Response to reviewer #1:

General Comments:
This study investigates the impacts of shipping emissions in northern Norway on local air quality and short-wave radiative effects. The study combines measurements and modeling tools in several ways: Airborne measurements from the 2012 ACCESS campaign are combined with FLEXPART-WRF to generate ship-specific emissions of NO_x and SO_2, which are then used to evaluate the STEAM2 shipping emissions inventory for the same ships. Next STEAM2 is used in WRF-Chem to quantify the impact of shipping on local levels of atmospheric pollutants and radiation, and model results are compared with ACCESS measurements. The topic of the paper is suitable for ACP and of importance in light of the expected increase in shipping activity in the Arctic in the coming years. The paper is quite comprehensive, but well structured and written. A few clarifications (see specific comments) are needed, and once these are addressed the paper should be accepted.

COMMENT: 1) P 18411, L2: suggest noting that the combination of reduced SO2 and the expected continued increase in CO2 is important.
COMMENT: 2) P 18411, L 3: Is this really expected? For instance, the future projections of shipping emission in the Second IMO GHG study do not show sustained reductions in shipping SO2 and NOx emissions towards 2050. Suggest rephrasing.
RESPONSE TO COMMENTS 1 and 2: In order to clarify this we have updated the text of the paper from:
"Although ship emissions have competing warming and cooling impacts, the climate effect of ships is currently dominated by the cooling influence of aerosols, especially sulfate formed from SO_2 emissions (Eyring et al., 2010). In the future, declining global SO_2 emissions due to IMO regulations are expected to change the global climate effect of ships from cooling to warming (Fuglestvedt, 2009; Dalsøren et al., 2013)."
To:
"The current radiative forcing of shipping emissions is negative and is dominated by the cooling influence of sulfate aerosols formed from SO_2 emissions (Eyring et al., 2010). However, due to the long lifetime of CO_2 compared to sulfate, shipping emissions warm the climate in the long-term (after 350 years, Fuglestvedt et al., 2009). In the future, shipping emissions of SO_2 are expected to decrease due to IMO regulations, while CO_2 emissions will continue to grow due to increased traffic. This combination is expected to cause warming relative to the present day (Fuglestvedt et al., 2009; Dalsøren et al., 2013)."

COMMENT: 3) P 18411, L 20: This is a bit imprecise and should be rephrased. The results is not purely an effect of reduced SO2 emissions, but also of the fact that SO2(SO4) gives a stronger cooling effect for the southern routes.

RESPONSE: We replaced:
"In addition, it has recently been shown that routing international maritime traffic through the Arctic, as opposed to traditional routes through the Suez and Panama canals, will result in warming in the coming century and cooling on the long term, due primarily to the competing effects of reduced SO_2 due to IMO regulations and reduced CO_2 emissions associated with fuel savings (Fuglestvedt et al., 2014)."
By:
"In addition, it has recently been shown that routing international maritime traffic through the Arctic, as opposed to traditional routes through the Suez and Panama canals, will result in

warming in the coming century and cooling on the long term. This is due to the opposite sign of impacts due to reduced SO_2 linked to IMO regulations and reduced CO_2 and O_3 associated with fuel savings (Fuglestvedt et al., 2014). In addition, sulfate is predicted to cause a weaker cooling effect for the northern routes (Fuglestvedt et al., 2014)."

COMMENT: 4) P 18416, L 6: how important is the quality of STEAM2 CO2 emissions for this method and has the quality previously been evaluated?
RESPONSE: The quality of the STEAM2 CO_2 emissions is directly related to the quality of the prediction of fuel consumption by STEAM2. Jalkanen et al. (2012) compared the fuel consumption predicted by STEAM2 for 5 ships in the Baltic Sea with the reported fuel consumption for each ship. This comparison indicated that the model bias on fuel consumption was low (less than 21 %). CO_2 emissions predicted by STEAM2 are only used in our study to calculate plume injection heights. We investigated the sensitivity of the plume rise model to the largest CO_2 bias presented in Jalkanen et al. (2012) (decreasing the CO_2 in the plume rise model by 21%) and found that this leads to a modest variation in plume injection heights. For example, the plume injection height changes by 12 % for the Costa Deliziosa. Given this small impact, a full discussion of the impacts of CO_2 emissions uncertainties on plume injection height has not been included in the paper. We also note here that when calculating the emissions in Sect. 2, the peaks that were the most sensitive to the injection heights were excluded from our analysis. We have included more information about the STEAM2 emissions in the response to comment 12 by reviewer 2.

COMMENT: 5) P 18417, L 20: VOC speciation both for shipping and anthropogenic emissions?
RESPONSE: In order to address this comment, we have updated the text of the paper:
"Bulk VOCs are speciated for both shipping and anthropogenic emissions, based on Murrels et al. (2010). Ship emissions are speciated using the 'other transport' sector (transport emissions, excluding road transport) and anthropogenic emissions are speciated using the average speciation for all remaining sectors."

COMMENT: 6) P 18420, L 13: is this consistent with lines 4-5 above which says that ship emissions are injected using the plume model for CTRL and CNTR3?
RESPONSE: The emissions of the 4 targeted ships overlap with several other ships in the 15 km × 15 km grid used in the CTRL simulation. Because of this, the plumerise calculations for these 4 ships in the CTRL simulation are using the default plume rise characteristics (stack height, CO_2 emissions) from the Wilson Leer. The same characteristics are used for all the other ships from STEAM2 in the CTRL run. However, the CTRL3 simulation is using more detailed information for the Costa Deliziosa, Alaed and Wilson Nanjing ships. In summary, ship emissions are injected using the plume rise model for both CTRL and CTRL3, but the plume rise model does not use the same input parameters for both simulations. To clarify this, we have updated:
"In the CTRL simulation, there are usually several ships in the same 15 km × 15 km grid cell, and the 4 targeted ships were treated in the same way together with all unidentified ships, using the exhaust parameters of the Wilson Leer and local meteorological conditions to estimate injection heights."
To (new text in bold):
"In the CTRL simulation, there are usually several ships in the same 15 km × 15 km grid cell, and the 4 targeted ships were treated in the same way together with all unidentified ships, using the exhaust parameters of the Wilson Leer and local meteorological conditions to

estimate injection heights. **This means that, for the Costa Deliziosa, Alaed and Wilson Nanjing, the plume rise model is used in CTRL with exhaust parameters from a smaller ship (the Wilson Leer) than in CTRL3. Because of this, emission injection heights for these ships are lower in CTRL than in CTRL3.**"

We have also added in section 5.1 a sentence mentioning this difference when comparing the CTRL and CTRL3 simulation. We have updated:
"In contrast, the CTRL run has wider NO_x peaks and lower peak heights, because of dilution in larger grids."
To (new text in bold):
"In contrast, the CTRL run has wider NO_x peaks and lower peak heights, because of dilution in larger grids. **Another difference between the simulations is the treatment of plume rise (Sect. 3.3), such that the Costa Deliziosa plume is located at lower altitudes in CTRL than in CTRL3.**"

COMMENT: 7) P 18421, L 14: looking at Fig. 3 the agreement seems to be a bit poorer for Costa Deliziosa (panels C and D), which might be worth noting if really the case.
RESPONSE: This point was also raised by reviewer #2. We now mention that the agreement is less good for the Costa Deliziosa during the second flight leg at 165 m. However, we also note that the method used to derive the emission estimates is not sensitive to the plume location but to the error in the angle formed between the plume and the flight track, which is relatively small even for this case. Furthermore, emission estimates for the Costa Deliziosa are based on plume interceptions during both the first and second leg, which would reduce the impact of this source of error. In order to clarify this, we added a discussion of peak displacements in the paper (new text in bold):
"During the second altitude level on 11 July (Fig. 3c and d) the Wilson Leer was farther south and the Costa Deliziosa had moved further north. Therefore, the plumes are farther apart than during the first pass at 49 m. **Modeled and measured plume locations agree well for the first run (z = 49 m). For the second run (z = 165 m), the modeled plume for the Costa Deliziosa is, on average, located 4.7 km to the west of the measured plume. This displacement is small considering that, at the end of this flight leg, the plume was being sampled ~80 km away from its source. This displacement is caused by differences between the simulation (MET) used to drive the plume dispersion model and the observed meteorological conditions (-16° for wind direction, +14 % for wind speed)."**

COMMENT: 8) P 18422, Eq. 1: how is the SO2 background derived? If derived from model output, has the general model performance been evaluated?
RESPONSE: In order to address this comment, we have added to the text:
"The background mixing ratios were determined by applying a 30 second running average to the SO_2 and NO_x measurements. Background values were then determined manually from the filtered time series. For each NO_x peak, an individual background value was identified and used to determine the NO_x enhancement in each plume. For SO_2, a single background value was used for each flight leg (constant altitude)."

COMMENT: 9) P 18426, L 12: is there a reference that could be used for the current growth?
RESPONSE: Previous shipping emission inventories (e.g. RCP8.5, Riahi et al., 2011; Corbett et al., 2010) make the assumption of traffic growth during the current period, and, as we

showed in Table 5, shipping emission inventories based on activity data from recent years (e.g., for 2012, Winther et al., 2014 and STEAM2) contain higher emissions than earlier inventories for previous years (e.g., for 2000 and 2004, Dalsøren et al., 2007; Dalsøren et al., 2009; Corbett et al., 2010). However, to our knowledge, there is no reference specifically showing that ship emissions in northern Norway have grown recently. It is possible to estimate this growth using ship activity data such as the AIS database, but this data is not freely available.

COMMENT: 10) Section 5.1 suggests that because the CTRL simulation performs well on average, the STEAM2 is able to represent the average emissions from ships. Given the significant differences for individual ships shown in Section 4, how confident are the authors that this is indeed valid in a more general context?
RESPONSE: This point was also raised by reviewer 2, and was answered in detail in the response to comment 12 by reviewer 2. Beecken et al. (2015) showed that STEAM2 performs well on average in the Baltic Sea region, even if larger biases are possible for individual ships. Vertical profiles (Figure 8) above the sea surface can also be expected to be more representative of the regional background pollution from ships than the comparison with specific ship plumes. We mentioned that model biases over the average SO_2 profiles are significantly better than the ones from a previous model intercomparison by Eyring et al. (2007) in similar conditions. Since shipping emissions from STEAM2 are the main source of SO_2 (Figure 2), this result gives another indication of the overall quality of STEAM2 emissions. However, because there is very limited information available for individual ships operating in the Arctic region, we agree that more measurements and model case studies are needed in order to make draw more general conclusions.

COMMENT: 11) Section 5.1, first paragraph: is there evidence that a 3km x 3km resolution is sufficiently small to capture subgrid plume processes? And is the difference between the two resolutions used here sufficient to actually capture relevant non-linearities?
RESPONSE: This is an important point, 3 km is not sufficiently small to capture all sub-grid plume processes. Certainly, ships emissions are not instantly diluted into 3 km × 3 km grid boxes. However, by comparing with measurements we evaluate if 3 km × 3 km is sufficient to reproduce some of the plume macroscopic properties. Given the comparison with measurements, it is clear that individual plumes are resolved. We also find that this resolution is sufficient to represent some ozone titration in ship plume (e.g. Figure 5).

COMMENT: 12) A more general comment is that it is not entirely clear from the start what the scale of "local" versus "regional" is. For instance, in the Section 5.1 "local" essentially means ship plumes. I think that could be better reflected in the title of this section.
RESPONSE: We agree, and have defined the local scale as the plume scale in the abstract and in Sect. 1

COMMENT: 13) The title of section 5.1 should also better reflect the focus on model evaluation in order to separate it more clearly from section 5.2.
RESPONSE: We agree and have updated the section 5.1 title to "Model evaluation at the plume scale and the regional scale"

COMMENT: 14) P 18427, L 22: has Falcon 20 been mentioned before? Suggest mentioning in Section 2.

RESPONSE: “Falcon 20” is the aircraft used to perform the measurements. This has been replaced by “the aircraft”, and this aircraft has been identified as the DLR Falcon 20 in Section 2.

COMMENT: 15) P18428, L13: could you provide the absolute value (ppbv) for comparison with the results previously found for global models described below?
RESPONSE: This value (+0.08 ppbv) has been added in the text.

COMMENT: 16) Fig. 6: Could corresponding results for SO2 be presented?
RESPONSE: Yes, SO_2 results are now included in Fig. 6 in the main text of the paper (also below). The corresponding results for 12 July 2012 are shown in the electronics supplement (Fig. S3). The CTRL3 run is not able to reproduce the large SO_2 peak observed at 17:20, since we already note in the paper (p 18423, lines 12-15), that “this large increase in SO_2 in an older, diluted part of the ship plume suggests contamination from another source [than the Costa Deliziosa]”. A corresponding short discussion of these results has been added to the main text of the paper.

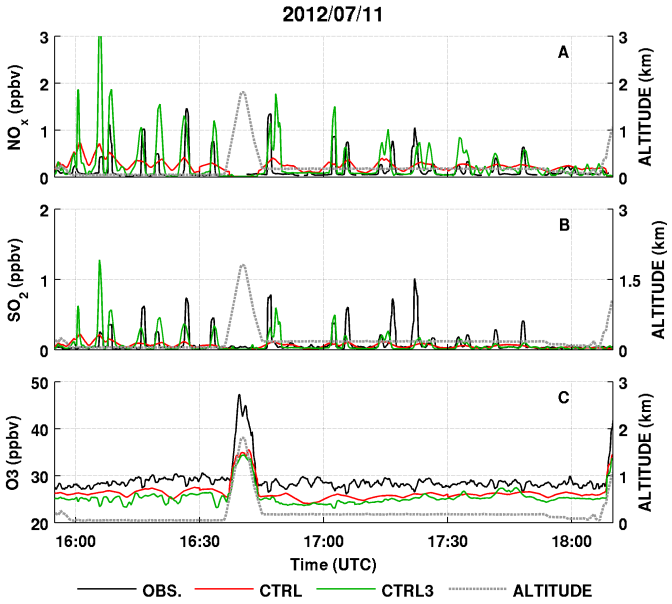

Figure 1. (Figure 6 in the paper) Time series of measured NO_x, SO_2 and O_3 on 11 July 2012 compared to model results extracted along the flight track for the CTRL and CTRL3 runs. Observations are in black, the CTRL run is in red, and the CTRL3 run is in green. A 56 s averaging window is applied to the measured data for model comparison (approximately the time for the aircraft to travel 2 × 3 km). Flight altitude is given as dashed black line.

COMMENT: 17) P 18429, L 3: could this overestimation also be caused modeling issues, e.g., chemistry?
RESPONSE: The lifetime of NO_x (by loss with OH) in the Costa Deliziosa plume is sufficiently long (12 h, estimated using modeled OH concentrations) that this over estimation should not be due to chemistry, but rather due to emissions. If the overestimation were caused by an overestimated NO_x lifetime, the bias on NO_x would also be larger at the end of the constant altitude runs, when older parts of the plume were sampled. A short discussion of this has ben added to the paper for clarity.

COMMENT: 18) P 18432: it would be interesting to compare the ozone changes on a

per unit emission basis to examine the sensitivity. Is this possible with the available data?
RESPONSE: We agree that it would be interesting, however because other studies have not reported these values for ozone and due to the non-linear nature of ozone chemistry, we have not included/reported these values.

COMMENT: 19) P 18433, L5: what is the average lifetime of BC in WRF-Chem?
RESPONSE: The BC lifetime is not prescribed in the MOSAIC aerosol scheme, which we use to describe aerosols in WRF-Chem. In each model grid cell, aerosols are represented as 16 size bins (8 interstitial and 8 activated in clouds), representing internally mixed particles with varying compositions, mass concentrations and number concentrations. Particle size distributions and compositions (which includes BC) evolve by coagulation, condensation and nucleation of condensable gases, and cloud processing. This ageing changes the sizes and hygroscopicity of the particles, which influences their activation in clouds and water uptake. Dry and wet removal of particles containing BC depend on aerosol composition, aerosol size, and the amount of aerosols activated in clouds. Currently, the model cannot calculate the resulting BC lifetime. Samset et al. (2014) estimated the BC lifetime (more accurately referred to as a residence time) in global models by calculating the ratio of the global annual mean BC burden (kg m^{-2}) divided by the annual mean BC emissions (kg m^{-2} s^{-1}). This calculation cannot be performed in a regional model such as WRF-Chem for two reasons. First, part of the total BC burden in the domain is originating from the boundary conditions and not from the in-domain emissions. Second, part of the BC burden from in-domain emissions can exit the domain through its boundary. However, we can use the method of Samset et al. (2014) to calculate the residence time of BC emitted by ships, since Figure 9H indicates that most of the BC originating from shipping emissions remains within the domain. Using this method, we find a residence time for shipping BC of 1.4 days. This value is lower than the global average BC residence times of 3 to 7 days estimated by Samset et al. (2014), most likely because the global value represents the residence time of BC emitted in more dry regions and seasons. This is now discussed in the paper and the BC residence time for ships (1.4 days) is included in the abstract.

COMMENT: 20) Section 5.2.2: It would be good if this section could be expanded to include some further comments about how the modeled overestimation of $PM_{2.5}$ (P 18430) influences the estimate of radiative effect, the role of black carbon (incl. BC in snow) and placing the results of this study in the context of the large uncertainty ranges previously found for the indirect aerosol effect of shipping.
RESPONSE: The radiative effect of shipping emissions is uncertain, in part because of uncertainties in the emissions and in calculating the indirect effect of aerosols. Eyring et al. (2010) estimated that the global radiative effect of shipping emissions was -0.408 W m^{-2}, but found an uncertainty range of +/-0.425 W m^{-2}, larger than their estimated impact. Uncertainties have been estimated in previous studies by using multi-model ensembles (Eyring et al., 2007), by using several emission inventories with the same models (Lauer et al., 2007). A more comprehensive evaluation of uncertainties has been performed by Fuglestvedt et al., (2007) who estimated the uncertainties due to fuel use, emission factors, modeling atmospheric dispersal and removal, and radiative forcing calculations. In our case, we have chosen to perform simulations at high resolution and using a relatively complex aerosol scheme (MOSAIC), therefore other runs (using different emissions and parameters) were not possible. Ødemark et al. (2012) estimated uncertainty in radiative effects by applying to their results uncertainty ranges estimated from previous studies that were not necessarily specific to the Arctic or to shipping, which were subjectively adjusted for Arctic

shipping. Using this approach, Ødemark et al. (2012) found a range of -3.9 mW m^{-2} to -1.3 mW m^{-2} for the global and annual indirect forcing of Arctic shipping emissions. In our study, we show that PM_1 in the plume of the Costa Deliziosa are overestimated by 26 % on average. However, it is not clear if this number is representative of the whole ACCESS campaign and what it means in terms of radiative effect, as the indirect effect does not depend linearly on PM. In order to address these comments, we have updated the text to (new text in bold):

"This means that the stronger **radiative effect** found here is not due to increased sulfate concentrations from larger emissions, but to the way aerosol/cloud interactions are treated in both models: the indirect aerosol effect was calculated by Ødemark et al. (2012) based on parameterizations of the relationship between clouds droplet numbers and aerosol mass, whereas the MOSAIC aerosol module used in this study explicitly treats aerosol activation within clouds and the impacts on cloud properties (Yang et al., 2011). **It's important to note here that that the indirect radiative effect of shipping emissions is uncertain and that the difference between the estimate of Ødemark et al. (2012) and ours can easily be explained by these uncertainties. Based on the work of Eyring et al. (2007), Lauer et al., (2007) and Fuglestvedt et al., (2007), Eyring et al. (2010) estimated that the global radiative forcing of global shipping emissions was -0.408 W m^{-2}, but found an uncertainty range of +/-0.425 W m^{-2}. Ødemark et al. (2012) considered that the uncertainty on the indirect effect in their simulations was the same than the uncertainty for the global indirect forcing of aerosols as estimated by the IPCC (Forster et al., 2007, Table 2.12). Using this method, Ødemark et al. (2012) found a range of [-3.9 mW m^{-2},-1.3 mW m^{-2}] for the global and annual indirect effect of Arctic shipping emissions. It is important to better understand and constrain this effect, which would require more aerosol measurements in shipping lanes (including number concentrations and aerosol compositions in ship plumes) and more model case studies."**

The radiative effect of BC on snow was not computed in the present study since this effect is not currently included in WRF-Chem. However, Ødemark et al. (2012) estimated that for Arctic shipping the radiative effect of BC (BC on snow + direct BC effect) was small compared to the indirect and direct effects of sulfate. In order to address these comments, we have updated the text to (new text in bold):

"Yang et al. (2011) and Saide et al. (2012) showed that including cloud aerosol couplings in WRF-Chem improved significantly the representation of simulated clouds, indicating that the indirect effect was relatively well simulated using CBM-Z/MOSAIC chemistry within WRF-Chem. **Our calculations do not include the effect of BC on snow, since this effect is not currently included in WRF-Chem**"

COMMENT: 21) P 18436, L 21: did these studies include calculations of the CO_2 impact?

RESPONSE: These studies only calculated the impact of O_3. We corrected this in the text.

Technical Corrections:

COMMENT: 1) P 18420, L4-5: clumsy, consider rephrasing

RESPONSE: This has been rephrased for clarity.

COMMENT: 2) P 18420, L 25: "First", is there a "second" in this paragraph

RESPONSE: The "First" has been removed.

COMMENT: 3) P 18428, L 3: NO should be NOx?

RESPONSE: This has been corrected.

COMMENT: 4) P 18433, L23: "Averaged over the surface of the Earth"; strange wording, consider rephrasing
RESPONSE: This point was also brought up by reviewer 2 (technical corrections, comment 7). We have include a detailed response to this issue in our response to Reviewer 2 and have revised the wording in the text.

COMMENT: 5) P 18436, L 23: missing period between "challenging" and "Our"?
RESPONSE: This has been updated.

COMMENT: 6) Figure 3: is y-axis label on top right panel correct? Please check.
RESPONSE: The top right panel was incorrectly using a different label than other panels showing model results. This has been corrected.

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