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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 Dr. Peters for his review. His comments are given in italics, with replies below each one.

This is a relevant and interesting article, and should be published after appropriate modifications. In a few places the paper lacks some explanations and can be a little misleading at times. I would also like to see a few more tables showing the ARTP, k values, RTP, etc, so one can follow how the calculations were done. My comments are outlined in the following:

The title and abstract talk about the ARTP, but the article seems to focus more on the RTP (it took me a while to realise this!).

The relationship between ARTP, RTP and RTP coefficients has been clarified as it was rather muddled.

The introduction constantly seems to interchange between RTP and ARTP as though they are synonyms. E.g., RTP not defined page 13814, line 22. Page 13815, lines 3 and 4, lines 8-15.

Corrected.

Page 13814, line 18: It is implicitly implied that the ARTP is only defined for latitude bands, but I presume this is not a necessary restriction? Perhaps relevant to be a little more general? E.g., drop "(latitude bands)" and later add ". . .Shindell and Faluvegi (2010) developed the ARTP for latitude bands. . ."

Agreed, thank you. The text has been revised to give a more general description of the ARTP followed by the explanation that its definition using the 4 latitude bands is simply the way it was done in the simulations underlying this work (a rationale for choosing those bands is also given).

Page 13815, line 22: This appears to be a definition, but then it uses the word "essentially". Is this the definition or not? Perhaps at the state of the section define clearly "The ARTP is defined as. . ." Additionally, add somewhere "RTP is defined as. . ."

Both ARTP and RTP are now defined clearly, as are the RTP coefficients.

Page 13816, line 7: "kx,y is the dimensionless coefficient relating temperature response in area y to forcing in area x (Table 1)", but Table 1 says "RTP . . . regional response per Wm−2 forcing in the indicated area relative to global sensitivity". Is this an ARTP versus RTP issue, or one is k and one is RTP?

Corrected. k no longer used, RTP coefficients stated when those are being discussed.

I presume that we cannot back calculate the k's as we do not have the forcing, in that case, I think it is quite valuable to have tables with k, F, ARTP, RTP. It is nice to be able *to see how the calculations fit together, and how the numbers are taken from Shindell and Faluvegi.*

What were formerly referred to as k's are now called RTP coefficients, and are presented in Table 1. Those are the values taken from the earlier Shindell Faluvegi papers. I have added a Table presenting the forcing values from the 4 models by region (Table 2). The dT values produced by using the forcing/response portion of the ARTP are shown in Figure 1, while no ARTP emission metric values are computed nor are RTP values.

Page 13816: The scaling of the IRF is a little misleading. It assumes that the time constants are the same in GISS and HadCM3. It also gives the perception that a climate model is only dictated by a single parameter, the sensitivity, when even when it is expressed as the HadCM3 IRF 4 parameters are required. Additionally, the paper focuses on short-lived components, and in this case the short time constant may be more important than the sensitivity. I am not sure it is feasible, but a better approach would be to take a some parameters from the GISS model that link to a 2-box energy balance model (e.g., see Berntsen and Fuglestvedt PNAS 2008, Supporting Information). Given the IRF will probably make a small difference to the results, I think it is better to use the HadCM3 IRF than to give the perception one can do a simple scaling to have whatever climate model they want.

This seems a fair point, and so I've revised to use the original Hadley Centre values for the IRF as suggested. Note that scaling to another climate sensitivity value is now only presented in the equation for time-invariant dT for the equilibrium case (though the available simulations require the use of the transient sensitivity for IPSL and GISS as those are the only results available).

Page 13816, lines 18+: "approximate equilibrium response, or transient response. . ." and then an equation line 24. This is a little confusing. You mean the "climate sensitivity" here is either equilibrium or transient, depending on what you want? Perhaps split

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this into two sentences or make this more explicit. Also the text "(the first term above)" refers to what first term? I am also not sure why we need the Equation on line 24. Is it used later in the article? It also assumes an infinite time horizon (at least very large?), and is this what you are proposing?

This description is now restricted to the equilibrium case only, since as reviewer 3 pointed out, the time dependence in the IRF should still be used for transients if available. The text also now clarifies that this equation is used in the case when the forcing/response portion of the ARTP is used and the equilibrium response is estimated (so yes, arbitrarily long time scales).

Page 13817, line 1: I suggest to make the "As with" a new paragraph as it is changing topic quite a bit

Done.

Page 13817, line 3: Perhaps be a bit more explicit on the CO2 case. What would the k's be?

For a globally uniform (or nearly so) forcing, the coefficients for the response within the four bands are given in Table 1.

Page 13817, line 7-8: Is this definition of k (or RTP?) consistent with the previous?

Yes, it is consistent (and clearer in the revision).

Page 13817, line 9: "normalises" what? ARTP? Or k? There seems to be a set of k in K/(W/m2) and a set of k normalised to the global mean (0.91)? One for CO2 and one for the species? If so, perhaps distinguish clearly as ka and kco2, ARTPa and ARTPco2, etc.

Clarified the normalization and that there is only one set of RTP coefficients which are normalized to the global sensitivity to a globally uniform forcing.

Page 13817, line 20: The row sums do not add to the global, as you mention later due *to nonlinearities. You also mention that this is only done for comparisons? In any case, how can half the 158allocate the difference? Or should the comparison be done with the some of the rows?*

Indeed the row sums do not add to the global due to non-linearities. Given that there are non-linearities, there is no obvious answer as to which set of values is more sensible to use in a linear calculation. That is the reason I used the word ‘roughly’ when saying ‘roughly half the sensitivity in the Arctic is to local forcing’, as the Arctic response of 0.77 times the global response to globally uniform forcing is about half of either the row sum of 1.70 or the Arctic response to global forcing of 1.58, and either is a reasonable comparison.

*How do I use the values? o If I have a 1 W/m2 forcing in the Arctic, then the temperature response in the Arctic is 0.77 the global mean? If the global mean is 0.91K per 1W/m2 (global forcing), then the temperature response in the Arctic is 0.77*0.91=0.70K/(W/m2)?*

A forcing of 1 W/m2 in the Arctic causes a temperature response of 1 W/m2 * 0.77 * 0.91 K/(W/m2) = 0.70 K. Taking into account that a 1 W/m2 forcing in the Arctic is a much smaller global mean forcing (0.07 W/m2) as the Arctic is a small fraction of the Earth’s surface area, it’s clear that the Arctic response is about an order of magnitude larger than the global mean.

If I have a 1 W/m2 global forcing, then the temperature response in the Arctic is 1.70 (row sum) or 1.58? Which do I take? In K/(W/m2) this would be 1.55 or 1.44?

As in my response to the previous comment about non-linearity, since this multiplication is inherently linear there is no clear reason to choose one over the other. The difference provides some estimate of the uncertainty due to non-linearities, and the best estimate is probably the mean.

These values are all only relative to the global means, and thus they would scale to a

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*GTP? For example, If I had an emission in a given region, then the response in another region would be RTP*GTP?*

In response to this comment, the relationship between RTP and GTP is now explicitly presented in the text.

*At the start of section 2 it says "ARTP was developed as an analogue to the AGTP". While this may be the case, it can also be a little confusing. The AGTP converts emissions to a response, whereas most of the discussion around the ARTP is from forcing to response (even though emissions are just as easy mathematically). The RTP, however, seems quite different to the GTP. The RTP seems more concerned with relative difference between regions. It seems I would not take an RTP value and multiply it by an emission to get a CO2-eq emission? Perhaps I would do RTP*GTP*emissions? In any case, it would be useful to discuss these subtleties on how to use the values you have presented.*

The distinction between ARTP and RTP coefficients and the use of ARTP as either an emission metric or only using the forcing/response portion of the ARTP metric has been clarified.

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