-11, CFC-113, CFC-114, CFC-115), three HCFCs (HCFC-22, HCFC-141b, HCFC-142b), three halons (Halon-1211, Halon-1301, Halon-2402), methyl chloroform (CH3CCl3), carbon tetrachloride (CCl4), methyl chloride (CH3Cl), methylene chloride (CH2Cl2), chloroform (CHCl3), and methyl bromide (CH3Br), and 23 other fluorinated compounds made up of 11 HFCs (HFC-134a, HFC-23, HFC-32, HFC-125, HFC-143a, HFC-152a, HFC-227ea, HFC-236fa, HFC-245fa, HFC-365mfc, HFC-43-10mee), nine PFCs (CF4, C2F6, C3F8, C4F10, C5F12, C6F14, C7F16, C8F18, and c-C4F8), NF3, SF6, and SO2F2.”

Comment 6: The introduction or Section 4 could mention how other important greenhouse gases (e.g. O3), 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.

Reply 6: Thanks. Rather than providing a description ourselves, we however opted to point the modelers to the experiment specific protocols. The addition to section 4 reads: “Depending on the specific CMIP6 experiment, different protocols and recommendations can apply. Modelers should hence also check the experiment specific descriptions (see special issue available at http://www.geosci-model-dev.net/special_issue590.html), including protocols regarding the important other forcing input datasets like aerosols, their emissions and optical properties, landuse patterns, but also short-lived GHGs like tropospheric and stratospheric ozone for models without interactive ozone chemistry.”

Comment 7: lines 52-56: this sentence is misleading, other contributions could be emphasized such as Buizert et al. (2012), WDCGG etc.

Reply 7: We revised the respective section, so that it reads now: “To date, recon-

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structions of millennial global-mean time series based on ice and firn data have been performed, e.g. for CO2 over the last millennia (Ahn et al., 2012; MacFarling Meure et al., 2006; Rubino et al., 2013). For the more recent past, several studies investigated firn and ice data to constrain halocarbons (Buizert et al., 2012; Martinerie et al., 2009; Mühle et al., 2010a; Sturrock et al., 2002; Trudinger et al., 2016), some of them with hemispheric resolution. In terms of latitudinally-resolved monthly data, there have only been a few synthesis products, namely for CO2, CH4 and N2O over the instrumental record over the past 20 to 40 years (NOAA, 2013; NOAA ESRL GMD, 2014a, b, c). For this recent past, the World Data Centre for Greenhouse Gases (WDCGG) (ds.data.jma.go.jp/gmd/wdcgg/) also provides a synthesis with global and hemispheric means for CO2, CH4 and N2O (Tsumumi et al., 2009).”

Comment 8: lines 76-78 and Tables 3 and 10: it would be useful to provide a radiative forcing ranking of the 43 species considered.

Reply 8: Table 3 provides an atmospheric abundance ranking (for all species) and Table 10 provides a radiative forcing ranking (for the first 15 species). We hence feel that inserting a radiative forcing ranking also in Table 3 would be a redundancy. We hope the reviewer is ok with this clarification of why we limited one ranking to Table 3 and one to Table 10.

Comment 9: lines 83-85: only a few references are provided here, as well as in Sections 3.4 and 3.5 and the Supplement.

Reply 9: Thank you. We acknowledge that our list of references was not complete. We now complemented the list of provided references in the introduction and also added a Table 12, which details all the literature studies we show and use. The new text now reads:

“Furthermore, many detailed literature studies (Arnold et al., 2013; Arnold et al., 2014; Aydin et al., 2010; Butler et al., 1999; Ivy et al., 2012; Martinerie et al., 2009; Montzka et al., 2014; Mühle et al., 2010b; Oram et al., 2012; Sturrock et al., 2002; Trudinger et

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al., 2004; Trudinger et al., 2016; Velders et al., 2014; Vollmer et al., 2016; Worton et al., 2006) for radiatively less important species are compared with our data product in the factsheet figures for the specific gases (Table 12 and Appendix A with Fig. 20 to Fig. 59) or synthesised where direct observational records from the above networks were not available. Furthermore, while we added a couple of references to the Results section 3.4 and 3.5, we referenced the new Table 12 again in section 5.6 “Comparison with other literature studies” as we believe this is the most appropriate place.

[SEE NEW TABLE 12 ATTACHED AT BOTTOM OF THIS REPLY]

Comment 10: lines 101-112: the role of the ocean could be mentioned in the discussion of past latitudinal CO₂ gradients.

Reply 10: Given the extent of the manuscript, we would prefer not to enter into a discussion on the ocean in pre-industrial gradients. We did however clarify that our reference to the carbon cycle relates to both the ocean and land domain. The new sentences now reads: “One complication to retrieve the latitudinal pre-industrial CO₂ concentration profile is that CO₂ fertilization and temperature effects on the carbon cycle, both over ocean and land, change both the magnitude and spatial patterns of natural CO₂ fluxes...”

Comment 11: line 121: more recent references could be provided for CH₄.

Reply 11: Thanks. We added the recent review by Kirschke et al. (2013) in Nature Geoscience.

Comment 12: lines 176-180: references are provided for a subset of AGAGE data (not CH₂Cl₂ 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.

Reply 12: In regard to CH₂Cl₂ data reference from the AGAGE network: We reproduced the key references as stated here:

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http://cdiac.ornl.gov/ftp/ale_gage_Agage/AGAGE/. Our apologies that we did not provide a balanced referencing of the NOAA data (that would be Spivakovsky, 2000). We now deleted the respective AGAGE section and moved all references to the new Table 12, reproduced above. This Table now provides a comprehensive overview of the used data sources, both from the NOAA and AGAGE networks. We also provide all the ftp links in Table 12. Yes, the data is publicly available.

Comment 13: 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?

Reply 13: We thanks for the suggestion and inserted two new sentences reading: “Gas measurements on different measurement scales, and even when using the same scales by different laboratories, are subject to uncertainties (Hall et al., 2014). For halocarbons, the difference in calibration scales has been estimated as small, but not negligible, i.e. within 2.5%, often within 1% (Rhoderick et al., 2015).” The earlier scales were used in some of AGAGE network data (which includes a global range of partners), before the scales were converted to the most recent standard).

Comment 14: lines 252-262: I'm not at ease with the principle of scaling CO₂ variations with temperature variations while producing inputs for models aiming at evaluating the impact of CO₂ on temperature. On Figure 2 a.3 the seasonality change is provided only after about 1950. I think that the earlier CO₂ seasonality change should be illustrated and discussed.

Reply 14: We now added a new figure to discuss both various alternatives as well as pre-1950 times in regard to their assumed CO₂ seasonality changes. As we've indicated in our text, we had tested several regression options, including using only

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CO2 concentrations at regressor of the seasonality changes. The added text now reads:

“Specifically, we tested global-mean CO2 concentrations, global-mean annual average surface air temperatures and lagged averages of surface air temperatures as regressors (see Fig. 5). The R-squared values of the regressions over the 1984-2014 period are relatively similar across all regressors, around 0.8. The marked difference is that the regression with only CO2 concentrations would result in a stronger reduction of seasonality around 1940-1960 and before 1900. By 1850, the reduction of summertime CO2 concentrations in the zonal band around 52.5°N would be around 8.6 ppm compared to 2014 (multiply the differences of the seasonality scaling difference between 1850 and 2014, about 21, with the 0.41 ppm maximum of the EOF pattern, shown in Fig. 9 a.2). In contrast, the other regression options would limit the maximal seasonality change to about 5.7ppm, closer to the maximal seasonality change detected within the period 1984-2014, of 4.5ppm (cf. Fig. 5e). Given the uncertainty in regard to pre-1960 seasonality, we opted for the more conservative extrapolation method that implies a less significant change outside the observational period and chose the regressor with the least variability, namely our composite regressor combining temperature and CO2 concentrations.

Despite the differences in the regressors, it should be noted that early CO2 observations are too sparse to come to a definite conclusion in regard to which regressor is best suited – given the induced differences around 1960s and 1970s are fairly small compared to the noise in the observations (see panel f and g of Fig. 5). Furthermore, ... “

Regarding the first point, i.e. the potentially circular argument of scaling CO2 concentrations with temperatures. We appreciate the concern. However, we use global-mean temperatures in our composite indicator with which we change the seasonality changes. Given that any scaling of our zero-mean seasonality patterns does not change global-mean CO2 concentrations, the actual influence in CMIP6 model runs

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of higher or lower seasonality patterns should be a rather small second-order effect related to the differential global-mean and annual average warming resulting from the high-northern latitude winter and summer-time CO2 concentrations that are not offset by correspondingly lower to CO2 concentration changes elsewhere. The error that results from a uniform application of global-mean annual average CO2 concentrations in the concentration-driven runs will likely be orders of magnitude higher. We are therefore not clear, why the scaling of the seasonality changes with an observed record of global-mean annual average temperatures would materially affect the CMIP6 modelling results.

The new figure to address Comment 14 is: [SEE NEW FIGURE 5 AT END OF THIS COMMENT]

Caption Fig. 5 – Comparison of various scaling options for the change of seasonality of CO2 concentrations over time. The first EOF of the residual fields of observations minus the mean 1984-2014 CO2 seasonality (Fig. 9 a.2) is scaled with an EOF score. Before 1984, this EOF score is regressed against a composite of global-mean CO2 concentrations and global-mean surface air temperatures (see text and panel b). Alternative regressors include global-mean CO2 concentrations (panel a), lagged averages of monthly global-mean surface air temperatures (panel c) and raw global-mean annual average surface air temperatures (HadCRUT4v) (Morice et al., 2012) (panel d). The regressed EOF score back in time is shown in panel e. A comparison to the first CO2 measurements of higher northern latitudes at so-called Station P (STP) and Point Barrow in Alaska (PTB), where the seasonality change is most pronounced, is provided in panels f and g, respectively (see text for discussion).

Comment 15: 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

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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 N2O data dispersion, I'm not convinced that the dip in the N2O scenario around 1850 is really reliable.

Reply 15: We hope that the updated Figure 6 addresses the concern and provides better comparability of the different firn and ice records. In regard to CO₂, we choose our settings for the median-preserving smoothing such that the shape of the WAIS record by Bauska et al. 2015 is approximately matched, although corrected by the offset. Thus, while the smoothing originates from the Law Dome record, we took the evidence of the other ice records into account and opted not to follow the more pronounced variations in the Law Dome record (although we are happy to produce such a higher-frequency CO₂ history for interested modelling groups). In regard to N₂O: Indeed the divergence between different ice record histories is large and unresolved. It is outside the scope of this study to arrive at a conclusive best-estimate synthesis of all available ice core records. We hence would like to see our N₂O histories to be seen as one plausible history.

Comment 16: 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?

Reply 16: We clarified the text to read now: "We used the NEEM CH₄ firn measurements from Buizert et al (2012) (2008 campaign), with effective ages from Ghosh et al. (2015) based on the iterative dating method of Trudinger et al (2002b), corrected for the effect of gravity (as applied in other firn data) and put onto the NOAA 2006 primary calibration scale."

Comment 17: 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?

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Reply 17: We would not call it circularity. We simply base the CMIP6 datasets on available literature studies in some cases. For some gases that lack a good representation in the main measurement networks (at least for some periods), like the PFCs listed in this sentence, we opt for replicating the same NH and SH averages as the cited literature studies. Given that we do not claim of having derived those latitudinal gradients from raw station data, we are not sure we understand the issue.

Comment 18: 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.

Reply 18: Thank you and apologies for our oversight of not having provided a full reference list before. We hope that the new Table 12 in conjunction with the updated Factsheet figures addresses that concern. We also revised the text to provide missing references. The ranking in terms of the 15 most radiatively important species is provided in Table 5.

Comment 19: 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?

Reply 19: We clarified that by adding the sentence: "The three radiatively most important fluorinated species CFC-12, CFC-11 and HCFC-22 (Table 5) follow the global mean concentrations provided by Velders et al. (2014), in conjunction with separately derived latitudinal gradients and seasonality."

Comment 20: 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?

Reply 20: That choice arises from an informal survey among modellers, with some CMIP6 modelling frameworks being set up in that way (and ignoring vertical gradients

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anyway). We agree with the reviewer that many other choices would make more sense. As in CMIP5, modelling groups can and likely will also apply their own aggregations to the provided original gas-by-gas data. We encourage modelling groups to do so.

Comment 21: lines 380-390: are the inputs used for CMIP5 simulations (CO2 fluxes?) and the CMIP6 input scenarios discussed here fully independent?

Reply 21: Yes and no. Yes in regard to the spatial patterns and the seasonality. No, in regard to the global-mean average values. For CMIP5, only global-mean annual average CO2 surface concentrations were provided as recommendations. Thus, in terms of input recommendations, everything but the global-mean annual-averages are independent (as the latter are partially based on the same data in both CMIP5 and CMIP6, such as Law Dome records). To what extent CMIP5 models used NOAA network data to constrain their internal and spatially varying CO2 fields is outside of the scope of this study as we did not undertake a systematic survey in this regard.

Comment 22: 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.

Reply 22: The new Figure 6 panel c that focusses on the 1850 to 2014 period hopefully addresses this concern. We've also added a new sentence in that regard: "The differences between the WAIS and the Law Dome record persist in 1850 to 1890 with subsequent data points being more aligned with each other (Fig. 6c)."

Comment 23: 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 CO2 at the dates mentioned would be useful.

Reply 23: To assist the reader, we added the following sentence: "Our methodology does not include a formal uncertainty analysis. As a minimum uncertainty for the

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1850's pre-industrial values, we refer to the 1.2ppm variability stated by Etheridge et al. (1996), also used in Rubino et al. (2013) and Trudinger et al. (2002a) as minimum uncertainty for that period." Comment 24: lines 497-498 and Figure 1f: in view of the large discrepancies between ice core records and large dispersion of the N2O data in the 1850-1970 period, more firm 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 forcing and bibliographic references.

Reply 24: We took the reviewer's suggestion on board, plotted the Machida et al. H15 ice core record, the South Pole firn data by Battle et al. (1996) and also the modelling study results presented in Ishijima et al. (2007). Please see our revised Figure 6 e above. The amended discussion of our data in comparison with these alternative ice and firn records reads now: "A temporary local maximum indicated by individual Law Dome data in the 15th century is not resolved by our smoothing, and a similar spike in the 17th century is only just reflected (Fig. 6f). Several data points indicate a small decrease after a 1750 maximum with a minimum in 1850 of around 273.02 ppb. This 1750ish maximum and subsequent minimum around 1800-1850 is also apparent in the H15 ice core record by Machida (1995) (we scale-corrected the Machida data downwards by 1 ppb as in Battle et al. (1996)) (Fig. 6b). After 1850, N2O mixing ratios increased markedly, reaching 1900, 1950, 2000 and 2014 values of 279.5, 289.7, 315.8 and 327.0 ppb, respectively (Table 6). Comparing the different firn and ice records, the 1920 – 1940 period seems particularly uncertain with some high measurements close to and beyond 290ppb from both Law Dome and H15, while some of the Law Dome data is still at levels around 285 ppb or even 280 ppb in the case of H15 (Fig. 6e). The South Pole firn data (Battle et al., 1996) suggest lower N2O concentrations in the 1920s and around 1960 – compared to both the smoothed Law Dome data (thin dashed line in Fig. 6e) and consequently our even higher global-mean estimate. Although the Ishijima estimate (Ishijima et al., 2007) (their Figure 6a) around 1952 is almost identical to our global-mean, their modelling study suggests slightly lower values around 1960

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before being closely matching again from 1970 onwards. The Law Dome firn record (Park et al., 2012) suggests slightly higher N₂O concentrations for the high southern latitudes compared to our global-mean (Fig. 6e). “

Comment 25: lines 536-545: bibliographic references should be provided for the nineteenth century mixing ratio estimates.

Reply 25: We apologize for not having been clear on the origin of those pre-industrial concentration estimates. We now extended the paragraph to read: “Four of the considered chlorinated and ozone depleting substances are assumed to have natural emissions and hence non-zero pre-industrial mixing ratios. We estimate those pre-industrial natural background concentration by a simple budget equation under the assumption of a constant lifetime (IPCC, 2013) of 1 year for CH₃Cl and 0.8 years for CH₃Br – minimizing the error term when taking into account anthropogenic emission and atmospheric concentration estimates over 1950 to 1990 by Velders et al. (2014). Specifically, methyl chloride (CH₃Cl) is assumed to have pre-industrial global-mean mixing ratios of 457 ppt, and methyl bromide (CH₃Br) of 5.3 ppt. Chloroform (CHCl₃) is assumed to have a pre-industrial mixing ratio of about 6 ppt, approximately in line with findings by Worton et al. (2006) and the estimation by Aucott et al. (1999) that in 1990 CHCl₃ was at about 8 ppt, with 80% of emissions assumed to be of natural origin. Lastly, in the absence of other information (a good understanding of the natural vs anthropogenic source fraction or historical industrial production records) the available firn measurements (e.g., Trudinger et al., 2004) supplying information about methylene chloride (CH₂Cl₂) mole fractions in the early 20th century are used to suggest a 6.9 ppt pre-industrial mean mixing ratio with a strong latitudinal gradient that results in northern (southern) hemisphere average mixing ratios of 12.8 (1.0) ppt. The transition of mixing ratios of some species between the observational station data and pre-industrial levels are also uncertain. For CH₂Cl₂, our derivation is in line with the smooth trajectory of Trudinger et al. (2004), indicating an almost monotonic transition between 1997 values and pre-industrial mixing ratios (Fig. 26f). Our assimilation approach (which is based on the

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Walker et al. data (2000)) causes our carbon tetrachloride (CCl₄) reconstruction to have a near-zero pre-industrial concentration of 0.025 ppt (0.025% of its peak value of 100ppt). We note that Walker et al. (2000) suggest zero pre-industrial concentrations before 1910, although the lowest empirical evidence from firn records suggest <5ppm ppt (Butler et al., 1999) or 3-4ppt as measured by S. Montzka for 1863 firn air and reported in Liang et al. (2016)”

Comment 26: lines 717-721 and Figure 9: the reason why the early part of the CMIP6 CO₂ trend is smoother than the CMIP5 trend whereas the early part of the CMIP6 N₂O trend is less smooth than the CMIP5 trend is unclear to me. Could this choice be commented?

Reply 26: This was not a deliberate “choice” but is rather an outcome of the chosen methodology. The CMIP5 extension of the RCP histories was an assemblage of existing timeseries (see Meinshausen et al. 2011). The CMIP6 derivation is much different by using raw measurement data points to derive a global field of concentrations and then back out the global mean. Thus, there was no deliberate choice involved regarding the smoothness of the timeseries in regard to CMIP5.

Comment 27: lines 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.

Reply 27: We followed the suggestion to move the Figures 10, 11, 13 and 14 into the Supplementary Appendix B. In regard to Figure 12, we opted for also moving this to the Appendix and not overloading the figure by additional timeseries. We however show the ensemble mean of the bottom panel in former Figure 12 in the CO₂ overview (new Figure 9, panel b).

Comment 28: lines Section 5.5: the comparison with other literature studies lacks priorities in terms of radiatively most important species and a check of the independence

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of the data used for evaluation with respect to those used to generate the assimilated fields. For example, the CO₂ and CH₄ 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 Buizertpdf). On the other hand, the comparison with early CO₂ atmospheric data (Keeling et al., 1976) is not commented. I'm surprised that the Cape Grim air archive data are not commented for N₂O (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.

Reply 28: Thank you for those helpful comments and clarifications. We now honored the origin of the CO₂ and CH₄ timeseries by labelling them "Petrenko-2010" and included them in the source Table copied above. Furthermore, we now inserted a new section as comparison to the mostly independent Keeling Scripps CO₂ station data before 1984 (exception is Mauna Loa of which we use the annual averages) – see Figure X copied above. We inserted the Park et al. Law Dome firn data in Fig 6 panel e. We added the Newland Halon data on the UEA scale to the Halon factsheets and referred to it in the text, such as: "Halon-2402 is also an illustration of how big differences in some measurement scales can potentially be. The Cape Grim data analysed by Newland with a volumetric UEA scale indicates 10-15% lower mixing ratios (Fig. 33f) (Newland et al., 2013)."

Comment 29: 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)?

Reply 29: We now increased the strength of the triangular data symbol, which is now labelled Petrenko-2010 in the CO₂ overview figure. With a vector-graphic reproduction of the figures in the final manuscript, we hope that the data is better visible. Regarding the differences: Yes, the Petrenko-2010 data is in line with the seasonal cycle for high

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northern latitudes. It might be a different story for the variations seen in the CH₄ record from Petrenko/Buizert, which seems to show a higher year to year variability than our data suggests. For example, the jump from December 1955 CH₄ mixing ratios of 1251ppb to January 1956 mixing ratios of 1307 is not reproduced by our data. (see new Figure 11f, former Figure 4f).

We now clarified the text so that the mentioned differences between the high-northern seasonal cycle and the northern-hemispheric average are not misunderstood as inconsistent. The new text reads: "For CO₂, the Petrenko data set has, as expected for the high northern latitudes, a very strong seasonal cycle, consistent with our less pronounced northern hemispheric-average cycle, as the data represents higher northern latitudes (Fig. 9f, g, and h). The long-term mixing ratio trend over time in the Petrenko CO₂ record seems similar to the global CMIP5 data set which in turn was based on previous Law Dome data, indicating a slight local maximum in 1890 and lower 1940s plateau (cf. Fig. 9g and Fig. 15)."

Comment 30: 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?

Reply 30: If a wider age distribution were assumed in the analysis of Butler then the derived CCl₄ history would indeed be consistent with a later onset, and steeper increase towards higher values. So, yes, a wider air age spread assumption in the Butler methodology would in this case close the gap towards the Velders et al. 2014 dataset. We added a sentence in that regard, reading: "The difference between the Butler and Velders datasets can probably be explained by the wider firn air age distribution in the study by Butler."

Comment 31: lines Section 6: major additional uncertainties for the early part of CMIP6 historical simulations such as the lack of constraints on nineteenth century CO₂ meridional gradients could be mentioned. Tables 2, 4, 5, 6, 7, 8, 9, 11 (technical documen-

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tation) could be placed in a Supplement

Reply 31: We structured the limitations section now with clear subheadings and added that particular point by saying “More generally, further research into observational and modelling-derived constraints regarding pre-1950 latitudinal gradients of CO₂ could allow future studies to go beyond our simplified assumption of a zero pre-industrial gradient in the light of the uncertainty.” In section 6.6 We would prefer the keep the tables with the data descriptions in the main manuscript so that the full list of references is retained – given the importance of many people’s data contributions. However, we are happy to reconsider based on the editor’s guidance.

Technical corrections:

Comment 32: lines 60, 708, 709 etc. and references: Meinshausen et al., 2011, 2011a and 2011b seem to be the same article

Reply 32: Thanks. Corrected.

Comment 33: lines 158-159: the first figure quoted in the manuscript should be Figure 1 rather than Figure 22

Reply 33: Thanks. Corrected.

Comment 34: 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.

Reply 34: Suggestion implemented.

Comment 35: line 230: define EOF notation at first use

Reply 35: Done.

Comment 36: line 595: is it really needed to quote Eyring et al., GMDD, 2015 instead of Eyring et al., GMD, 2016?

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Reply 36: No. Thanks. We updated all Eyring references to the final GMD manuscript.

Comment 37: lines 799-800: check the writing. This sentence seems contradictory with lines 167- 171, 801-802 and 824-829.

Reply 37: Thank you. We corrected the sentence and clarified the difference between the NOAA MBL and other NOAA products.

Comment 38: lines 904-905: Trudinger et al. (2004) is not more recent than WMO (2014) and Velders et al. (2014)

Reply 38: Thank you. The “more recent” statement referred to the Butler et al. 1999 study that WMO and Velders are based on. Sentences corrected to read now: “Here, we follow again the WMO (2014) and (not independent) Velders et al. (2014) reconstruction that are based on Butler et al. (1999) firn reconstructions. However, we note that the more recent Trudinger et al. (2004) CH₃Cl reconstruction indicates both a significantly lower mixing ratio for southern latitudes in the 1970s and a smoother increase compared to the more sudden rise of mixing ratios around 1940 as implied in this study (Fig. 29g).”

Comment 39: lines 1408-1410: incorrect list of authors

Reply 39: Our apologies. Corrected.

Comment 40: Figure 1: horizontal scale issue for panel c. A complete reference should be provided for each dataset. I saw only CO₂ data (no CH₄ and N₂O data) in Rubino et al. (2013), and Table 1 mentions different references for Law Dome CH₄ and N₂O data.

Reply 40: Thanks. We changed Figure 1 (see above) and corrected the references.

Comment 41: Figure 6: panels g, i, k, m, o, q would be much easier to read if the horizontal scale started in 1850 or 1900

Reply 41: Thanks. We will adapt the figure correspondingly.

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Comment 42: Figure 12: I can't see the 12 five lines mentioned in the caption, and the shaded areas are not described in the caption

Reply 42: Thanks. We now describe the shaded areas (they are the min-max ranges over those 12 lines) and hope that the vector graphic figure will come out more clearly.

Comment 43: Figure 15: wrong reference for the CH₄ "NEEM" scenario (see Supplement of Buizert et al. 2012, Section 2.4.2 in file Supplement Buizertpdf, and file SCENARIO_NEEM2008_....xls, CO₂ and CH₄ scenarios were made by Vas Petrenko). The NOAA global mean and WDCGG global mean results should be made easier to distinguish.

Reply 43: Thanks. Apologies. We corrected the data label to Petrenko.

Comment 44: 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

Reply 44: Thanks. We do include these references now in the reference list.

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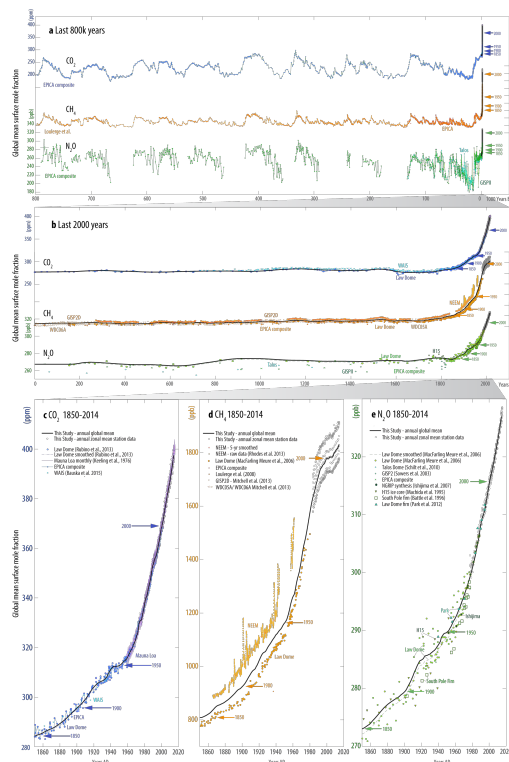


Fig. 1. New Figure 6

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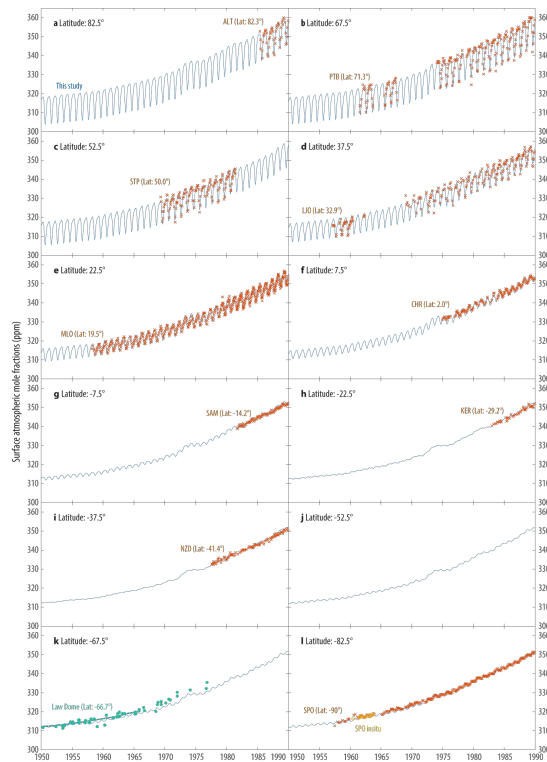


Fig. 2. New Figure 7

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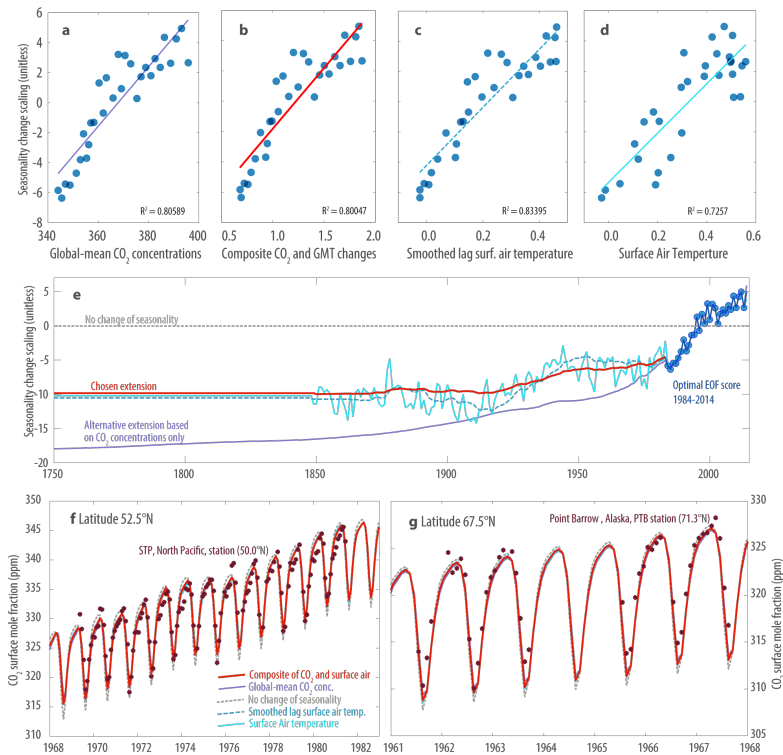


Fig. 3. New Figure 5

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Table 12 – Description of data labels shown in Factsheets, namely Fig. 9, Fig. 11, Fig. 12, and Appendix A with Fig. 20 to Fig. 29.

LABEL	GASES	DESCRIPTION / SOURCE
NOAA_SURFACE_FLASK	CO ₂	Atmospheric Carbon Dioxide Dry Air Mole Fractions from the NOAA ESRL Carbon Cycle Cooperative Global Air Sampling Network, 1988-2014, Version: 2015-08-03surface flask, available at data: ftp://ftp.cmdl.noaa.gov/data/trace_gases/2/surface/flask/ (Dlugokencky, 2015b)
NOAA_SURFACE_IN Situ	CO ₂	Atmospheric Carbon Dioxide Dry Air Mole Fractions from quasi-continuous measurements at Barrow, Alaska, Mauna Loa, Hawaii, American Samoa, and South Pole, 1973-2013, National Oceanic and Atmospheric Administration (NOAA), Earth System Research Laboratory (ESRL), Global Monitoring Division (GMD), Carbon Cycle Greenhouse Gases (CCGG), Version: 2014-11-10, available at: ftp://ftp.cmdl.noaa.gov/data/trace_gases/2/surface/ (NOAA ESRL GMD, 2014a, b, c, d)
NOAA_SURFACE_FLASK	CH ₄	Atmospheric Methane Dry Air Mole Fractions from the NOAA ESRL GMD Carbon Cycle Cooperative Global Air Sampling Network, 1988-2014, Fileversions: 2015-08-03 available at: ftp://ftp.cmdl.noaa.gov/data/trace_gases/4/flask/ (Dlugokencky, 2015b)
HATS_GLOBAL_COMBIN ED	N ₂ O, CO ₂ , CFC-11, CFC-113, CFC-12, SF ₆	Combined data from the NOAA/ESRL Global Monitoring Division and two or more measurement programs: WofO, Aug 10, 2015 2:40:55 PM available at: ftp://ftp.cmdl.noaa.gov/hats/combined/HATS_global_N2O.txt , ftp://ftp.cmdl.noaa.gov/hats/combined/HATS_global_F113.txt , ftp://ftp.cmdl.noaa.gov/hats/combined/HATS_global_F11.txt , ftp://ftp.cmdl.noaa.gov/hats/combined/HATS_global_F12.txt , ftp://ftp.cmdl.noaa.gov/hats/combined/HATS_global_SF6.txt , ftp://ftp.cmdl.noaa.gov/hats/combined/HATS_global_CO2.txt
MONITZKA_NOAA_GMD	CO ₂ , CFC-11, CFC-113, CH ₂ Cl ₂ , CH ₂ Cl, CH ₃ Cl, HCFC-22, HCFC-141b, HCFC-142b, HFC-134a, HFC-152a, HFC-32, HFC-125, HFC-143a, HFC-365mfc, HFC-227ea, Halon-1211, Halon-1301, Halon-2402	Flask data provided from the Global Monitoring Division of the National Oceanic and Atmospheric Administration's Earth System Research Laboratory (NOAA/ESRL/GMD) as a result of analyses on gas chromatography with mass spectrometry instrumentation. Principal investigators S. Montzka and James W. Elkins. Version: 13 Nov 2015. Data available at: ftp://ftp.cmdl.noaa.gov/hats/flask/113/flask/GCMS/CFC113_GCMS_flask.txt , ftp://ftp.cmdl.noaa.gov/hats/flask/113/flask/GCMS/CH3COCl3_GCMS_flask.txt , ftp://ftp.cmdl.noaa.gov/hats/flask/113/flask/GCMS/CH3Br_GCMS_flask.txt , ftp://ftp.cmdl.noaa.gov/hats/flask/113/flask/GCMS/CH3I_GCMS_flask.txt , ftp://ftp.cmdl.noaa.gov/hats/flask/113/flask/GCMS/CH2Cl2_GCMS_flask.txt , ftp://ftp.cmdl.noaa.gov/hats/flask/113/flask/GCMS/CH2Cl_GCMS_flask.txt , ftp://ftp.cmdl.noaa.gov/hats/flask/113/flask/GCMS/CHCl3_GCMS_flask.txt , ftp://ftp.cmdl.noaa.gov/hats/flask/113/flask/GCMS/HFC142B_GCMS_flask.txt , ftp://ftp.cmdl.noaa.gov/hats/flask/113/flask/GCMS/HFC134a_GCMS_flask.txt , ftp://ftp.cmdl.noaa.gov/hats/flask/113/flask/GCMS/HFC152a_GCMS_flask.txt , ftp://ftp.cmdl.noaa.gov/hats/flask/113/flask/GCMS/HFC32_GCMS_flask.txt , ftp://ftp.cmdl.noaa.gov/hats/flask/113/flask/GCMS/HFC125_GCMS_flask.txt , ftp://ftp.cmdl.noaa.gov/hats/flask/113/flask/GCMS/HFC143a_GCMS_flask.txt , ftp://ftp.cmdl.noaa.gov/hats/flask/113/flask/GCMS/HFC365mfc_GCMS_flask.txt , ftp://ftp.cmdl.noaa.gov/hats/flask/113/flask/GCMS/Halon1211_GCMS_flask.txt , ftp://ftp.cmdl.noaa.gov/hats/flask/113/flask/GCMS/Halon1301_GCMS_flask.txt , ftp://ftp.cmdl.noaa.gov/hats/flask/113/flask/GCMS/Halon2402_GCMS_flask.txt
AGAGE_GC-MS-MONTHLY	CFC-11, CFC-12, CH ₂ Cl ₂ , CO ₂ , N ₂ O, CFC-113, CH ₂ Cl, CHCl ₃	Chemical species measured by AGAGE GC-ECD/ID/MSD system. Version 20 June 2015. Data available at: http://agage.eas.gatech.edu/data_archive/agage/gc-ms/monthly/ (Cunnold et al., 2002; Cunnold et al., 1997; Fraser et al., 1996; O'Doherty et al., 2001; Pinn et al., 1991; Pinn et al., 2005; Pinn et al., 2001; Strömberg et al., 2005; Strömberg et al., 1998)
AGAGE_GC-MS-MONTHLY	HFC-134a, HCFC-22, HFC-141b, HCFC-142b, CH ₂ Cl, CH ₂ Br, Halon-1211, Halon-1301	Chemical compounds measured by AGAGE GC-MS (MS) system. Version 20 June 2015. Data available at: http://agage.eas.gatech.edu/data_archive/agage/gc-ms/monthly/

Fig. 4. New Table 12 - Page 1

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LABEL	GASES	DESCRIPTION / SOURCE
AGAGE_GC-MS-MEDUSA-MONTHLY	1301, HFC-152a, CH ₂ Cl ₂ , CH ₂ Cl, CHCl ₃ , CFC-11, CFC-12, CFC-113, CFC-114, CFC-115, HCFC-22, HFC-141b, HFC-142b, HFC-125, HFC-134a, HFC-152a, HFC-365mfc, HFC-227ea, HFC-4310mee, Halon-1211, Halon-1301, Halon-2402, CH ₂ Cl, CH ₂ Br, CH ₂ Cl ₂ , CO ₂ , SF ₆ , SO ₂ , NF ₃ , PFC-14, PFC-116, PFC-218, HFC-32, HFC-143a, HFC-227ea, HFC-236fa, HFC-245fa, HFC-237ca, CFC-113, CFC-115, HCFC-141b, CDM, CFC-12, HCFC-142b, CH ₂ Cl ₂ , H-1211, H-1301, H-600, CH ₂ Br, SF ₆ , CFC-11, CFC-12, CFC-113, CO ₂ , CH ₂ Cl ₂ , HFC-134a, CO ₂ , CH ₄	Chemical compounds measured by Medusa GCMS system. Version 20 June 2015. Data available at: http://agage.eas.gatech.edu/data_archive/agage/gc-ms/medusa/monthly/ (Pinn et al., 2000a)
MONITZKA_NOAA_ODS UPDATE 5/2014	HFC-125, HFC-134a, HFC-152a, HFC-227ea, HFC-236fa, HFC-245fa, HFC-237ca, CFC-113, CFC-115, HCFC-141b, CDM, CFC-12, HCFC-142b, CH ₂ Cl ₂ , H-1211, H-1301, H-600, CH ₂ Br, SF ₆ , CFC-11, CFC-12, CFC-113, CO ₂ , CH ₂ Cl ₂ , HFC-134a, CO ₂ , CH ₄	Data from 5/2014 update of NOAA compilation of monthly global mean mixing ratios, made available on web as 2014 update total Cl Br & F air by S. Montzka at: ftp://ftp.cmdl.noaa.gov/hats/total_Cl_Br/
MARTINERIE-2010	SF ₆ , CFC-11, CFC-12, CFC-113, CO ₂ , CH ₂ Cl ₂ , HFC-134a, CO ₂ , CH ₄	Monthly high-latitude NH data by Patricia Martinerie, made available as supplementary by Buzent et al. (2012) in Res SCENARIO_NEMEM8_XX.txt
PETRENKO-2010	SF ₆ , CFC-11, CFC-12, CFC-113, CO ₂ , CH ₂ Cl ₂ , HFC-134a, CO ₂ , CH ₄	Monthly high-latitude NH data by V. Petrenko, made available as supplementary by Buzent et al. (2012) in Res SCENARIO_NEMEM8_CO2.txt and SCENARIO_NEMEM8_CH4.txt
WDCGG	CO ₂ , CH ₄ , N ₂ O	Data synthesis as available from the World Data Centre of Greenhouse Gas Emissions (Tsatsumi, 2009), available at: http://ds.data.jma.go.jp/gmd/wdcgg/ Version: v2, monthly, 2015/11/09.csv, ch4_monthly, 2015/11/09.csv and n2o_monthly, 2015/11/09.csv
NOAA_MBL	CO ₂ , CH ₄	NOAA Greenhouse Gas Marine Boundary Layer Preference, derived from atmospheric carbon dioxide, methane and nitrous oxide mixing ratios from the NOAA ESRL Carbon Cycle Cooperative Global Air Sampling Network, available at http://www.esrl.noaa.gov/gmd/ghg/ml/ for CO ₂ and CH ₄ . Zonal means for SH and NH, as well as global means. File creation dates: 2016-02-11
CMP5_HIST.	Many	The global-mean annual average concentrations that were used as default recommendation for concentration-driven runs in the CMP5 experiment (Meinhausen et al., 2011)
CMP5_CTRL.	Many	The global-mean annual average concentrations in 1850 that were recommended as prescribed concentrations in the CMP5 experiment (Meinhausen et al., 2011)
FIRM - MONITZKA-2009	CFC-12, HFC-134a, HCFC-22, and CH ₂ Cl ₂	"Southern Hemisphere atmospheric trace gas histories used in the analysis of firm air" data compiled by Montzka in 2009 (available at: ftp://ftp.cmdl.noaa.gov/hats/firmair/) in file "SH Atmospheric Trace Gas Histories.txt", based on several earlier studies (Buzent et al., 1998; Elkins et al., 1983; Montzka et al., 1993; Montzka et al., 1996; Montzka et al., 2000; Pinn et al., 2005) and is reported in Aydin et al. (2010) for CFC-12 and undating (Montzka et al., 2010)
WMO (2014)	CFC-11, CFC-12, CFC-113, CFC-114, CFC-115, CO ₂ , CH ₂ Cl ₂ , HCFC-22, HFC-141b, HFC-142b, Halon-1211, Halon-1301, Halon-2402, CH ₂ Br, CH ₂ Cl, CHCl ₃ , SF ₆ , SO ₂ , NF ₃ , PFC-14, PFC-116, PFC-218, HFC-32, HFC-143a, HFC-227ea, HFC-236fa, HFC-245fa, HFC-237ca, CFC-113, CFC-115, HCFC-141b, CDM, CFC-12, HCFC-142b, CH ₂ Cl ₂ , H-1211, H-1301, H-600, CH ₂ Br, SF ₆ , CFC-11, CFC-12, CFC-113, CO ₂ , CH ₂ Cl ₂ , HFC-134a, CO ₂ , CH ₄	Data from Table SA2 in the 2014 Ozone Assessment (WMO, 2014), starting with 5-year intervals from 1955 to 1980 then annually. We interpolated the data to annual values using a local polynomial regression between 1955-1980
WMO2014AGAGE 'LATE'EARLY'	HFC-125, HFC-134a, HFC-152a, HFC-227ea, HFC-236fa, HFC-245fa, HFC-237ca, CFC-113, CFC-115, HCFC-141b, CDM, CFC-12, HCFC-142b, CH ₂ Cl ₂ , H-1211, H-1301, H-600, CH ₂ Br, SF ₆ , CFC-11, CFC-12, CFC-113, CO ₂ , CH ₂ Cl ₂ , HFC-134a, CO ₂ , CH ₄	The network average global-mean mole fractions from the AGAGE network as shown in the WMO (2014) Ozone Assessment Report (WMO, 2014)
WMO2014NOAA	HFC-125, HFC-134a, HFC-152a, SF ₆ , CFC-11, CFC-12, CFC-113, CO ₂ , CH ₂ Cl ₂ , HFC-134a, CO ₂ , CH ₄	NOAA global-mean annual average time series as shown in WMO Ozone Assessment Report (WMO, 2014)
WMO2014PFC	C ₂ F ₆ , C ₃ F ₈ , C ₄ F ₁₀ , C ₆ F ₁₄	PFC data compiled and shown in WMO Ozone Assessment Report (WMO, 2014)

Fig. 5. New Table 12 - Page 2

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LABEL	GASES	DESCRIPTION / SOURCE
AGAGE - GLOBAL MONTHLY AVERAGE	HFC-23, HFC-125, HFC-134a, HFC-152a, HFC-227ca, HFC-236b, HFC-245a, HFC-365mfc, HFC-22, HCFC-141b, HCFC-142b, H-1211, H-131i, CH ₃ B, CH ₃ Cl, CH ₃ Cl ₂ , C ₂ Cl ₂ , C ₂ F ₂ , SF ₆ , SO ₂ , PFC-14, PFC-116, PFC-218, CFC-113, CFC-114, CFC-115, HFC-4310mcc	Monthly global mean of baseline data derived from AGAGE measurements based on AGAGE GC-MS/MS measurements (from 2004 to current) from file global_mean.ms.M available at: http://agage.ces.gatech.edu/data_archive/global_mean/
BINNED ANNUAL OBSERVATIONS	All	These are the monthly averages for each 15-degree zonal mean derived from the analyzed station data points (with three-digit station names provided in the top left corner of panel f of each factsheet). An "n/a" indication behind the latitude indicator means that not enough raw station data points were available to create zonal means for that latitude. The estimate of the latitudinal gradient is then based on the remainder available latitudinal bands.
Other labels, namely: Montzka et al. (2014) Velders et al. (2014) Munnich et al. (2015) Trudinger et al. (2018) Iry et al. (2017) Worton (2007) Butler et al. (1999) Arnold et al. (2013) Arnold et al. (2014) Vollmer et al. (2014) Oram et al. (2012) Walker et al. (2000)	Various	See respective literature studies (Arnold et al., 2013; Arnold et al., 2014; Butler et al., 1999; Iry et al., 2017; Montzka et al., 2014; Munnich et al., 2015; Newland et al., 2013; Oram et al., 2012; Trudinger et al., 2016; Velders et al., 2014; Vollmer et al., 2016; Walker et al., 2000; Worton et al., 2007). Note: the CCL data by Walker et al. is used as 1910 to 1950 amendment to the Velders et al. timeseries.
NEWLAND ET AL. (2013)		

Fig. 6. New Table 12 - Page 3