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Interactive comment on "Continuous records of the atmospheric greenhouse gases CO_2, CH_4, and N_2O and their radiative forcing since the penultimate glacial maximum" *by* Peter Köhler et al.

Anonymous Referee #3

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Manuscript summary: The authors combine recent direct atmospheric observations and available records from several ice cores to produce smoothed splines of CO2, CH4 and N2O back to 156 kyr BP. They also calculate changes in radiative forcing due to variation in each of these gases, for future use in paleoclimate modeling.

Overall assessment: It is unclear to me whether this is a contribution that merits publication on its own. There are no original data presented, and the scope of the work seems limited. This work may be better suited for a supplementary section to a future paleoclimate modeling study that the authors would conduct. However, my experience

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Discussion paper

with ESSD is limited and the editor would be the better judge of this. If it was clear that the results would definitely be used by multiple groups/multiple paleoclimate modeling studies, the case for independent publication would be stronger. Below I provide some suggestions on how this work could be improved / expanded to a point where it would more likely merit independent publication.

Major comments:

Stopping the smoothed record at 156 kyr seems somewhat arbitrary and strongly limits the usefulness of the work, in my opinion. A quick look at the EDC ice core CO2 data reveals that average Δt is actually lower ($\approx$630 yr) in the 700 – 800 kyr interval than in the 140 – 160 kyr interval ($\approx$1500 yr). I would recommend extending the spline for the entire duration of the EDC record for all 3 gases, even if the spline has to be discontinuous for a gas like N2O. This would add a lot of value to the splines, as it would allow them to be used for comparison of multiple glacial terminations as well as peak interglacials that may be analogues for near-term future climate (e.g., MIS 11).

Considering that it is very well established that there is a substantial inter-polar CH4 gradient (IPG), the authors should either present an additional northern hemisphere CH4 spline or present a global average estimate instead of a southern-only spline. Although the difference in radiative forcing would be relatively small, a global average would still be more useful for models. The authors could consider using the available information about the IPG from the last glacial cycle, and extrapolating this further back in time while accounting for additional uncertainties.

I was surprised that continuous CH4 data from WDC (Rhodes et al., 2015) were not used in this compilation. Surely, these data would provide the best temporal resolution for a paleoclimate model run? I would recommend the authors include these data, accounting for any offsets from discrete CH4 data sets and associated uncertainties.

Comparing tables 3, 6 and 8 to associated text, it seems that CO2, CH4 and N2O uncertainties associated with offsets between different ice core records (and in the

case of N2O, possible in situ production) were not included in the uncertainties for the splines. As these are all components of the true overall uncertainties in absolute CO2, CH4 and N2O atmospheric histories, they need to be included. Such a more thorough treatment of uncertainties would also make the compilations more useful for comparison with future ice core measurements.

Minor comments:

While the writing overall is quite clear, there are multiple small mistakes in the English. This manuscript would benefit from careful editing from a native English speaker.

The title is somewhat misleading. Given the title, I was expecting to see new measurements of the 3 gases in ice cores done via a continuous technique. I would recommend revising the title to something like: "A smoothed atmospheric history of CO2, CH4 and N2O compiled from available ice core data to 156 kyr BP"

I found the discussion on page 4 that involves equations 1 and 2 somewhat confusing and circular. First, Pcutoff is defined in terms of lambda, and then lambda is defined in terms of Pcutoff. Which is prescribed first? Later in the manuscript, it becomes clear that Pcutoff is prescribed and to a large degree determines the smoothing, but perhaps it would be best to explain this more clearly on page 4.

On a related note, it would be useful to explain the choices of Pcutoff better – they seem generally reasonable in the tables given the Δt values for different intervals, but what is the process for assigning the Pcutoff values?

Do EDML CH4 data have better or comparable resolution to EDC for some of the time intervals considered? If so, they should be included. It may be useful to include these regardless to help in estimating uncertainties arising from offsets between different data sets / cores.

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