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Interactive comment on "Regional temperature change potentials for short lived climate forcers from multiple models" *by* Borgar Aamaas et al.

Anonymous Referee #4

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General comments

The paper is interesting and in general well-written. As it is clearly described in the paper it builds on the work by Collins et al (2013), make use of a methodology developed in Shindell & Faluvegi (2009) and with data largely from Bellouin et al (2016). The study makes contributions regarding the estimation of Absolute Regional Temperature Potentials for NH3, the effect of aerosols on the ARTP for O3 precursors, extended analysis of the warming effect of BC on snow and ice and perhaps most importantly they analyze summer and winter specific metrics.

My key comment relates to the division of metrics into four latitude bands. I understand that the paper here builds on the framework by Shindell & Faluvegi (2009), but why is it 4 latitude bands, and not 6, 8 or any other number of latitude bands that is a relevant

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Discussion paper

separation for the ARTPs? This needs to be discussed and problematized. More importantly, why is not a separation between temperatures impacts on land surfaces and ocean surfaces used? The land-ocean separation may be critical for regionalized metrics due to the significant land-ocean warming contrast (Joshi et al, 2008; Boer, 2011) and the very different climate impacts on land and ocean areas. I do not think the authors need to change their calculations, but a discussion regarding the relevance of the approach they take, what important aspects they miss with the regionalization they use and how the regionalization can be developed further is needed in the paper. Finally, overall I think the paper is a valuable contribution to the scientific literature and deserves to be published after the general comments above and the specific comments below have been taken into account.

Specific and technical comments

1. Page 1 line 31-32. The authors write “CH4 is often also included because its lifetime of around 10 years is shorter than or comparable to climate response timescales.” I am not sure if I have seen this argument before. Please, justify with a reference why this is the reason why CH4 is included among the short lived climate forcers.

2. Page 4 line 131-132. The authors write “We assume that the time evoluation of temperature in each response band follows the global mean temperature”. Is this a valid assumption? For example, Cherubini et al (2016) do a related, but brief analysis using MAGICC, where they estimate regional metrics based on emissions that take place in either NH-ocean, NH-land, SH-ocean and SH-land, and where the NH and SH temperature response is analysed. If one contrast the assumption that the "time evaluation of temperature in each response band follows the global mean temperature” with results presented in Cherubini et al (2016) figure 3 the assumptions appears to be rather crude, especially on short time scales. The authors need in greater length justify this assumption.

3. Equation 2. In equation 2 the indices r,m,s, for the ARTP is dropped. Why? Please,

be consistent throughout the equations or explain carefully difference between similar variables used in different equations.

4. Equation 3 and line 14-143. The authors write: “the general expression for the ARTP can be simplified to”. Even though the mathematics behind this approximation is quite simple it should be shown and/or explained in a footnote, in the supplementary material or a reference to a paper where this is done should be included.

5. Page 5 line 152. The authors mention that the average adjustment time of CH4 is 9.7 years in the three models used. This is relatively short compared to the IPCC AR5 assumption (12.4 years). Can the authors explain why a relatively short atmospheric adjustment time is find in the models used in the paper?

6. Page 5 lines 164-165. The authors write “RCS matrices only exist for annual emissions, we assume we can apply the same set of matrices for 165 emissions during NH summer and winter.” Please justify this assumption.

7. Page 8 lines 277-279. The authors write “For all the species, the response bands with the largest ARTP values are for the responses in the NH mid-latitudes (60% of the cases) and Arctic and the band with the least response the SH mid-high latitudes (see all panels in Fig. 1). This skewness is partly due to the emissions occurring mainly in the NH, but the same pattern is seen for CH4 (Figure 1(O)), for which the emission location is less important.” The argument “This skewness is partly due to the emissions occurring mainly in the NH” is confusing. I first thought that the authors were referring to actual real world emissions, but that is totally irrelevant since you study equally sized emission pulses from different regions. Please write clearly what you mean with “This skewness is partly due to the emissions occurring mainly in the NH”. There is also a similar argument in line 282 where the authors write “most emissions occurring in NH”. Please clarify!

8. Figure 4 and page 13 line 445-447. The authors write “The relative differences are generally larger for the aerosols than the ozone precursors, as seen in Fig. 4, where

only the emissions regions and seasons with a relative difference larger than 20% are presented." Why is only cases where the relative difference between ARTP and AGTP are larger than 20% shown? Wouldn't it be equally relevant to see the cases where the difference is small?

References

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For other references please see the paper by Aamaas et al, 2017

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