Review of “Multimodel emission metrics for regional emissions of short lived climate forcers” by B. Aamaas

Anonymous Referee #1

The comments by the Reviewer are in black, our responses are given in red. We thank Reviewer 1 for positive feedback and comments that will improve the paper. Most of the changes and suggestions have been accepted.

Due to comments from Referee #2, we have waited for the publication of the Bellouin et al. paper. That paper is now out in ACPD (http://www.atmos-chem-phys-discuss.net/acp-2016-310/). As some of the radiative forcings have been revised, all figures and numbers have been updated to reflect this. The changes are mostly minor. Among the updates is an improvement of the BC semi-direct effect, as well as the best estimate of the aerosol effect of BC is no longer based on ECHAM6, as argued for in Bellouin et al. The revisions cause the best estimate of BC to increase by about 10-20% and negative BC RF is now unlikely. The text have been updated to reflect these small changes, in addition, to include minor fixes of language and minor clarifications, as well as referring to new literature.

General comments

This is a fascinating and well-written paper, and should be accepted with minor revisions. It tackles an (almost) endlessly complex topic, and although it is quite tough going in places, I think it is (almost) as clear as it could be. From my specific comments below, I would like a little more discussion of a few points. I realise this will only lengthen an already quite long paper, so I recommend these requests are dealt with as concisely as possible.

Specific comments

Title: maybe it’s OK, but I’d prefer not to see ‘emission/s’ twice. ‘Metrics for regional emissions of short lived climate forcers from multiple models’?

We agree that the title needs a change and revised the title to:

“Regional emission metrics for short lived climate forcers from multiple models”

P26090 l6 Delete ‘season’

Accepted

P26090 l16 (and throughout) I appreciate ‘ramp up’ is the commonly used phrase, but in most cases when we are thinking about emissions mitigation, it is a reduction, or ‘ramp down’ of emissions that is being considered. How about dispense with ‘up’, and just say ‘ramp’, or ‘ramping’?

We agree and decided that we will use the term ‘ramping’ throughout the article.

P26090 l26 ‘…when accounting for correlations’ – clarify

We agree that this formulation is unclear and have explained in more detail with this edited sentence:

"Further, the estimated climate impact of an illustrative mitigation policy package is robust even when accounting for the fact that the magnitude of emission metrics for different species in a given model are correlated."

P26091 l7 Define BC, OC, VOC at first use. I know these are near universal acronyms now, but let's do this properly. Maybe this should be in the abstract.

Accepted. We write out the first time black carbon, organic carbon, and volatile organic compound are first presented in the introduction.

P26091 l9-10 Reword sentence beginning CH4. Explicitly state the CH4 lifetime is ~10 yr.

Reworded and stated the lifetime of CH_4:

"As CH_4 has an atmospheric perturbation lifetime of about 10 years, this gas is generally as well-mixed as the LLGHGs, but is often categorized together with the SLCFs since its lifetime is shorter than a realistic time scale for stabilizing anthropogenic influence on climate."

P26092 l2 …emit a range of species…

Accepted

P26092 l16 However, for SLCFs,…

Accepted

P26092 l22 …distinct patterns…?

Accepted

P26093 l4 …the impact also depends upon the season of emissions.

Accepted

P26093 l19 ramp up (see earlier) – ramped here?

Accepted

P26094 l5 Delete 'for'

We think the Reviewer is pointing to P26095 l5. Accepted. We have deleted the first 'for'.

P26095 l1 Clarify you mean aerosol species, not the precursor – i.e. the important lifetime is that of the aerosol (e.g. (NH4)2SO4), not its precursors, NH3 and SO2.

We think the Reviewer is pointing to P26096 l1. Clarified. We state "aerosol species".

P26095 l2 You perhaps need to define 'adjustment time', clarifying the difference between a lifetime and an adjustment time (e.g., with the example of CH4).

We think the Reviewer is pointing to P26096 l2. We mean the same lifetime as the sentence before. We have clarified the previous sentence by using the term "perturbation lifetime". We also add this sentence for clarity:

"The adjustment time can be dependent on different processes with different timescales, such as wet and dry deposition."

P26095 l5 It is probably best for clarity to be consistent, and define IRFT as 'impulse response function for temperature', in line with later uses of IRF.

We think the Reviewer is pointing to P26096 l5. Accepted. New sentence:

"…where $IRF_T(H-t)$ is the impulse response function for temperature at time *H* to a unit radiative forcing at time t."

P26095 l11-15 I am not completely familiar with the concept of IRFT. From what is written I first get the impression that it is independent of species, but then I am left unsure. If it is dependent on the species, should it have a subscript i?

IRFT is assumed here to be the same independent of species, although we acknowledge that the temperature response may be to some degree dependent on the species, due to differences in efficacies and fast adjustments. We have clarified in l11-12:

"The IRFT is here treated independently of the emitted species and based on simulations with the Hadley Centre CM3 climate model (Boucher and Reddy, 2008)."

We have also edited the next sentence ending with: "…and the spread due to IRF_T is larger for SLCFs than for species with longer lifetimes (Olivié and Peters, 2013). The new sentence:

"These parameterizations have uncertainties, and Olivié and Peters (2013) studied the effective of different IRFT from different atmosphere–ocean general circulation models and found that the uncertainty is the largest for the most short-lived SLCFs."

P26100 l13 Clarify – is it (NH4)2SO4 aerosols that are higher in summer?

Yes. We have clarified the sentence by writing "ammonium sulphate aerosols" instead of "sulphate aerosols".

P26100 l23 Comparison of the VOC and CO GTP(20) values maybe merits discussion – I certainly find Figure 2 fascinating. Clearly, VOC oxidation generates CO, so the VOC values should bear some resemblance to the CO. One difference is that the VOC generates more O3, so the O3 component is more important. This O3 itself generates more OH, but clearly the overall impact on OH of VOC is more negative than for CO, as the methane components are about 4x larger. This factor must depend quite strongly on the emitted mix of VOCs. Is there a dominant VOC, or is the signal coming from a whole range of different VOCs, with different lifetimes and O3 production potentials? Is the VOC mix and chemistry different between the models?

ECLIPSE models differ strongly in the type and amount of VOC considered. The diversity in specific radiative forcing indeed reflects differences in ozone production potentials. Other sources of diversity likely contribute, but there is first a need to harmonise the VOC species considered in different models. This is discussed in the Bellouin et al. paper and, thus, we do not repeat the discussion here.

P26100 l29 Ships emit into the lowest background NOx environments – I guess this is why they have larger impacts?

Yes, this is how we explain the large contribution of methane to shipping specific radiative forcing. This is the main effect, and we include a clarification on this. However, among contributing factors that we don't describe in paper is the low latitude of the emissions, that the speed of the reaction CH4 + OH increases with temperature and high specific humidity means that O(1D) radicals have a higher probability to react with H2O. We added a comment on the low background concentrations in the sentence:

"Shipping has the largest GTP(20) values in magnitude for all ozone precursors, especially the large methane effect, driven by the relatively clean atmospheric conditions around the emission locations."

P26103 l8-9 So why do European SO2 emissions have higher value metrics compared to East Asian? Is it purely due to geography, or is it because background levels are higher in Asia, and the impact of emissions tends to saturate?

Although differences in cloud regimes between Europe and East Asia may explain some of the regional differences, it is indeed likely that the more polluted baseline in East Asia means that aerosol-cloud interactions sit at the saturated end of cloud responses, decreasing the radiative efficiency of emission reductions there. This type of analysis belongs to the Bellouin et al. paper and that paper includes this: "RFaci non-linearities also explain why models simulate weaker SRFs for East Asian than European perturbations. With a more polluted baseline, East Asian aci stands more often at the saturated end of the CCN-cloud albedo relationship, where RFaci is weak (Wilcox et al., 2015)." We have added this sentence on P26099 l13:

"The higher emission metric values for Europe than for East Asia is likely caused by a more polluted baseline in East Asia, which leads to a saturation for some of the interactions."

P26103 l16 And why are CO emissions the opposite (E Asia > Europe)? Is it just because they are emitted nearer the equator, where they have greatest impact on OH and CH4 lifetime?

Although model agree that East Asian perturbations yield a stronger ozone response than European perturbations, differences are small and reasons are not fully understood. It could be due to East Asia being closer to the Equator. The Bellouin et al. paper points out that the stronger response in East Asia is partially due to stronger ozone effect, which is also based on higher NOx background in East Asia. We have added this sentence:

"The difference may occur since the East Asia region is located closer to the Equator."

P26107 l25 Why is the aerosol effect from NOx not cooling for ships? Do they somehow shorten the SO4 lifetime?

The main reason for the different behavior for shipping emissions is the different subset of models. For the other regions, two models find a cooling and one model warming. The best estimate is a cooling. Unfortunately, only two models did the detailed analysis for shipping, one cooling and one warming. The discrepancy between the models concerning the aerosol effect of NOx is discussed in detail by the Bellouin et al. paper. Here is a part of this discussion: "Figure S8 shows that those disagreements stem from differences in regional responses. Both HadGEM3 and NorESM1 show positive aerosol RFs centred on the regions being perturbed, caused by a decrease in sulphate aerosol formation through OH oxidation because OH levels are decreased. The SO2 not oxidised and not deposited is transported

downwind of the perturbed region, where it promotes sulphate aerosol formation in the absence of oxidant limitation: in those regions, both models simulate negative aerosol RFs. The balance between regions of positive and negative aerosol RF varies depending on the model, the perturbed region, and the season. In contrast, OsloCTM2 does not simulate this dipole of responses: its aerosol contribution is negative almost everywhere on the globe." We change the sentence to:

"This aerosol effect is cooling for all regions, while the models disagree about the impact for shipping. The results for the shipping sector should be considered with care as the best estimate is based on only two models with large inter-model variability."

We also add these sentences to the second last paragraph in Section 3.1.1:

"One notable feature for NOx is that the aerosol effect is negative for all cases except for shipping, mainly because the values for shipping are based on two models and the other values are based on three models. The positive value for shipping is the average of two models with opposing signs; thus, there is significant uncertainty in the best estimate. This model disagreement for NOx is discussed in detail by Bellouin et al. (2016)."

Interactive comment on “Multimodel emission metrics for regional emissions of short lived climate forcers” by B. Aamaas et al.

Anonymous Referee #2

The comments by the Reviewer are in black, our responses are given in red. We thank Reviewer 2 for the valuable comments that strengthen the paper. Most of the suggestions have been followed.

The Bellouin et al. paper is now out in ACPD (http://www.atmos-chem-phys-discuss.net/acp-2016-310/). As some of the radiative forcings have been revised, all figures and numbers have been updated to reflect this. The changes are mostly small. Among the updates is an improvement of the BC semi-direct effect, as well as the best estimate of the aerosol effect of BC is no longer based on ECHAM6, as argued for in Bellouin et al. The revisions cause the best net estimate of BC to increase by about 10-20% and negative BC RF is now unlikely. The text have been updated to reflect these small changes, in addition, to include minor fixes of language and minor clarifications, as well as referring to new literature.

Traceability for Climate Metrics is Essential

This discussion paper by Aamaas et al. on climate metrics for emissions of short-lived climate forcers (SLCFs with general atmospheric decay times of < 20 yr) is interesting and holds some lessons for us all, from scientists to the policy arena (for which is it clearly intended). Using results from 4 different chemistry-transport/climate models is an important step and the authors talk about robustness. The work is well written and would provide a useful ACP-appropriate contribution except for two serious flaws (correctable) and some minor fixes.

(#1) Having searched the Supplementary Material, I realized that there is no information given as to the model results, how the four models varied (perhaps even in sign), and even no scientific explanation / justification for the large “aerosol effect” from CH4 and CO emissions, but no stratospheric H2O effect. This is all quite different from the recent IPCC summary and such differences need better explanation at least. I was interested in the shipping results which only come from 2 models and just how different they are. Given the recent problems with representation of ship plumes (e.g., Vinken et al. ACP 2014-1353; Holmes et al. ACP 2014-6801; Eide et al., ACP 2013-4183) I do not see how these results could be promoted without a better comparison with what people are doing with shipping. It seems that all this is in a Bellouin et al (2015) manuscript that has not even been written, much less co-submitted and available for review. This ACPD manuscript simply cannot go forward until the Bellouin 2015 paper is publicly available.

The Bellouin et al. paper is now publicly available in ACPD, which discusses in length the radiative forcing applied in our paper.

The stratospheric water vapour effect is bundled into the methane RF by increasing direct methane RF by 15%, following Myhre et al., doi:10.1029/2006GL027472, 2007.

The reviewer is correct that instantaneous dilution of emissions over a grid box leads to an overestimate of ozone production by NOx. Like most other global aerosol models for climate applications, the ECLIPSE models do not represent shipping emissions differently from emissions from other sectors, and there is indeed no concept of "plume" as a sub-grid feature. We now acknowledge that this may lead to an overestimate of ozone SRF by citing the review paper by Paoli et al., doi:10.5194/gmd-4-643-2011, 2011,

as well as in the Bellouin et al. paper. We have added this sentence in the sixth paragraph in Section 3.1.1:

"The models may overestimate the ozone production of NOx emissions from shipping, as they do not represent ship plumes, but assume instantaneous dilution of emissions in the grid boxes (Paoli et al., 2011)."

(#2) The approach here lacks a traceability that would be necessary for others later to reproduce and at least compare results at a level to understand what drives the model differences. For example, the paper gives a well founded and thorough review of previous work on short-lived RF agents but it misses the early work on short-lived ODPs such as n-propyl bromide (Olsen et al GRL 2000; Bridgeman et al JGR 2000; Wuebbles et al JGR 2001) which explored indexing for these ozone metrics. The work itself is not essential but it led to the mathematical codification (e.g., Prather, GRL 2002) of the relationships between steady-state impacts from sustained emissions (easy to calculate); the pulse-response function; the steady-state lifetime (burden/emission rate). The pulse response of (e.g.) RF is not a simple e-fold decay but is quite complex for short-lived species. Nevertheless the steady-state lifetime and impact (e.g., RF) are shown to be an exact integral of a pulse function. This applies also to integrated CH4 impact (12 yr) from a shipping NOx (1 day) emissions. This paper confuses these simple relationships with the problem that they do not separate the clear components. Do models differ because of different lifetimes or different steady-state RF or because of the mix of time scales (mix of e-folds as in ship NOx example here). If this paper is to be used in the future or compared with new results from the next generation of models, traceability is essential.

We have added a clarification in Section 2.2 that includes these older studies. The model results differ because the steady-state RFs and steady-state lifetimes for the respective species are different.

These sentences are added after Equation 1:

"The calculations of the RFs build on the framework previously shown for short-lived ozone depletion gases for the metric the Ozone Depletion Potential (Olsen et al., 2000; Bridgeman et al., 2000; Wuebbles et al., 2001). This work led to the mathematical relationship between the steady-state impacts from sustained emissions, the pulse response function, and the steady-state lifetime (Prather, 2002), which we follow in our RF calculations."

This sentence is added before Equation 3:

"The perturbation lifetimes are model specific and given in Bellouin et al. (2016)."

This sentence is added after Equation 3:

"The equations for the AGTP calculations for aerosols and ozone pre-cursors are given in Aamaas et al. (2013)."

(#3) This includes some minor fixes and just plain questions. The GWP/GTP figures for CH4 show a short-lived ozone component – is this long-lived ozone? The CH4 aerosol affect is presumably through lowered OH reducing aerosol formation (warming) by pushing more dry-deposition of SO2 – how robust is this? In Figure 8 and some others, it seems odd that the SO2 impact (5 days?) decays away more slowly than CO or NOx (12 yr from indirect CH4 perturbation). Overall, the interaction between the gases and aerosols is not clear – are connections one-way or two-way? The text implies that maybe

some of these components are only from one model? “Not all processes, nor species, have been modeled by all models, and hence, the average for a process can be based on anything from only one model to four models.”

The ozone component from CH4 emissions is a naming issue. We would like to keep the current naming as this is not a secondary effect as “methane-induced ozone” for the ozone precursors. We agree that the ozone is here long-lived since CH4 is long-lived, but the ozone itself is not long-lived.

The reviewer is correct about the CH4 aerosol effect, which is discussed in the Bellouin et al. paper. The models disagree on how strong this CH4 aerosol effect is. As this is thoroughly discussed in the Bellouin et al. paper, we refer to that study (Section 3.5 in Bellouin et al.). We add this paragraph at the end of Section 3.1.1:

“We provide only one global emission metric value for CH4, as CH4 emissions are relatively well-mixed in the atmosphere and expected differences due to regionality and seasonality are small (Bellouin et al., 2016). The aerosol effect is weakly positive, while the models give a wide range from weakly negative to strongly positive, as discussed in Bellouin et al. (2016).”

The reviewer says that the impact from SO2 may seem to decay slower than the impacts from CO and NOx. We have checked the numbers and cannot find this behavior. CO and NOx have a longer response tail from the indirect CH4 perturbation.

Aerosol-chemistry interactions are two-way in the ECLIPSE models. The main link is indeed through OH, although secondary organic aerosols also play a role in VOC perturbations. The aerosol response to ozone perturbations is the contribution where models are most in disagreement, and the strength of that response varies with model, perturbation, region, and season in a complex way. However, the reviewer is correct that some of the processes come from one model. The best estimate is based on one model for BC on snow and BC semi-direct, while all other processes (aerosol effects, short-lived ozone, methane, and methane-induced ozone) are based on three models. The quoted sentence is deleted and replaced with this sentence:

The best estimate is based on only the OsloCTM2 model for BC deposition on snow and BC semi-direct effect, while the best estimate are based on three models for all other processes (aerosol effects, short-lived ozone, methane, and methane-induced ozone).