

Interactive comment on “Regional temperature change potentials for short lived climate forcings from multiple models” by Borgar Aamaas et al.

Anonymous Referee #2

Received and published: 23 April 2017

The manuscript by Aamaas et al. presents new calculations of regional temperature change potentials (ARTPs) calculated using radiative forcing estimates from four different models, and regional (actually zonal) climate sensitivities from one model, all taken from past studies. In a way it replicates the work of Collins et al. (2013), though with different data used for the radiative forcing estimates. It also includes some methodological advances compared to that previous work, such as the separation of the impact of different seasons, and accounting for the vertical structure of BC. The paper is certainly within the scope of ACP. It does not include any major specific new findings (those have been documented in earlier papers on which it is based), but it will be a useful addition to the literature when it comes to exploring the development and application of regional emission metrics. Therefore, I suggest its publication following the revisions and clarifications suggested below.

GENERAL COMMENTS:

- I find the title somewhat misleading. It suggests that multiple models were used, but for the actual temperature response, the calculations still rely on one model. I suggest that this kind of title is kept for when the community has RCSs from more than one model, and therefore I recommend removing that part of the title of the current manuscript.

- I suppose if a policy maker (or a scientist) was in need of regional metrics, they might end up being confused as to whether they should use those presented here or those presented in Collins et al. (2013). The models used for radiative forcing estimates here may be somewhat newer, but they are also fewer. Is there anything convincing that could be said (perhaps in Sect. 3.1.3) as to which of the estimates is more reliable, specifically when it comes to the radiative forcing terms? Also, it would have been interesting to see how the numbers in this study would have differed had the same method as Collins et al. (2013) been used here (i.e. without the methodological advances), but I appreciate that this may be a quite substantial task at this stage.

SPECIFIC COMMENTS:

Page 1, Line 14: Suggest changing “the globe” to “the entire globe”.

Page 1, Lines 21-22: Sentence not entirely clear.

Page 1, Line 31: Suggest rephrasing to “included in the definition because”.

Page 1, Line 32: The temporal variation has changed over time?

Page 2, Line 43: component -> constituent.

Page 2, Line 54: I do not think that all the papers referenced here (e.g. Stevenson et al. (2005), Wild et al. (2001), Fry et al. (2012)) quantify the global temperature response to emissions broken down by region. But that is what the reader is left to think.

Page 2, Lines 55-57: Strictly speaking, Shindell and Faluvegi (2010) did not present

[Printer-friendly version](#)[Discussion paper](#)

regional temperature potentials; a potential is a response per unit emissions, whereas their paper provided responses per unit forcing.

Page 2, Lines 67-68: I think you need to explain up front to the reader what the differences are with Aamaas et al. (2016).

Page 3, Line 73: Applied to do what?

Page 3, Line 82: Perhaps rephrase to “The regional RFs are then averaged for four latitude bands. . .”?

Page 3, Line 86: Perhaps “effects” is a slightly better choice of word here than “processes”. Also, I would say it is worth briefly mentioning/listing those effects here in the sentence so that the reader does not have to necessarily look at the figure to see what is meant.

Page 3, Lines 89-90: So, these are instantaneous forcings?

Page 3, Line 94: “methane induced” -> “methane-induced”.

Page 3, Lines 100-102: Not clear how such an experiment can diagnose the semi-direct effect alone. Imposing a perturbed concentration of BC would have both direct and semi-direct effects, no?

Page 3, Lines 102-104: Not clear what ozone and methane have to do with aerosol direct and 1st indirect effects.

Page 3, Line 108: So what is ECHAM6 used for in this study?

Sect. 2.2: What are the implications of using RCSs derived from equilibrium simulations to infer metrics for transient situations?

Page 4, Line 129: “Pattern” implies “geographical distribution”, so I suggest replacing with e.g. “ratios”.

Page 3, Line 138: It is not mentioned what t' represents. Presumably it is the timing

[Printer-friendly version](#)[Discussion paper](#)

of emission. Also, the indexing of location and season is suddenly dropped here. It should be mentioned in the sentence before the equation that this equation holds for the impact of every region/season or summations need to be added around the integral.

Page 5, Line 154: Shouldn't the upper limit of integration be H?

Page 5, Line 160: do -> apply.

Page 5, Lines 162-163: What is meant here by "are only incorporated in RT"?

Page 5, Line 166: What does CO₂ have to do with scattering aerosols?

Page 5, Line 167: I suggest changing "long-lived" to "longer lived", as, from a climate perspective, "long-lived" implies something even longer.

Page 5, Lines 165-169: I am not at all sure what has been done here. Shindell and Faluvegi (2009) provide values for every individual effect, i.e. sulphate (proxy for all scattering aerosols), ozone, BC, CO₂, methane. So why haven't those simply been used here?

Page 5, Line 170: "based on several sources" is vague here.

Page 5, Line 171: Is it really for the "aerosol effects", or for the BC part of the aerosol effects?

Page 5, Line 173: Again, what does CO₂ have to do? I may be missing something, but I guess so will several other readers, since this is not explained clearly.

Page 6, Line 187: do -> apply.

Page 6, Lines 190-193: A few references are needed to support these statements.

Page 6, Line 212: This is somewhat confusing. Since Flanner (2013) is used for the estimates of sensitivities for the case of BC on snow effects, how can the semi-direct effect be implicitly included?

Page 7, Line 234: Suggest rephrasing to "Results for continuous time horizons...".

Page 7, Line 242: regions -> changes.

Page 8, Line 252: Not clear what is meant.

Page 8, Line 265: Is the increased efficacy a consequence of accounting for the vertical structure? If so, worth mentioning.

Page 8, Lines 273-274: (Last sentence in paragraph) On this timescale only, right? Also, this seems to also hold for NO_x; perhaps worth mentioning?

Page 8, Lines 277-278: Emissions should not matter since the metrics are normalised by emissions, right?

Page 8, Lines 280-281: OK, but why? Shindell et al. (2015) provide some insight worth discussing.

Page 9, Line 299: Due to the largest presence of snow in the winter in the NH, presumably?

Page 9, Line 302: Why is there this negative aerosol response to VOCs?

Page 9, Line 308: Is it really the emissions region that drives larger differences than for other species, or the response region? It seems that the latter is the case.

Page 9, Line 319: of -> on

Page 10, Line 329: for -> among

Page 10, Line 336: Shouldn't Shindell and Faluvegi (2009) be cited here?

Page 10, Lines 343-345: Whereas what is the case here?

Page 10, Line 360: disagree -> disagrees

Page 11, Lines 370-371: Is this because in the summer there is more OH expected per unit NO_x change, due to higher insolation?

Page 11, Line 380: Link with the earlier Equation 2.

Printer-friendly version

Discussion paper



Page 11, Line 382: Pulse emissions, not sustained, right?

Page 11, Lines 388-390: Larger than what? Presumably it refers to winter vs summer, but it needs to be clarified.

Page 11, Lines 390-391: All ozone precursors or just CO?

Page 12, Line 406: A bit confusing that earlier methane was included and here it is not, especially since often the “short-lived” terminology includes methane. Anyway, apparently methane has been dropped in this section, and it has to be mentioned upfront in it that it is not accounted for.

Page 12, Line 410: Larger even than those of sulphate? I doubt it. Probably it is meant that there is a larger seasonality for BC than for other species.

Page 13, Line 444: Perhaps add “for the same species” after “seasons”.

Page 13, Line 456: Uncertainty in emissions is not accounted for, right? In which case E_i should be removed from the fraction shown in this sentence.

Page 13, Line 469: Correlated with what?

Page 14, Line 476: And not just for SLCFs, right? Given that WMGHGs also cause regionally varying effects.

Sect. 3.4 (general): And what about the propagation of uncertainties in RCS due to internal variability, as reported in Shindell and Faluvegi (2009)? And also uncertainties due to spatial variability and subsequent averaging? Not that it would be expected to account for them at this stage, but worth mentioning and perhaps speculating on their importance.

Page 14, Line 487: Suggest removing “the more”.

Page 14, Line 491: indicate -> indicates

Page 14, Line 500: Suggest adding “by” before “up” and “individual” before “regions”.

Printer-friendly version

Discussion paper



REFERENCES:

Shindell, D. T., G. Faluvegi, L. Rotstayn, and G. Milly (2015), Spatial patterns of radiative forcing and surface temperature response, *J. Geophys. Res. Atmos.*, 120, 5385–5403, doi:10.1002/2014JD022752.

[Interactive comment on Atmos. Chem. Phys. Discuss.](#), doi:10.5194/acp-2017-141, 2017.

ACPD

[Interactive
comment](#)

[Printer-friendly version](#)

[Discussion paper](#)

