Interactive comment on "Short lived climate forcers from current shipping and petroleum activities in the Arctic" by K. Ødemark et al.
Anonymous Referee #1
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We thank the reviewers for thorough and very useful comments. In response to the issues brought up, we have somewhat extended the detail level of the study and added more discussion and figures.

Especially the seasonal variations of emissions and forcing responses are now more thoroughly treated, and the discussions of uncertainties and normalized radiative forcings have been substantially revised. We hope that these revisions cover the points raised by the reviewers. Additional responses to the individual points are given inline below (in italics).

General points
This paper presents results from global atmospheric chemistry and radiative transfer modelling, to assess the radiative forcing impacts from emissions of SO2, BC, OC, NOx (and other ozone precursors) in the Arctic. Two sectors, shipping and 'petroleum activities', are investigated. I find the latter term a bit confusing – I guess it refers to oil refineries etc. – this should probably be clarified. The modelling approach seems sensible, but the presentation of results could be clearer. For example, the authors explain that the unusual characteristics of the Arctic (high albedo, continuous light/dark, high angle sun) make it different, i.e. it has starkly different seasons. Yet most of the results are presented as annual averages, thus the (presumably large) seasonal cycles in emissions, atmospheric composition, and RF are (frustratingly) concealed.

Presumably, most of the climate forcing occurs in summer, but there may be some interesting effects in other seasons. Some of the results and discussion is quite brief and grammatically terse – this should be expanded and improved. The importance of the high albedo from the underlying ice and snow is clear, but it is less obvious why a high angle sun or continuous day/night should be important. I can imagine these factors are important, e.g. in the way aerosols scatter incoming radiation, or the photochemical lifetimes of some compounds, but these factors are not explored with model experiments, and so I don't think it is justified to include them, at least not without some further justification. If these points, and those outlined in more detail below, are rectified, then the paper should be acceptable for publication in ACP.

Specific points
p21571
l4 non-methane hydrocarbons
l5 affects -> affect
l22 forcing -> forcings
l25 Better to say identical magnitude emission reductions? Emissions are characterised by their magnitudes and distributions. Clearly, in different locations, the distributions will almost certainly differ, so describing them as identical is incorrect.
Reply: *These points are corrected in the text.*

p21572
l11 on -> of
Reply: *corrected in the text.*

p21574
l15 emission volumes -> emissions. In several places the word 'volume' is associated with emissions, which I think is confusing. Invariably you are talking about masses, or mass fluxes, not strictly volumes.
Reply: *corrected in the text.*

C8616
P21575
I think section 2.3 could benefit from a figure showing emissions maps, perhaps for NOx, BC and SO2? I guess these are shown in Peters et al., but I think they could usefully be repeated here. Also, is there a strong seasonality in emissions? This isn't discussed, but would seem likely (especially from tourism and fishing-related emissions).

Reply: *A plot showing the seasonality in the ship-emissions has been added. The petroleum emissions are assumed to be constant over the year. The figure with Nox concentration change resembles the spatial distribution of the emissions, and a note of this is now in the text, but we have not added emission-plots (other than for the seasonality).*

l18 Figure 1 shows the annual mean NOx change, but I would guess that the NOx change has a large seasonal cycle (much larger in winter, when photochemical processing of the NOx is switched off; although this may also be influenced by any seasonality in emissions). It would seem more useful to this reviewer for Figure 1 to show NOx values in ppt, rather than mg m-2, even if averaged over a height.

Reply: *New figure of Nox is made.*

P21576
As per my previous comment, Figure 2 is an annual mean which I am sure conceals a large seasonal cycle. This is partly resolved in Figure 3, but I think it would be more useful to show winter/summer maps in Figures 1 and 2, or find some other mechanism to show the seasonality. On line 1, both an absolute and a percentage O3 change are quoted. This is useful as it (partly) shows readers how important these local emissions are relative to other influences. It would be good to indicate percentage perturbations to NOx (and other constituents) as well, so that the relative importance (or otherwise) of local emissions is made more clear.

Reply: *New plots added in the text*

L7 not reformed to -> not broken down into

Reply: *corrected in the text.*

L12 There is a missing milli in the units: should be mW, not W.
Reply: *Typesetting error, corrected*

L17 It is normal practice to number figures in the order that they are referenced in the text.
Reply: *Corrected in the text.*

C8617
L23 'reaction species' sounds odd – rephrase this sentence
Reply: *Sentence is rephrased in the text.*

P21577
Is the CH4 RF calculation some sort of steady state extrapolation, since you only have 1 year long runs (hence CH4 will not be in equilibrium)? This is probably explained in the Berntsen et al./Myhre et al. references, but more details should be provided here.

Reply: *The method for calculating the forcing due to CH4 changes follows the approach described by Berntsen et al. (2005) and Myhre et al. (2011). We avoid repeating the details of the method in the text but instead refer to those studies. However, it is now noted in the text that the methane RF, CH4-induced O3 RF and stratospheric H2O RF apply for equilibrium conditions. The CTM do not explicitly calculate the change in CH4 concentrations and in any case the simulations are not long enough for the CH4 to come into equilibrium with the changed OH field. Instead, the OH imposed fractional change in CH4 lifetime calculated by the CTM is used. For the effect from direct methane emissions the fractional contribution to total global emissions are used. The fractions are then multiplied by the present-day concentration of methane and a model-average feedback factor of 1.4 (IPCC 2001), to account for the impact of changes in CH4 concentration on its own lifetime, to yield the fractional changes in CH4 concentration for steady-state conditions. The indirect (through OH) and direct RF is calculated assuming a specific CH4 RF of 0.37 mW m^2/ppbv (IPCC 2001), which assumes a background concentration of 1740 ppb for methane and 319 ppbv for nitrous oxide (IPCC 2001).*

The radiative forcing due to the methane-induced ozone change is estimated using multimodel means (IPCC 2001) of both the response of ozone to a methane change (a 10% increase in methane leads to a 0.64 DU increase in ozone) and an ozone specific radiative forcing of 42 mWm^2 /DU. The stratospheric water vapour RF is set to be 0.15 times that of the CH4 RF. (See Myhre et al. 2011 for reference)

In the above calculations it is assumed that the CH4 concentration in 2004 is in steady state with that year's change in OH. The actual degree of imbalance depends on the history of change in OH, which is not accounted for in the chemical model calculations which used year 2004 emissions. The degree of imbalance will be largest for emission sectors like shipping growing rapidly in recent years. The factor to correct this transient response in year 2004 is taken from Myhre et al. 2011 to be 0.8 for the shipping sector. For the petroleum sector we assume similar change as the total global methane emissions change and therefore set the factor to 1. These factors are then applied to the CH4 RF, the CH4-induced O3 RF and the stratospheric water vapour RF.

The section on methane forcing in the text is now as follows:

Emissions of short-lived components (CO, NOx and NMVOCs) influence the chemical loss of methane. Even if methane is not a short-lived climate forcer a significant proportion of the methane

perturbation could stem from changes of short-lived components. In this section we have therefore included simplified estimates of methane RF. We used the approach described in Berntsen et al. (2005) and Myhre et al. (2011) to calculate the global radiative forcings from methane and associated ozone and stratospheric water vapor changes. The RF values from this method apply from when the perturbations have reached equilibrium conditions. Due to the relatively high NOx /CO emission ratio both the Arctic shipping and petroleum activity leads to increases in OH and thereby decreases in methane lifetime (Fuglestvedt et al., 2008). However, the changes in OH are rather small due to inactive photochemistry in winter. The effect on methane loss is further limited by low temperatures prevailing for most of the year in the Arctic. Due to this it is mainly the ship emissions that have a significant impact on methane lifetime since the traffic and effect on ozone (Figures 3 and 4) peak during summer. We find the resulting indirect methane RF (at steady state, i.e. corresponding to sustained NOx , CO and VOC emissions) from shipping to be -0.59 mWm−2 (global mean). This further results in a methane-induced ozone RF of -0.18 mWm−2 and stratospheric water vapor
RF of -0.09 mWm−2 . For the Arctic petroleum activity the forcings due to changes in methane lifetime are very small amounting to -0.03 mWm−2 for methane, -0.01 mWm−2 for ozone and -0.005 mWm−2 for stratospheric H2 O. The ship emissions of methane (Table 1) are small compared to the total global methane emissions from all anthropogenic and natural sources. We find that the radiative forcing due to direct methane emissions from Arctic shipping is negligible. The methane emissions from the petroleum activity are more than a factor 1000 larger than for Arctic shipping (Table 1). The resulting change in methane concentration at equilibrium leads to radiative forcings of 1.1 mWm−2 for methane, 0.33 mWm−2 for ozone and 0.17 mWm−2 for stratospheric H2 O.

P21578

L8-9 (and also earlier/later) When RF values are quoted, it is rather important to be clear if these refer to the average over the 60-90N region, or to global values. This is not always entirely clear. The reader should also be clearly warned of the important difference in meaning from the outset.

Reply: *clarified in the text.*

L12 Figure 5 shows column changes, not concentration changes.
Reply: *Corrected*

L16-17 Revise sentence (grammar).
Reply: *Corrected*

L20 ->of the high
Reply: *Corrected*

L29 Should this be percent or per mille?
Reply: *Corrected*

P21579
L1-2 Revise sentence (grammar).

Reply: *Corrected*

L6 As earlier, I dislike the use of volume.
Reply: *Corrected*

L20 As above, is the 20.2 mW m-2 a global value or for 60-90N?
Reply: *60-90N. Added in the text.*
P21580
L1 0N -> 90N!
Reply: *Typesetting error, corrected*

L4 anthropogenic
Reply: *Corrected*

L6 the Arctic
Reply: *Corrected*

C8618

L19 It is stated that the weaker values for OC RF over the Arctic are because the reflective aerosols are over bright surfaces, but this argument is not mentioned with respect to SO4 – why not?

Reply: *This argument is also the case for SO4. For OC, this effect is stronger than for SO4, meaning that scattering from OC (in the Arctic) is weaker than for SO4, because of high relative humidity.*

This text has been added:
High surface albedo and lower solar irradiance reduces the NRF for emissions of scattering aerosol in the Arctic compared to global emissions, as seen for OC. For nitrate and sulphate, which are more hygroscopic than OC, the generally high relative humidity in the Arctic strengthens the NRF.

L27 Equation (1): Why is there a 't' on the left-hand side? I suggest you use the nomenclature on p210 of IPCC AR4.
Reply : *Corrected*

P21581
Section 4.1 on uncertainties seems brief and not very comprehensive. Aren't there important uncertainties associated with the indirect effect and BC on snow, for example?
It seems odd just to highlight plume effects on ozone.

Reply: *The section is re-written, and error-bars with uncertainties are added in figure 7.*

The Uncertainties section is re-written to:
The main results from this work is given in Table 2 in terms of RF for the different components. Based on the available information it is not possible to perform a formal uncertainty propagation from emissions through CTM calculations of concentrations and radiative transfer simulations. To estimate the uncertainty in the RF numbers we therefore have to rely on estimates from other

studies that often have a more global focus and make a subjective adjustment to the individual uncertainties for the Arctic region. Peters et al. (2011) do not estimate uncertainties in the emission factors for 2004 emissions. For the emissions we apply the same relative, component specific, uncertainties as were used for the shipping sector in Fuglestvedt et al. (2008). For the RF by sulphate, BC and OC aerosols we use the multimodel 1-σ range of global mean RFs from model simulation with equal emissions from the AEROCOM project (Schulz et al., 2006).
For nitrate aerosols, BC on snow and the indirect effect of aerosols we apply the range given by IPCC (Forster et al. (2007), Table 2.12), while ozone we apply the same relative uncertainties for were used in Fuglestvedt et al. (2008) for the shipping sector. The uncertainties are given in Figure 9. There are some Arctic or sector specific factors that could add to the uncertainties, but is not included in the estimates due to lack of information. This includes potentially higher uncertainties in the activity data and emission factors for shipping and petroleum exploration at high latitudes, impact of large variability in surface albedoes and non-linear plume effects in ship plumes (e.g. Huszar et al. (2010)) .

P21582
L7 leads -> lead
Reply: *Corrected*

L9 exert -> exerts
Reply: *Corrected*

L11 strongest -> stronger
Reply: *Corrected*

L18-19 Why is the high solar angle important? Also why is continuous light/dark important? I can imagine these features of the Arctic are important, but I don't think you have demonstrated they make any difference in the experiments you have presented.

Reply: *We agree with the reviewer that the remark is presently poorly founded, and have added more discussion of this point.*

We have added a reference to Haywood and Shine (1997). They show that "the strongest direct forcing occurs when the solar zenith angle is 70"- 80" despite the decrease in the incident solar radiation as the solar zenith angle increases. This is because the shape of the phase function means that as the solar zenith angle increases, a greater proportion of the radiation that is scattered into the forward hemisphere is included in the upward- scattered irradiance. This effect combines with decreases in the radiation incident upon the aerosol with increasing solar zenith angle to produce a direct forcing that is strongly dependent upon the solar zenith angle, results that are consistent with those of Nemesure et al. (1995) and Pilinis et al. (1995). "

The effect of continuous light/dark is perhaps clearer with the figures we've added.

L21 show -> shows
Reply: *Corrected*

P21583

You refer to potential changes in human activity in the Arctic – but these would also

affect (for example) your RF calculations, as the underlying albedo would change. This sort of effect should also be mentioned.

Reply: *Yes, we agree. Peters et al have, in addition to the emissions from shipping and petroleum in the Arctic for current emissions used here, made a dataset for future emissions. Here, the changed shipping patterns and different petroleum extraction cites due to decreasing ice-extent is taken into account. The effect from these emissions will be presented in a separate paper.*

P21589
Table 1: not volumes. You should clarify units – are NMVOC in kt-C or kt-NMVOC, is SO2 in kt-SO2, is NOx in kt-N?, etc.
Reply: *Updated*

C8619
P21590
Table 2: I would refer to these as column amounts, rather than burdens.
Reply: *Corrected*

P21592
Figure 1: Why not show in ppt, a more commonly used unit?
Reply: *New plot added*