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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of the absolute regional temperature potential" *by* D. T. Shindell

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I thank the referee for their review. His/her comments are given in italics, with replies below each one.

Anonymous Referee 3

The paper further develops and evaluates the Absolute Regional Temperature Potential (ARTP) and constitutes a timely contribution to the development and assessment of potential new climate metrics for evaluation and comparison of impacts of different emissions. The ARTP concept is the first attempt to develop a simple metric to be applied to emissions causing inhomogeneous forcing and use regional temperature change as the impact parameter in the metric definition. The paper makes use *of existing model simulations in a very clever way to evaluate the robustness of the RTP-coefficients calculated by the GISS model. The paper is well written and with some relatively minor modifications, outlined below, I recommend that it is accepted for publication in ACP.*

General comment: What is a metric? It is stated a few places in the manuscript (e.g. page 13814, line 18, and page 13815, line 18-19 – here with ref. to Shine et al., 2005) that a metric provides an estimate of the response (here dT) to a given radiative forcing. This is not correct as these (the GWP and the GTP) are emission metrics that gives the response to a given emission (i.e. a 1 kg pulse emission in the standard case). This is part of a general confusion (that should be easy to fix) in the paper about what is a metric (i.e. an emission metric). It is clear that the ARTP is such a metric while the RTP coefficients are not. The very first sentence in the Introduction introduces the ARTP while in fact it describes the RTP coefficients. However, the RTP coefficients has a large potential as part of an operational metric definition since the ARTP values can now be generated for specific sources by a combination of an off-line CTMs and the RTP coefficients. This needs to be clarified. One way to go about this is to define the first part of equation 1 as the definition of the ARTP, while the second part (the integral) is how it calculated operationally. Then there should be a generic discussion that the RTP coefficients can be obtained from idealized GCM experiments, while then the regional forcings (the Fi, i denotes the region) can be calculated from CTMs or offline GCMs, or taken from pre-calculated tables for LLGHGs (regional forcing per unit of emission).

The distinction between emission metrics and utilization of the ARTP method with a known forcing pattern rather than emissions is now made clear, as is the difference between the ARTP and the RTP coefficients.

Abstract: A caveat about the regional deviation of the RTP-coefficients of BC and ozone at high latitudes should be included.

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Added as suggested.

Page 13814 line 26: "Very few metrics have attempted to examine sub-global scales thus far". Note that Shine et al., (2005) and Lund et al. (2012) used sub-global scale information to derive a global metric but with a different approach, i.e. using local non-linear damage functions.

Agreed this is a good point; added as suggested.

Page 13815/16: Definition of the ARTP. The wording leading up to equation 1 could be interpreted as if eq. 1 is the definition of the ARTP. However, I believe that is not the case, eq. 1 is only a practical way to calculate it. The definition of the ARTP is the regional and annual mean temperature response in region j at a time t, to an emission in region k at time 0 (in the pulse case). If coupled GCMs were quick and cheap to run we would probably not use eq. 1 but rather the full GCMs.

This section has been revised and the definition of the ARTP is now clearly stated with regard to emissions.

Page 13816 line 4. In the introduction the term "RTP coefficients" is introduced, while here when the kx,y is described it is not used. To help the reader, the term RTP coefficients should be used here as well.

RTP coefficients now explicitly defined and used throughout (k's no longer used anywhere).

Page 13816, line 13. . . ..two exponentials represent the relatively rapid response of the land and upper ocean and the slower response of the deep ocean. . .. This is an over-interpretation of the two exponentials. These are just mathematical fittings and are not directly related to physical processes (e.g. Li and Jarvis, Clim. Dyn., 2009).

Agreed, changed from "represent" to "approximate".

Page 13816, line 18: The statement below about transient change is related to equa-

tion 2, but is only correct if the forcings are constant in time or the rate of change is equal in all transient cases. For a transient response this is not necessarily true and the transient sensitivity will depend on the rate of change of the forcing. Equation 1 (with the impulse response function) should be used for all applications with transient forcings. The approximate equilibrium response, or the transient response at a particular point in time, in any model (or for any chosen climate sensitivity) is simply the regionally weighted RF (the first term above) multiplied by the climate sensitivity (equilibrium or transient, as appropriate):

The discussion of the transient case has been deleted as I agree that the IRF should be used when the forcing history is known and the transient response is of interest. In the historical cases evaluated here with transient runs (IPSL and GISS), the full history is not available. The sensitivity used for those models is the sensitivity obtained from the response in the historical transient runs based on the linear temperature trend over the full run.

Page 13816, line 26. In my opinion the key point with the paper is to establish that the GISS based RTP-coefficients are relatively robust so that the ARTPs can be calculated from equation 1 using forcing estimates from any (off-line) model (relatively cheap to calculate) and the pre-calculated RTP coefficients. This point should be made much clearer when it is described how the ARTP is linked to the emissions (as it needs to be in order to be a real metric).

Agreed, clarified both that a primary aim of the paper is to evaluate the RTP coefficients and that using the forcing/response portion of the ARTP can save the expense of running a full climate model.

Page 13817, line 5: I found the text below very technical and I suggest that it should start with an introductory statement telling that now you will describe how the RTP coefficients were modified (i.e. let the reader know that this is about the RTP coefficients.) Since impulse response functions have been given in terms of response to *global mean forcing, the ARTP must weight the impact of forcing in different locations on the response region relative to the impact of global mean forcing on that region. I therefore give the regional response coefficients required for the ARTP calculation based on responses in the GISS model relative to the same model's global sensitivity in Table 1. Compared with Shindell and Faluvegi (2010), this representation normalizes by the global sensitivity rather than the local temperature response to global forcing (kGlobal,a). This is a better representation of the regional responses, as the kGlobal,a values incorrectly removed the regional inhomogeneity in sensitivity seen even for a globally uniform forcing.*

The text has been clarified as suggested to state that this discussion refers to how the RTP coefficients are constructed.

Page 13818, line 18. ". . . driven by historical changes in aerosols". I presume that all models were driven by the same changes in emissions of aerosols and aerosol precursors in these experiments. I little more detail should be provided.

Clarified what was used, but the models did not use the same emissions as that was not part of the protocol for experiments in support of the AR4 (it is for AR5 though).

Page 13818, line 25. It is stated that also indirect forcing of the aerosols are included. Does this include also the cloud-lifetime effect and the semi-direct effects of BC? It is important that the distinction between forcings and feedbacks are treated equal in all models. Is the forcing due to BC on snow included in any of the models?

Several of the models do include these indirect forcings, including BC on snow. As the evaluation tests the response to the diagnosed forcing, the key point is that all forcings that affect the temperature have to be included in the forcing diagnostic (which they are) rather than it being important that the models have the same forcings included I believe. It would be interesting to test the response to forcing caused by different processes, but doing so in a multi-model context is a topic for future work.

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Page 13819 line 3. It is unclear what is meant by "total linear trend". Does this mean that the present response relative to the pre-industrial for a transient simulation is used instead of the equilibrium response? If so then the response of e.g. the IPSL model of 0.89 K/Wm-2 is the transient sensitivity for this particular experiment? Please explain.

Revised to clarify this point saying the results are linear trends over the full length of transient simulations. So yes, the values for IPSL and GISS are the transient responses, which is now explicitly pointed out in the text, both that these are used and that these are more appropriate for the available transient experiments. There is no obvious systematic bias in either the IPSL or GISS results (Figure 1).

Page 13819 line 19-23. It is surprising that the GISS model does so well for the Arctic for the all-aerosol comparison. Is the Arctic RTP-coefficients for BC (negative, -0.17) used for BC forcing in this comparison or is it only the sulphate-based RTP-coefficients that are used?

Only the sulfate-based values given in Table 1 are used throughout. Arctic BC is very small, hence although the coefficient was negative for Arctic BC in our model runs, it has a minimal impact. In addition, that coefficient did not include the BC albedo forcing, which may make the response much more like that seen for sulfate.

Page 13821 line 5: Again, a metric relates emission to response. I suggest changing the first sentence to: This paper presents a revised ARTP metric for estimating the regional temperature response to emissions leading to inhomogeneous forcing.

Agreed, this text has been revised similar to the suggestion.

Page 13821 second paragraph in the Conclusions. The discussion of the role of uncertainty in the impulse-response function (or the climate sensitivity) is here limited to the ARTP. However, it will also affect the RTP (i.e. relative to a reference gas as CO2), but to a lesser extent. As the ART/RTP concept is likely to be used also relative to CO2, such a discussion should be added.

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Interactive Comment

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Agreed, added discussion on this as suggested.

Page 13821: The paper makes use of existing model simulations in a very clever way to evaluate the robustness of the RTP-coefficients calculated by the GISS model. An alternative way and maybe better(?) approach could be to run all the sensitivity simulations done with the GISS model through the other models and calculate RTP coefficients for all models. A comment about this option in the discussion section would be useful.

True. I've added a comment about this to the second paragraph in section 5.

Table 1. The RTP coefficients are dimensionless, while the caption indicates differently. Please clarify.

I've added that the coefficients are unitless to the title of the table as well. The values are described in the caption and title as W/m2 over W/m2, and hence are dimensionless.

Figure 1. It would of interest to know which point relates to which region. This can be given if e.g. the shape of the symbols relates to region while the colors relate to model.

The figure has been revised as suggested. Thank you for suggesting this improvement.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 13813, 2012.