We thank all the reviewers for helpful comments that will improve our manuscript. Our responses are given in red.

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.

We agree that “multiple models” might be misleading. We have readjusted the title to:

“Regional temperature change potentials for short lived climate forcers based on radiative forcing from multiple models”

- 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.

The reviewer is raising an interesting question. The methodological improvements and the separation between summer and winter emissions are the most important arguments for going for our study. But other factors, such as more models included, will give more weight to Collins et al. (2013). Bellouin et al. (2016) make a comparison of the RF data with previous studies (such as Yu et al. (2013) and Fry et al. (2012), which is the background to Collins et al. (2013)) in their Table 1. As a result, we are not repeating that comparison in our study. We add two sentences in Section 3.1.3, but we do not want to make a final advice on not using Collins et al. (2013):

“The study by Collins et al. (2013) is more comprehensive than our study in terms of the number of models included, while the RF dataset we use is newer and more detailed (see Table 1 in Bellouin et al.,

2016) and the forcing-response coefficients are improved. Hence results from both studies will be of benefit to those wanting to apply our metrics."

While a comparison with Collins et al. (2013) by using their methods is an excellent idea, we would like to keep that out in the interest of keeping the article length short, as well as we are uncertain how much more value that would give to our study. This would also be a substantial task at this stage, as the reviewer recognizes.

SPECIFIC COMMENTS:

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

Accepted.

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

We have included "per unit BC emission" to clarify the statement:

"The temperature response in the Arctic per unit BC emission is almost 4 times larger and more than 2 times larger than the global average for Northern Hemisphere winter emissions for Europe and East Asia, respectively"

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

Accepted.

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

The word "temporal" is removed to avoid saying the same thing twice.

Page 2, Line 43: component -> constituent.

Accepted. We have also made similar changes throughout the manuscript.

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.

We agree that those studies did not quantify the global temperature response. We tried to be general and include both temperature and RF in the word "global response". To simplify, we will remove Berntsen et al. (2005), Stevenson et al. (2005), Wild et al. (2001), Fry et al. (2012)).

Page 2, Lines 55-57: Strictly speaking, Shindell and Faluvegi (2010) did not present regional temperature potentials; a potential is a response per unit emissions, whereas their paper provided responses per unit forcing.

We see your point. We change the end of the sentence from "to regional emissions" to "from regional RFs".

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).

We have changed the sentence, so there is a short description of the Aamaas et al. (2016) paper. End of sentence:

"…and extend the global temperature responses estimated by Aamaas et al. (2016) to responses on latitude bands."

Page 3, Line 73: Applied to do what?

We have added this in the sentence:

"…applied to calculate regional temperature responses of…"

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

Changed.

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.

Accepted. We have changed the wording throughout the manuscript. We have added the six processes/effects in the text.

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

Page 3, Line 94: "methane induced" -> "methane-induced".

Fixed.

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

We tried to be short, but we have now expanded the explanations to:

"The semi-direct effect is quantified in Bellouin et al. (2016) by prescribing control and perturbed distributions of BC mass-mixing ratios based on OsloCTM2 in 30-year, fixed sea-surface simulations with the Community Earth System Model (CESM). The RF from aerosol-radiation interactions was quantified with multiple calls to the radiation scheme. Because the semi-direct effect is not included in the CAM4 component of the CESM, the semi-direct effect is calculated as the difference between the RF from aerosol-radiation interactions and the effective RF."

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

We have added this sentence:

"The ozone precursors and CH_4 can influence the aerosol effects, as a reduction in CH_4 concentration leads to increase in OH, which promotes sulfate aerosol formation."

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

This is a good comment. ECHAM6 is included in the checking of robustness of individual models against the best estimate in Section 3.1.4.

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

This is a comment relevant for all studies on emission metrics, whether calculations are on ARTPs or AGTPs. Emission metrics are, by their nature, based on simplifications. Ideally, we would like to use temperature responses specific for each species, while what we use is the temperature response estimated by Boucher & Reddy (2008) because it has been widely used in similar work, including in IPCC AR5. See also response to reviewer 4 on temporal temperature response, which lead to including a reference to Cherubini et al. (2016).

Cherubini F., J. Fuglestvedt, T. Gasser, A. Reisinger, O. Cavalett, M.A.J. Huijbregts, D.J.A. Johansson, S.V. Jørgensen, M. Raugei, G. Schivley, A. Hammer Strømman, K. Tanaka, A. Levasseur (2016) Bridging the gap between impact assessment methods and climate science. Environmental Science & Policy 64: 129-140.

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

We have replaced "pattern" with "in the different latitude bands".

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

We have added information, such as stating that t is the time and included the indexing.

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

Yes, this is corrected. RF(H-t) has also been corrected to RF(H) in the same equation.

Page 5, Line 160: do -> apply.

Changed.

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

The climate sensitivity is only found in the temporal temperature response function (RT). We have change the last part of the sentence to:

"the climate sensitivity in our ARTP calculations is only included in one of the parameters, in the temporal temperature response (RT)."

Page 5, Line 166: What does CO_2 have to do with scattering aerosols?

We follow the practice in the literature, the first study to take the average was Shindell & Faluvegi (2010). This was probably done to get more robust numbers and that the coefficients did not vary that much between SO_2 and CO_2.

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

Accepted.

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?

As above, we follow the practice by Shindell & Faluvegi (2010) and Collins et al. (2013).

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

We have rephrased, so that part of the sentence is now this:

"the regional sensitivity matrix applied is more complex"

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

We have included the word BC.

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.

We are not aware of any RCS in the literature on the semi-direct effect. Our strategy is that if we do not know better, we use the RCS values presented by Shindell and Faluvegi (2010). We think CO2 is a decent second best option. We have split the sentence into two and edited the second sentence to:

"As we are not aware of a RCS matrix for RF explicitly calculated for the semi-direct effect, we use the average CO2 and SO2 coefficients shown in Shindell and Faluvegi (2010) based on Shindell and Faluvegi (2009)."

Page 6, Line 187: do -> apply.

Changed.

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

We have added reference to Quinn et al. (2008). We think the other references later in that section covers the material.

Quinn, P. K., Bates, T. S., Baum, E., Doubleday, N., Fiore, A. M., Flanner, M., Fridlind, A., Garrett, T. J., Koch, D., Menon, S., Shindell, D., Stohl, A., and Warren, S. G.: Short-lived pollutants in the Arctic: their climate impact and possible mitigation strategies, Atmos. Chem. Phys., 8, 1723-1735, 10.5194/acp-8-1723-2008, 2008.

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?

Flanner (2013) is also used for the BC aerosols effects for Arctic-to-Arctic, and this sentence is based on that fact. We have included this clarification in the sentence:

"for the BC aerosol effects for Arctic-to-Arctic warming"

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

Changed.

Page 7, Line 242: regions -> changes.

We have changed from "the other regions" to "other differences".

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

We have changed sentence to:

"Due mainly to heat transport between the latitude bands, the RCS coefficients also represent non-local temperature responses, thus, the temperature response is seen more evenly in all latitude bands."

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

This line is about the efficacy of BC in snow, not the vertical structure in the atmosphere. The reviewer may have meant another line. We do not have evidence that the vertical structure gives a significant increase in efficacy for ARTP relative to AGTP. We keep sentence as is.

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

CO has a longer lifetime than NOx, so this is most relevant for CO. We keep the sentence as is.

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

Yes, emission size should not matter due to normalization. But the location of the normalized emissions matter, which is the focus of this sentence.

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

Thanks for reference. We have added this sentence:

"Shindell et al. (2015) argue that the high responses in NH mid- and high-latitudes are not due to feedbacks particular for the SLCFs, but mainly due to the efficacies driven by the large land fraction in this area and strong snow albedo feedbacks."

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

Yes. We add this to the end of the sentence:

"when the snow cover area is at its largest"

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

We have added this sentence:

"VOC emissions perturb aerosols via secondary organic aerosol formation, which two out of three models find to be cooling."

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.

We focused on different emission regions and seasons, but we see that response regions should also be mentioned here. We have added this to the sentence:

“,and response regions (comparing Arctic with other latitude bands for European emissions in Fig. 1(B)).”

Page 9, Line 319: of -> on

Changed.

Page 10, Line 329: for -> among

Changed.

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

Yes. Included.

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

We extend the sentence with this:

“whereas Collins et al. (2013) did not.”

Page 10, Line 360: disagree -> disagrees

Changed.

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

Yes, we agree. However, as the RFs and the chemistry behind were discussed by Bellouin et al. (2016) and in the interest of space, we would like to avoid too much discussion of chemistry. We would like not to mention this detail. We see the sentence was unclear, so we have edited to:

“The results show that NOx emissions in Europe have in general more negative ARTP values for summer emissions than for winter emissions”

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

We have added a link to Equation 2 by attaching this to a sentence:

“based on Eq. (2)”

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

Correct, we have clarified by adding “pulse”.

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

Yes. We have added this clarification:

“in winter than in summer”

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

All the ozone precursors. We have added the word "all".

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.

We have clarified stating "non-CH_4 SLCFs" in the first sentence.

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.

Yes, we were thinking about the seasonality of BC, but we see that sulphate should also be mentioned. We have changed the sentence to:

"The main reasons for the seasonality differences are the strong heating from the BC deposition on snow for winter emissions close to snow and ice surfaces, the relatively larger BC emissions in winter than for the other species, and weaker cooling effects of SO2 in winter."

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

Added.

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

This is a good comment, but we think we should keep as is. The data from Bellouin et al. (2016) is normalized radiative forcing; hence, emissions are included in the denominator. The uncertainty we discuss is on the normalized radiative forcing.

Page 13, Line 469: Correlated with what?

We have clarified by adding this after correlated:

"for different species in a model"

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

The manuscript does not address long lived greenhouse gases, but we agree that this point could be made here. We have added long lived greenhouse gases in parenthesis.

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.

The RTP concept (and also other emission metrics) quantifies the expected response to an emission perturbation. This is understood as the mean response for a large ensemble. For natural climate system and for a single GCM simulation there will be unforced natural variability on top of that. The uncertainty numbers in Shindell and Faluvegi (2009) are the standard deviation of the last 80 years of an equilibrium run. As such, it is only a measure of the unforced internal variability. Since this should not be a part of the metric values, we believe it is not correct to include it in the analysis.

We have added several paragraphs in Section 3.4 on uncertainties related to spatial variability as a response to other review comments. See the other reviews.

Page 14, Line 487: Suggest removing "the more".

Deleted.

Page 14, Line 491: indicate -> indicates

Changed.

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

Added.

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.