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

The ARTP introduced by Shindell and Faluvegi in two previous papers is a great advance on the global AGTP concept by proving impact information on a regional scale. This paper is useful in that it expands on the earlier work; I particularly like the separation into land and ocean effects.

This paper importantly attempts to provide some corroboration of the robustness of the regional breakdown by comparing full model results of several GCMs with the predictions from the ARTP method. I would however like to understand more about how

strict a validation it is. There are 16 elements in the k response matrix so they can't all be constrained by examining the responses to 20th C aerosol changes, presumably mostly in the northern mid-latitudes.

I have a slight quibble about the terminology used, if the ARTP is analogous to the AGTP then it should relate emissions to temperature change with units K/kg. What are actually being evaluated in this paper are not the ARTPs, but the k's which relate forcing to temperature change. These k's are also referred to as "RTP coefficients" which would further imply a normalisation by the CO2 response. "Regional response coefficients" would be a more accurate description (the author may be able to come up with something snappier).

Agreed that this was unclear. The distinction between ARTP and the RTP coefficients is now clarified and the two are both explicitly defined. I also point out that the evaluation of robustness is indeed for the RTP coefficients rather than the ARTP.

Abstract: This needs rewording as the ARTPs need to be defined as responses to emissions, not responses to forcing changes.

Done.

Introduction, first paragraph: Need to define the ARTP as response to emissions, "RTP" is used where I think "ARTP" is meant.

Done.

Introduction, third paragraph: Use a different term to "RTP coefficients".

Done.

Page 13816, line 1 : To define the ARTP, the Fs need to be forcing responses to unit emission changes.

Revised as suggested.

Page 13816, line 26: The relation to emissions needs to come at the beginning of the section as it is part of the ARTP definition.

Revised as suggested.

Page 13817, line 2: I think the normalisation by CO2 needs a little more discussion. It should be made clear that this normalised quantity is the "RTP". How uniform exactly is the CO2 forcing? The temperature response to CO2 isn't uniform, so the RTPs aren't simply a scaling of the ARTPs.

I've added more discussion on the normalization as suggested, including that this is the RTP and how RTP relates to GTP and ARTP (now in second paragraph of section 2).

Page 13819, line 16-17: Can the author be more quantitative than "fairly robust". How many of the 16 response coefficients (not RTP coefficients) are being tested here?

I've added discussion of this as suggested, and indeed it is primarily 6 of the coefficients that are strongly constrained by this test.

Page 13819, line 18-19: What is the difference between these coefficients and the Shindell and Faluvegi ones? What does the fact that the correlations are "nearly the same" tell us? If it's not much, I would cut this sentence.

Agreed that this did not add much, so now deleted. The small difference between the two sets of coefficients is explained in the second to last paragraph of section 2.

Page 13820, line 1-14: This should be a bit clearer as to which elements of the response matrix are being discussed, i.e. the SHext and Arctic responses to which forcing latitudes.

This has been clarified as the paper now says the forcing was mainly in the tropics and NHml, so the SHext and Arctic are responding largely to those forcings (the off-diagonal terms).

Page 13820, line 20-25: The uncertainty range seems to go below zero whereas the full ranges from the models are all positive. Does this mean the uncertainties should be asymmetric?

This is a good point, and on further reflection on this it seems unwise to use the standard deviation when there are so few models. Hence the revised paper utilizes only the full model spread to characterize the uncertainties (which now do not go below zero as none of the models go below zero enhancement).

Section 5: Does the 20 percent uncertainty range refer to the all the elements in the Tropics and NHml rows in table 1, or just the diagonal elements, or just to the sum of the elements?

Given that the test is only for the response in a given area to all imposed forcings, the 20 percent is for the uncertainty on the sum of the responses to the various regions (i.e. the sum of the elements times the forcing, where the latter is assumed to be diagnosed in the models with no substantial uncertainty). Hence this would seem to be a good estimate for the 4 elements that are driving most of the response in those two regions as well (the response in each of those two regions to forcing in those two regions).

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