

Interactive
Comment

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

D. Shindell

dshindell@giss.nasa.gov

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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 2

The short note "Evaluation of the Absolute Regional Temperature Potential" by Drew T. Shindell address the climate sensitivity of zonal bands to forcing in zonal bands by CGCM model simulations. The analysis presents a simple equation (model) to evaluate the response in zonal bands. I think the analysis is interesting and is potentially relevant for the large climate change community. I think that it may be publishable in ACP with some (mostly minor) revisions and it should be considered for ACPD. Details below.

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comments:

** Clarity: Most of the article is well written, but I find the introduction, abstract and title lags clarity. The authors is mostly addressing a very small community and his work. Thinks like "Absolute Regional Temperature Potential" does not mean much to most readers or "RTP" is not introduce, but should be even if the reader can guess what it means. Also the first paragraph in the introduction could need much more details.*

Though the title is too short to elaborate on the meaning of the ARTP, the introduction of the revised paper has been expanded and in particular text has been added about the purpose and utilization of metrics in general. A sentence about the purpose of this metric was also added to the end of the abstract.

** I do not like the formulation of "I" in this study. I think the author should aim for a more objective presentation.*

The use of I is common in single authored papers (though such papers are no longer common). I do not see how I is any less objective than the more common we. It does not make sense to use we here (I'm not royalty), and I feel the paper would be unpleasant to read if it was all in passive voice, so I have kept the use of the first person (though in some cases replaced it when fitting - e.g. The results show... instead of I show...).

** The regional approach in this study is strongly limited by some wide latitude bands. I think the work would benefit from some discussion of how these where chosen and how this should be improved. When I think of regional differences in the climate sensitivity I would think much more about the climate differences such as land vs. ocean; cold vs. warm; dry vs. wet or cloudy vs. clear sky. Thus I would think more about those aspects of the climate system that control the main feedbacks (water vapor, clouds, ice-albedo, etc.). The simple approach of latitude bands is ok for a start, but I would assume that it may mask some more interesting regional differences.*

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Discussion of this topic has been added to the end of section 2, which now explains why these bands were chosen. Agreed that the bands may mask further interesting differences, and the text now states that it's be very interesting to explore ARTP at finer resolution, but also more computationally expensive. Clearly contrasts such as those noted by the reviewer (wet-dry, cloudy-clear) are at finer scales than the latitude bands used here.

** I wonder how these regional differences would look like in a globally resolved energy balance model such as the one from Dommenget and Floeter, climate dynamics 2011. Just a suggestions for future work. It may help to understand the physical processes causing it.*

Yes, this would be interesting for future work. It would be good to relate more clearly the underlying regional sensitivity differences, as seen here in the global forcing/Arctic response RTP coefficient in table 1 showing Arctic amplification, to the role of inhomogeneous forcing in causing different sensitivity.

** page 6 lines 24-27: The land-sea warming contrast needs to be discussed in the context of different feedbacks and not just in terms of a transient effect by heat capacities. Sutton et al. GRL, 2007 and more importantly Joshi et al. climate dynamics 2008 discuss this. Joshi et al. illustrate the different moisture availability over land and oceans cause different surface warming, because the moist oceans can heat the free atmosphere much stronger by latent heat release (moist adiabat lapse rates).*

Agreed. The text had not really explained why the land and ocean responses were different, but certainly this is not merely heat capacity. Text on this has been added to the first paragraph of section 4.

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