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

Anonymous Referee #1

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Review of "Emission metrics for quantifying regional climate impacts of aviation"

by Marianne T. Lund et al.

This manuscript attempts to calculate Global Warming Potentials (GWP), Global Temperature Potentials (GTP) and Absolute Regional Temperature change Potential (ARTP) for aviation emissions. This is a difficult paper to put together since there is essentially I think an important negative conclusion that this method does not work that well for aviation emissions. I would like to see a bit more uncertainty analysis to quantify where the problems are. I think a bit more discussion of the physics of the climate system would help the reader understand the problems with this method. This paper could be publishable with some important revisions.

Generally, it looks like the method described is highly model dependent, which is not

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really obvious until the end of the paper, and there is not really quantification of the uncertainty. I think this could be better woven throughout. It would be nice to comment a bit more on how Stohl et al 2015 and Shindell 2012 apply to these results: I am pretty convinced that for small perturbations and for non-gaseous species like contrails that these results may not apply.

I think that this work could also use a better focus on some of the physical aspects of the climate system and the feedbacks that might or might not operate. And at a minimum I would like to see quantification of uncertainty in the different parts of the ARTP terms.

Some of the figures (e.g. Fig 3 and S1) could use some clarification as noted below.

Specific comments are below.

Page 4, L108: how does any of this work account for effects on the coupled climate system? Regional forcing can show up very differently in surface temperature depending on whether the energy goes into sensible heat, latent heat, or evaporation, and will depend on surface type and coupled modes of the climate system. For example: cooling the N. Atlantic deep water formation region with contrails may simply cool deep water and not be felt at the surface. Or it may alter the formation of deep water itself. Don't you need a coupled climate model to do this?

Page 4, L110: I'm not sure you can do this with a CTM. The circulation doesn't adjust.

Page 5, L114: again: surface temperature response depends on the surface type. Is that accounted for?

Page 5, L117: how uncertain are the regional climate sensitivities?

Page 7, L173: I do not think you can throw out this term: it is a compensating effect or feedback. You could include the reduction in cirrus as a separate forcing, but now you are overstating the contrail impact.

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Page 7, L184: What are these uncertainties?

Page 7, L197: Does previous work show that this represents the physical response? I have a hard time believing it works for non-uniform emissions in the vertical, or for aviation emissions.

Page 7, L200: Are these done for contrails? This is a cloud forcing, which is very different than other forcings. For example: contrail forcing is a SW and LW forcing that depends strongly on surface albedo. Do these sensitivities take account of that? If not, I'm not sure the method is valid.

Page 8, L202: What does the statement in parentheses mean? What is a component? If the time is longer than the lifetime for a pulse emission, isn't the forcing gone?

Page 8, L207: This presumes that all RF is created equal with respect to sensitivity. I think this is valid for GHG in the IR. I'm not sure if it is valid for SW effects, and probably not for clouds.

Page 9, L229: So you are trying to put these uncertainties into 'efficacy', which would effect the contrails by a factor of 2, but then ignore it? I don't think that is appropriate. Efficacy might be a way to address some of the issues I am referring to with contrails in this context.

Page 9, L241: There are emissions scenarios for aviation that specifically address the non uniform growth of emissions and the efficiency improvements. Why not use them? There are already RF estimates in the literature with these scenarios.

Page 10, L270: I do not think the climate system is linear enough to small perturbations to make this a valid assumption. Can you provide a reference demonstrating that multiplying a forcing by an order of magnitude or more yields a linear scaling to the response to the small perturbation in a coupled climate model? I see now there is a reference below. Is it valid?

Page 10, L274: But isn't the temperature response the heart of this paper? Yet it is to be used ’with care’? I think you might need to quantify the potential uncertainties here.

Page 10, L278: I do not think figure S1 is correct. I do not see how a 2ppb ozone perturbation causes a 1K surface temperature change in Figure S1. Please clarify or justify that.

Page 11, L287: But I thought above the forcing was an impulse, I.e. Non- constant?

Page 11, L294: This is confusing regarding emission units: since they are rationed to CO_2, shouldn’t they be relative to aviation CO_2, then the comparison with aviation CO_2 is explicit. Pardon me if that is not the appropriate definition, but I’m not sure how to interpret the very large BC GWP number.

Page 11, L305: But does this treat compensating LW and SW effects which are a function of surface type?

Page 12, L323: Dry conditions in S. Asia? Really? In some seasons. What about the summer monsoon?

Page 13, L345: I’m not sure I agree with this. How do you deal with non local energy balances in each region?

Page 13, L364: Is it valid to apply RCS from one model to CTM results from another model? They might be vastly inconsistent, particularly in remote regions.

Page 13, L370: I’m not sure the remote effects being larger make physical sense. See next comment.

Page 14, L378: Here it looks like the GISS results are convolving the effect of transport with the effect of aerosols: they co-vary, but the aerosols do not cause warming. They just come in with warmer air. I think this highlights some of the problems with this methodology. I think you draw the wrong conclusion here.

Page 14, L390: Unless emissions occur in the stratosphere, in which case contrail formation drops.

Page 15, L406: what about significant changes in meteorology: warming reduces contrails, and also changes the tropopause height.

Page 16, L454: I'm not sure exactly what is being plotted in figure 3: please explain further and in a more detailed caption. Temperature change in each region by different emissions in each region? Are the asterix just a label for the bars or does their vertical position mean anything.

Page 17, L484: If previous studies only did surface emissions (e.g. Stohl et al 2015 and Shindell 2012), then how will some of the terms from these models be valid (e.g. GISS study with RCS I think it is)?

Page 18, L511: I could also argue using this figure that the ARTP concept only works within the main emissions region, and is off by 50% or more relative to a physical calculation.

Page 19, L528: I think this whole analysis highlights that the ARTP concept is very model dependent and not a general concept because of strong dependencies you have identified here.

Page 19, L546: I think you are going to have the same problem with contrails, which also have strong vertical effects, and regional forcing dependent on the climate system itself.

Page 19, L553: Can you do an uncertainty analysis here? Where are the largest uncertainties and model dependencies in the analysis.

Page 21, L589: Where do you demonstrate that the response is stronger than the forcing? And how does this analysis include effects of the large scale circulation and feedbacks? You are breaking most of those feedbacks with use of a CTM and unitless response functions from another model.

Page 21, L606: Add to this some scaling by a factor of 40 for emissions in here somewhere, and I think this is problematic.

Page 21, L608: At least I agree with the negative conclusion here: it is not clear to me that this concept has a lot of utility for aviation. Especially since you have left out aerosol indirect effects. Anything that involves clouds I think is highly problematic using this method. Would like to see a bit more uncertainty analysis to quantify where the uncertainty lies.

Page 28, L882: What are the units of BC? Per unit of fuel or something else? Not clear from the caption.

Page 32, L922: What are the units here? Mili-kelvin? Over what area? Do the regional colors (asterisk) correspond to the emissions colors? I'm not sure what is being plotted here.

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