Earth Syst. Dynam. Discuss.,
doi:10.5194/esd-2017-11-RC2, 2017


Earth System Dynamics Discussions

# *Interactive comment on* "Emission metrics for quantifying regional climate impacts of aviation" *by* Marianne T. Lund et al.

**Anonymous Referee #2**

Received and published: 16 March 2017

This manuscript provides calculations of GWP and GTP for aviation emissions, focusing on aerosol direct radiative forcing impacts, GHG impacts, and impacts via contrail-cirrus formation. The authors also project temperature estimates using ARTPs based on published regional climate sensitivities, and evaluate these in comparison to their own estimates computed using 1-3 different models. The work considers emissions from 6 aggregate continental-scale regions, and climate impacts across 4 latitudinal bands.

Overall, the results of this type of study are potentially valuable for estimating the climate impacts of current and future aviation scenarios. The evaluation of the ARTP concept itself, for a vertically distributed source, is also of interest on its own. The paper is well organized and generally easy to follow. The manuscript will be suitable for publication after addressing a few comments below, most of which are made in or-



der to provide some clarifications or additional information that will make the results presented here more broadly understandable and applicable.

Major Comments:

142: The RF kernels of Samset and Myhre (2011) are valuable because they are vertically distributed. But they are also limited in their spatial coverage. How do the authors map between the regions in their study and those in Samset and Myhre (2011)? For example, it would seem the latter does not provide any results for the author's SAS and SPO regions. A more complete 2D spatial mapping of aerosol direct radiative forcing efficiencies is provided in Henze et al. (ES&T, dx.doi.org/10.1021/es301993s, 2012). Perhaps results from these two studies could be combined to provide a more complete analysis? Or at least findings from the latter could be used to provide some sense of the uncertainty involved in using only the Samset and Myhre regions as the basis for the present work.

180-185: This argument feels a bit thin, given that some aspects of aerosol cloud interactions are at least better known that others. At the very least, could uncertainties owing to these processes be carried through the calculation, so that we know when uncertainties in these effect may alter the sign of the next outcome?

For Fig 4: why compare ARTP(20) and ARTP(100), when a more direct and fundamental comparison would be to just consider the RCS's? The RCS is what other people will need, if they are to use the results from this study themselves to calculate ARTPs. At the very least it would be quite useful to compare the RCS values in addition to the existing figures using ARTP in particular years.

General: For other people to make use of these results, it is useful to provide more information on the aviation emissions used in this study. The authors should provide a table of emissions by species and region, and they should provide separate total for emission by takeoff vs cruise altitudes. While it would be great if they could provide metrics broken down by the later category as well, I'd guess that would involve repeat-

ing a lot of calculations. But at least providing the details of the inventory they used would allow future users to be able to scale evaluations of the climate impacts of their own inventories accordingly, given some knowledge of how the authors' inventory was distributed vertically.

Minor comments:

35-38: True, but this is evident from the fact that the RCS's in e.g. Shindell 2012 are not uniform. So it is a bit odd to place this in the abstract, although I agree the application does being attention to the issue.

40-41: The feels a bit obvious (biggest emissions have the biggest impact) âĂŤ would discussing the impact per emission be of more interest?

66: This statement is missing references.

78: The phrase "in a grid cell" is vague (we don't know yet how big your model grid cells are) and also ambiguous with regards to whether you are referring to grid-scale changes in temperature or grid-scale changes in emissions.

83-84: Can the authors reference any in particular?

91-93: How much uncertainty / error can this aggregation lead to?

105-110: See also Sand et al., Nature Climate, doi:10.1038/nclimate2880, 2015.

183: I don't understand what mechanisms this refers to. Please be more specific and provide references.

Table 1: could you list NOx in the first half at the bottom the list of species, so that it is easier to compare these numbers to the results for NOx from other studies listed below?

Table 1: I must be missing something âĂŤ the GTP and GWP metrics are computed by emitted species (i.e., SO2 instead of sulfate), yet the authors report values for nitrate



(Table 1), and these are reported separately from NOx emissions, even though nitrate is formed secondarily in the atmospheric from NOx. Can the authors please explain this more?

Fig 2: Please explain the difference between the color bars vs star points in panels E and F, and define O3 vs O3PM in the figure caption itself.

381: Also Lacey et al. (PNAS, doi:10.1073/pnas.1612430114, 2017) used ARTPs to investigate this for cookstove emissions.

132: Is this perturbation positive or negative? Does it make a difference, for SO2 and NOx?

Fig 2: Given the factor of 0.5 in Eq 2, why isn't O3PM 50% that of the CH4 in panels E and F? Is this a consequence of the spatial re-scaling from Fry 2012? If so, I would have expected it to be less than 50% in some regions and greater than 50% in others.

222: Why not use the RCS for sulfate for sulfate, rather than for the mean of CO2 and sulfate? What RCS is used for nitrate (although not clear how nitrate is treated anyways)?

492-497: This statement is a convolution of two issues that could be separated, which are that the RF of O3 per ppb is horizontally and vertically variable, and that the climate response to this RF is also variable.

536-538 and 550-551: Fig 4 only shows the normalized results, so it is hard to know how much of an overestimate the authors are talking about here. Can they also provide the absolute results?

Did the authors consider using ARTPs for the land-only response from Shindell 2012?

Technical corrections:

87: as a bridge

133: sulfer dioxide

137: each region are

303: in the present analysis we

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