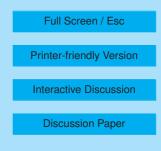**Atmospheric
Chemistry
and Physics
Discussions**

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

Anonymous Referee #3

Received and published: 29 June 2012

Review of the paper "Evaluation of the absolute regional temperature potential" by Drew Shindell, submitted for ACP.

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

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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 off-line GCMs, or taken from pre-calculated tables for LLGHGs (regional forcing per unit of emission).

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

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.

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.

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.

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).

Page 13816, line 18: The statement below about transient change is related to equation 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):

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).

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.

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.

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?

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.

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?

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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.

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 CO_2), but to a lesser extent. As the ART/RTP concept is likely to be used also relative to CO_2, such a discussion should be added.

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.

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

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.

Minor: Misprint: page 13814, line 24: addition $\Rightarrow$ additional

Page 13819 line 29: Should be RTP-coefficients, not ARTP- coefficients to be consistent with the rest of the paper.

References:

M. T. Lund et al., How much information is lost by using global-mean climate metrics? An example using the transport sector, Climatic Change, 2012, (doi:10.1007/s10584-011-0391-3)

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Shine KP, Berntsen TK et al (2005) Scientific issues in the design of metrics for inclusion of oxides of nitrogen in global climate agreements. Proc Natl Acad Sci U S A 102(44):15768–15773

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

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